MARKET DISCIPLINE, INTEGRITY, INFORMATION DISCLOSURE AND FINANCIAL MISCONDUCT

By

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ABSTRACT

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In my dissertation, I explore the factors which impact the outcome of financial misconduct. Financial misconduct imposes negative externalities on firm value, influences investment decisions, and results in both wealth transfer and destruction. The unique governance structure in the asset management industry amplifies the role of investors on firm behavior. I provide novel evidence on the variation in response towards enforcement actions by investor types; I find evidence consistent with two non-mutually exclusive explanations for this heterogeneity. First, investor sophistication affects the effective cost of information acquisition and processing, making the fund flow discipline less prevalent for retail investors. Second, investors are less likely to punish funds when the costs of moving capital become substantial. Besides investors, other market participants also significantly affect firm behavior. Internal factors, such as corporate culture, along with external factors, such as product market competition, have significant impact on corporate fraud. For example, a lack of focus on integrity in corporate culture is associated with unethical corporate behavior, cultures that neglect integrity are associated with a greater probability of SEC enforcement actions for accounting misstatements. In addition, firms with lower product market differentiation exhibit significantly lower rates of fraud; the relationship is more pronounced for complex firms and is robust to controlling for various measures of competition, predictors of fraud, and industry heterogeneity. Overall, the findings suggest that lower differentiation disciplines firms by facilitating fraud detection through a benchmarking channel.

This thesis is dedicated to my husband Alexander Ferko, my parents Zhao Hanxiang and Li Gang.

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CHAPTER 1

INVESTOR REACTION AND MUTUAL FUND MISCONDUCT

1.1 Introduction

Detection of financial fraud depends on both internal and external corporate governance control mechanisms (Dyck et al., 2010; Fang et al., 2016). Internal corporate governance is negatively associated with financial fraud (Dechow et al., 1996; Farber, 2005), whereas external governance depends on regulators and competitors facilitating information disclosures and the market's responses revealed the allocation decisions of investors (Giannetti and Wang, 2016; Gurun et al., 2018). The extent to which regulatory enforcement and market enforcement complement each other in protecting stakeholder interests depends on the behavior of market participants (DeMarzo et al., 1998; Garoupa, 2002). Yet, little is known about how investor heterogeneity affects this relation. In this paper, I study how shareholder composition affects market discipline by investigating variation in investors' responses to regulatory disclosures of mutual fund misconduct.

Why is the mutual fund a good laboratory to examine the role of investor heterogeneity in external governance? First, nearly half of U.S. households own mutual funds, whose assets under management exceed $10 trillion by 2016. Such high reliance on investment companies and investors' limited attention (Hirshleifer and Teoh, 2003; Sialm et al., 2015) expose investors to a greater risk of mutual fund fraud.[1] Second and more importantly, the unique governance structure of mutual funds disciplines management through redeemable residual claims (Fama and Jensen, 1983)[2], as mutual fund boards have limited oversight over the management and advisers[3]. In addition, unlike banking regulators, the Securities and Exchange Commission (SEC) facilitates the

[1] See ICI Research Perspective and ICI Fact book (2017).

[2] Fama and Jensen (1983) posit that redeemable residual claims should serve as a strong disciplining mechanism to resolve fiduciary conflicts of interest between shareholders and management, which is very different from equity shares are not redeemable, but rather transferred on secondary market.

[3] The 1940 Act and its rules set forth specific duties of mutual fund directors because mutual fund has no employees, the operations of mutual funds relies on the adviser and other service providers. (See Tufano and Sevick, 1997; Del Guercio et al., 2003; Khorana et al., 2007; Ding and Wermers, 2012)

transparency of mutual fund operations, but has no authority to intervene if an investment adviser is taking excessive risks (Jickling and Murphy, 2010). With weak governance structures in place and very limited scope for regulatory enforcement, market discipline plays a vital role in mutual fund governance.

I present novel evidence about the heterogeneity in responses to disciplinary actions for different investor types using regulatory disclosures against investment advisers from *Form ADV*. First, I show that investment adviser misconduct results in strong negative flows from institutional investors, but not from other investors (e.g, direct-investing retail clients). Second, I find supportive evidence for two non-mutually exclusive explanations of such asymmetric reactions. One explanation is that investor sophistication and information noise affect information transmission. Another explanation is that due to restricted investor mobility, elevated asset redemption costs reduce investors' incentive to reallocate assets. These findings contribute to a better understanding of how shareholder characteristics affect the extent to which market disciplines management.

Form ADV is a mandatory disclosure form used by investment advisers to register with both the SEC and state securities authorities, and must include any disciplinary events involving the firm and/or its employees.[4] For example, ADV disclosures reveal that Virtus Investment Advisers used false and misleading advertisements to grow its assets under management from \$191 millions in 2009 to \$11.5 billion by 2013; Barclays advisory programs overbilled their clients by nearly \$50 million; one of the portfolio managers from Morgan Stanley Investment Management unlawfully conducted prearranged trading that favored certain advisory client accounts over other. These instances of financial fraud not only cost investors millions of dollars but also destroy investors' trust in the financial system. Following disclosures of such regulatory disciplinary events, investors exert market discipline on financial institutions through asset redemption; however, the extent to which they do so depends on the prevalence of information friction and restrictions on investor mobility (Bliss and Flannery, 2002).

[4]Form ADV is used in recent studies regarding investment adviser fraud. Form ADV discloses information about investment advisers' affiliation, disciplinary events, and other material facts. The information is used to assess fraudulent risks of investment advisers (Dimmock and Gerken, 2012).

Consistent with previous studies, such mutual fund misconduct is significantly curtailed by asset redemptions upon the revelation of disclosed events. I find that in the month after regulatory disclosures, funds on average experience negative net flows of almost 1%. Such negative flows amounts to a reduction in asset under management of over $24.5 million, on average, assets under management. During the six months following the regulatory disclosures, the cumulative fund flows total to negative 3.6%. More importantly, I find significant variation in fund flow reactions across investor types. The magnitude of negative net flows is twice as large among institutional share classes compared to retail share classes. The lack of fund flow response among retail investors is primarily found in direct-sold funds. This asymmetric market discipline is alarming considering the increased reliance of individual investor on mutual funds.

Next, I explore two non-mutually exclusive explanations for the heterogeneous fund flow response by investor types. First, I find that elevated information costs significantly reduce the prevalence of the investors' response. Even though SEC enforcement information is public information, the effective information costs depend on investors sophistication and information quality. Moreover, the flows from broker-sold funds are sensitive to regulatory disclosures, suggesting that the costly service provided by full-service brokers (compared to discount brokers who exclusively carry out trade orders) can reduce such information frictions significantly. Second, capital reallocation can be costly for investors, search costs for investment options and tax liabilities can impair investors' ability to reallocate assets, therefore affect their responses to mutual fund fraud.

Investor sophistication has direct impact on their responses and market discipline through the mechanism of elevated effective information costs. There exists information frictions that could interfere information communication for less sophisticated investors and contribute to the variation in the efficacy of market discipline. For example, negative fund flows are more pronounced among highly visible funds, such as funds from *larger institutions* and *star fund families*.[5] Media coverage can significantly reduce the search costs of such information. These results suggest that retail

[5]Following Nanda et al. (2004), star funds are the top performing funds ranked by risk-adjusted return for the past 12 months. "Star" family is an indicator variable equal to one if a fund family has at least one star fund under management.

investors may rely on different information sources (see Sirri and Tufano, 1998; Chevalier and Ellison, 1997), and the heterogeneity in investor sophistication can drive market segmentation (Guercio et al., 2010).

Indeed, the differences in fund flow responses between institutional and retail investors disappear when the enforcement events are against repeat offenders. The magnitude of the negative flow reaction from retail investors more than doubles for repeated offenses (-0.6%) compared to first-time offenses (-0.2%). The negative flow response is also stronger when regulatory actions are initiated by the SEC, compared to other regulators, including state regulators, other federal regulators, foreign regulators, and self-regulating organizations such as Financial Industry Regulatory Authority (FINRA). Fund flow responses from retail investors also increase as the fraud charges gets more severe,[6] and increase with the media coverage of the mutual fund misconduct[7]. These results further convey that retail investors have an inferior ability to access information and therefore face a higher level of effective costs to public information.

There has been a long-standing debate over whether mandatory disclosure requirements improve transparency and financial efficiency (i.e. Hermalin and Weisbach, 2012; Agarwal et al., 2014; Edmans et al., 2016). However, the relation between investor response and information quality is overlooked, especially when considered in combination with investor sophistication. I use irrelevant disciplinary events to study how information quality affects investor response. I find that both enforcement events against *unrelated mutual fund products* (i.e., derivatives and insurance) and against funds associated with *financial institutions* (no indication of wrong-doings by funds themselves) result in negative fund flows by retail share classes. In addition, investment advisers are also forced to reduce management fees by 5% after disciplinary events, with the exception of direct-sold retail funds. These results raise further questions about the scope of market discipline in the presence of investor segmentation.

The second explanation for the variation in intensity of market discipline depends on investor's

[6]The severity of fraud is measured by the monetary fine as a result of regulatory enforcement.

[7]Media coverage is an indicator variable that takes value one if there is mentioning of financial fraud in the major U.S. business news outlet, such as The Wall Street Journals, The Dow Jones Newswire, Yahoo Finance, and The New York Times etc.

incentive to monitor, measuring investors' monitoring effort remains chalenging. Evans and Fahlenbrach (2012) argue that retail investors can benefit from the ability and willingness of institutional investors to exercise market governance; retail investors benefit from monitoring performed by institutional "twin" funds[8] and can mimic the asset allocation decisions of their twin funds. I find that the retail funds show much stronger negative fund flow responses when they have an institutional twin. The results suggest that investor's incentive to monitor has significant impact on the effectiveness of market discipline.

Another factor that affect monitoring incentives is investor mobility.[9] For instance, tax liability affect capital allocation decisions (Sialm and Starks, 2012); capital gains overhang increases investors' tax liabilities and costs associated with asset redemptions (Barclay et al., 1998; Ivkovic et al., 2005). Indeed, I only observe increased outflows among funds with low capital gains overhang. My results suggest that the lack of investor response is an outcome of the trade-off between the costs of asset redemptions and the benefits of switching funds.

In addition to capital gains overhang, outside options and other transaction costs also affect investor mobility. Fund flow responses are particularly strong during the *late-trading scandal* period (2003 to 2005), and became much weaker during the financial crisis period (2007 - 2009). The late-trading scandal is associated with extensive scrutiny by media, which reduces the search costs for alternative investment options. The trade-off between transactions costs (direct and indirect) and monitoring benefits can result in investor inattention and inaction.

To my knowledge, this is the first paper about how investor segmentation affects the efficacy of enforcement in case of mutual fund fraud by showing heterogeneity in market responses, based on investor types and regime of enforcement. Previous studies show that mandatory disclosures have significant power in predicting investment fraud (Dimmock and Gerken, 2012), and that disclosed financial misconduct affects mutual fund flows (Houge and Wellman, 2005; Brown et al., 2008; Qian and Tanyeri, 2017). In more recent work, Wu (2017) shows that mutual fund companies tend

[8]The "twin" funds are the funds with the same manager and similar performance but sold to different investors of a differing ability to select and monitor managers.

[9]Hubbard et al. (2010) argue that investor mobility significantly affects investors' asset allocation decisions.

to reduce contractual incentives, raise marketing expenditures, and relax investment restrictions after a revelation of misconduct.

Firms cooking the books are disciplined heavily by investors in the market place; for each dollar of value inflation, firms lose up to four dollars (Karpoff et al., 2008; Dyck et al., 2013; Johnson et al., 2014). Mixed evidence exists on whether capital market participants effectively discipline banks' risk taking behavior through deposit withdrawals and requests for higher interest rates (Billett et al., 1998; Martinez Peria and Schmukler, 2001; Bliss and Flannery, 2002). Asset management companies differ from both typical corporations and banks. As opposed to providing products that require active decisions, the service-for-fee model naturally entails inertia on the consumer's part. In addition, unlike highly-regulated banks, the main role of the regulator in the asset management industry is to maintain fair and orderly markets with no authority to limit risks taken by investment advisers. Thus, the mutual fund industry relies to a large extent on market discipline in governing fund managers, with the regulator's responsibility limited to information disclosure (Jickling and Murphy, 2010). This paper sheds light on how information frictions affect the outcomes of market discipline, in the presence of less-informed investors.

This paper also relates to the literature on information quality, investor mobility and asset allocation decisions. Previous research documents that retail investors use less sophisticated measures to evaluate funds, and their behavior incentivizes fund managers to alter risks (Chevalier and Ellison, 1997; Sirri and Tufano, 1998); in contrast, sophisticated institutional investors show more rational behavior (Del Guercio and Tkac, 2002). In addition, Evans and Fahlenbrach (2012) find greater monitoring and lower transaction costs when an institutional twin fund exists. This reduces agency problems and enhances the retail fund's performance. My paper complements this strand of the literature by showing that lack of investor sophistication together with information frictions can inflate the effective information costs, and significantly impair investors' ability to discipline mutual fund management. This casts doubt on whether market-based governance alone can safeguard investors.

The rest of the paper is organized as follows. Section 2 describes the data sources used in the paper and presents summary statistics. Section 3 introduces the empirical design and empirical results. How information costs influence the effectiveness of market discipline is examined empirically in section 4. Section 5 investigates how monitoring incentives and investor mobility affects market discipline. Section 6 briefly discusses the implications of the paper and concludes.

1.2 Data and Summary Statistics

1.2.1 Data

Four main data sources are used in this study. First, fund flows are from two separate data sources. Fund flows at the share-class level are directly obtained from the Survivor-ship bias-free mutual fund database provided by the Center for Research in Security Prices (CRSP) from year 2000 to 2016. As in other mutual fund studies (see Chevalier and Ellison, 1997; Barber et al., 2016), I exclude funds which have data of less than 24 months, and total assets under management below \$10 million at year end. Closed-end funds and variable annuity funds are also excluded. In the final sample, only equity funds, which contain 5438 fund share classes and 1,654 investment advisers, are included.

The second data source used to obtain fund flows are N-SAR filings from years 2000 to 2016. N-SAR is a mandatory semi-annual filing for investment companies. The main advantage of N-SAR data is that monthly gross inflows and outflows are reported separately. N-SAR filings also report other fund characteristics such as fees, expenses, investment objectives, etc. N-SAR filings are collected from the SEC's Electronic Data Gathering, Analysis, and Retrieval system (EDGAR). In my sample, there are 13,931 funds from 1,627 fund families and 1,232 unique investment advisers.[10]

The third main data source regarding regulatory disclosures are Form ADV obtained through a Freedom of Information Act request. All investment advisers are required to file Form ADV under the Investment Advisor Act of 1940. U.S mutual funds are subject to comprehensive

[10]I use CRSP data to differentiate institutional share classes from retail share classes. And in N-SAR, fund inflows and outflows are reported separately. However, studies have found that the matching rate between CRSP mutual fund database and N-SAR filings are lower than 40%. To avoid selection bias, I use both sources of data independently throughout this study.

requirements under the Investment Company Act of 1940. These regulations provide investors with transparency, assure daily liquidity, and ensure the trustworthiness of funds' stated returns. On the one hand, disclosure requirements aim to provide adequate and prompt information to help investors make better decisions. On the other hand, regulatory enforcement actions aim to seek injunctions, prohibit future violations and disgorge illegal profits. Disclosure and enforcement together can protect investors through reduced information costs and ensure the proper functioning of the market.

Form ADV contains each adviser's SEC number, which is used as the primary identifier to merge with its N-SAR filings. For the remaining advisers not matched using and SEC number, as well as merging Form ADV with CRSP data, I match the investment advisers by their legal company names. Regulatory disclosures obtained from Form ADV contain all regulatory events initiated and amended by all regulators, including the SEC, the Commodity Futures Trading Commission, self-regulatory organizations such as FINRA, state regulators and foreign regulators. In this paper, I focus on the *initial* regulatory events to assess investors' initial response to financial misconduct, which is the first time public become aware of a particular investigation on investment adviser's misconduct.

Form ADV also discloses the principal products that are related to each regulatory event, such as "Debt," "Equity," "Insurance" and "Mutual Fund(s)." These disclosures include all types violations, e.g. specific trading regulation, insurance laws, failure to disclosure requirement, or failure to meet the registration requirement, and most of these regulatory disclosures have no direct impact on the value of assets in the funds. The main variable of interest in this study is an indicator variable *MF Fraud SEC*, which takes the value of 1 if there is a new regulatory disclosure about the investment adviser that is initiated by the SEC, with the principal product being "Mutual Fund(s)." I also define *MF Fraud All* as an indicator variable, using the value of 1 when there is a regulatory disclosure related to "Mutual Fund(s)" initialed by any regulators. Additionally, an indicator variable *Other Regulatory SEC* utilizes the value of 1 if there are regulatory events initiated by the SEC against investment advisers and related to products that are *unrelated* to mutual funds.

Additionally, SEC enforcement actions data against financial institutions are obtained from the SEC's website.[11] The names of financial institutions are matched to SEC enforcement data; these names are also matched with mutual fund names to determine whether a fund and financial institution are associated. The association is identified through one of the following channels. One channel is through name association whereby mutual funds and financial conglomerates share common parts in their names. (For example, Goldman Sachs Trust: Goldman Sachs Asia Equity Fund Class A Shares are associated with Goldman Sachs.) The other channel is by business association whereby the mutual fund management company is a registered subsidiary of a financial conglomerate. Using 10-K filings, I manually verify whether the management company is listed as a subsidy of the financial institution. The financial institutions included in the sample consist of the top 40 financial conglomerates ranked by total assets.[12]

To capture the severity of fraud and the media coverage. I manually check the amount of monetary fine as a result of each regulatory enforcement from form ADV. For all the investment advisers with at least one regulatory disclosure, I manually check the media coverage for the misconduct. Media coverage data is from Factiva. By searching key words "fraud, misconduct, scandal, enforcement", the indicator variable "media" takes value one if the mutual fund misconduct has media mentioning in the headlines. I restrict the news source to the major business media in the U.S. (i.e. Yahoo Finance, The Wall Street Journal, and The Dow Jones Newswire etc.)

In the study, the main dependent variable of interest is fund flows as a proxy for residual claim redemption. I defined fund flow following Sirri and Tufano (1998). For each fund class each month, fund flow is defined as follows:

$$Flow_{f,t} = \frac{TNA_{f,t} - TNA_{f,t-1} \times (1 + R_{f,t})}{TNA_{f,t-1}} \tag{1.1}$$

where $TNA_{f,t}$ is a fund's total net asset at month t, and $R_{f,t}$ is the fund's return over the prior month.

[11] All SEC enforcement actions can be found at he SEC website: Litigation and Administrative proceedings. The format of judicial and administrative proceedings is variable; however there is no rigid formula dictating the choice of forum. Each year, the SEC brings hundreds of civil enforcement actions against individuals and entities that violate federal securities laws.

[12] The list of financial conglomerates can be found in table 1.10 in Appendix.

From N-SAR filings, monthly fund gross inflows and gross outflows are directly reported in Item 28. N-SAR filings also reports average fund TNA over the report period in Item 75. The variable $Inflow_{i,t}$ ($Outflow_{i,t}$) is calculated by dividing the grossInflow$_{i,t}$ (grossOutflow$_{i,t}$) by $TNA_{i,t}$, and fund net flows are calculated as follows:

$$Netflow_{i,t} = Inflow_{i,t} - Outflow_{i,t} = \frac{\text{grossInflow}_{i,t} - \text{grossOutflow}_{i,t}}{TNA_{i,t}} \tag{1.2}$$

Following the mutual fund literature, the following control variables are included in the regression analyses (see Chevalier and Ellison, 1997; Nanda et al., 2004; Cooper et al., 2005; Barber et al., 2016). $Size_{f,t}$ is the natural logarithm of TNA of each fund share class at month t. $ExpenseRatio_{f,t}$ is the funds' expense ratio, which is the ratio of total investment that shareholders pay for the fund's operating expenses, which include 12b-1 fees. $Age_{f,t}$ measures how long the funds have been publicly traded, and I use the natural logarithm of fund age in the regression. $Return_{f,t-12,t-1}$ is the accumulated return of the fund in the previous year. Fund returns from N-SAR filings are calculated using net asset value and distributions reported in N-SAR (Item 73 and Item 74) as follows:

$$Return_{i,t} = \frac{NAV_{i,t} + Payout_{i,t} - NAV_{i,t-1}}{NAV_{i,t-1}} \tag{1.3}$$

To account for the common shocks to economic cyclicality, I include β_t to capture time fixed effects. In accounting for potential preference over certain investment styles, I include $AvgStyleFlow_t$ as a control, where funds are grouped based on funds investment objectives. All the continuous variables are winsorized at the 1% and 99% level.

1.2.2 Descriptive Statistics

Table 1.2 reports summary statistics of the characteristics for all equity funds from 2000 to 2016. Panel A reports the summary statistics for the CRSP sample and Panel B reports the summary statistics for the N-SAR Sample. Table 1.2 also reports control variables including past returns, TNA, fund flows, expense ratio, management fees, and fund age each month.

In the N-SAR sample, on average, funds experience monthly inflows of 5.1% and average fund outflows is 4.2%. At some point in time, 9.4% of funds have regulatory actions taken against them. In the CRSP sample, about 31.4% of observations are institutional share classes and 67.6% are retail share classes, and on average fund flows are 0.3%. At some point in time, 15.4% of funds have regulatory actions taken against them, and 12.3% in the sample are associated with financial conglomerates, including both name association and business association.

Institutional and retail share classes have comparable sizes in terms of dollar value and both have a highly skewed distribution. The mean TNA is around $1,060 million, while the median TNA is $171 million. The annualized fund return is about 6.3% for the overall sample, with a slightly higher return of 7.0% observed within the institutional share classes versus 5.9% for the retail share classes. Expense ratios for retail share classes is much higher, equaling 1.3%, while institutional share classes have an expense ratio of around 0.7%.

Figure 1.1 plots the overall number of regulatory disclosures from Form ADV as well as the overall number of regulatory disclosures for the *mutual fund* principal product. The seasonality in the figures is consistent with financial crisis-related fraud, as well as the *late-trading scandal* that relates to mutual fund regulatory events. Mutual fund-related regulatory actions are heightened from year 2003 to 2005; other regulatory actions (such as asset-backed securities related allegations) are clustered around the recent financial crisis.

1.3 Empirical Design and Results

1.3.1 Empirical Design

The baseline specification to test mutual fund investors' response to regulatory disclosure is as follows.

$$Flow_{f,t+s} = \beta_1 \text{MF Fraud SEC}_{f,t} + \beta_2 \text{Past Fraud}_{f,t} + \gamma_{f,t} \text{Controls}_{f,t} + \beta_t + \epsilon_{f,t} \tag{1.4}$$

Where $Flow_{f,j,t+s}$ is the fund flow defined in (equation 1.1 and equation 1.2) of fund f (fund share class f) from t to $t+s$, where s equals 1 and 3 windows[13]. Using N-SAR filing data, I examine the effects of $Inflow_{f,j,t+s}$ and $Outflow_{f,j,t+s}$ separately. $PastFraud_{f,t}$ is an indicator variable accounting for regulatory events in the recent past one (five) year(s) against an investment management company. Controls include fund size, expense ratio, age, past return and average fund flows within the same investment objectives defined in the data section. Time fixed effects are included in all regressions. Standard errors are clustered at the mutual fund family level to account for serial correlation within a fund family.[14]

Using both inflows and outflows, separately, from N-SAR filings, allows for the examination of the different aspects of investors' reactions. At the same time, using the share class-level data from CRSP facilitates the comparison of the two distinct investor groups, institutional investors and retail investors. Existing evidence suggests a high likelihood that these two groups of investors face different information and monitoring costs. To examine the market discipline outcome among different investor types, the first variations comes from separating retail investors from institutional investors. In additional to controlling for investors' financial literacy, the effects between separate sales channels are further explored.

1.3.2 Fund Flow and Regulatory Disclosure

Figure 1.2 presents the investment style-adjusted fund flows (inflows and outflows) six months before and after regulatory disclosures about mutual fund fraud. A significant drop in net flows and inflows is observed in the month after regulatory disclosures, whereas outflows increase in the month of disclosures and is even anticipated one month before. Table 1.11 presents consistent t-test results that investment style-adjusted net flows and inflows are significantly 0.67% (0.89%) lower after regulatory disclosures.

To control for other identified factors that affect fund flows, I use specification (1.4) to test the change in fund flows following regulatory disclosures about investment advisers' misconduct.

[13]In unreported analyses, most results are robust when extending the window to 6 months and 12 months.

[14]see Gaspar et al. (2006) and Nanda et al. (2004).

Panel A of Table 1.3 uses N-SAR data to show the fund flows (inflows, outflows) from month t to t+1 and t+3, respectively. On average, one and three months after regulatory disclosure, fund flows decrease by 1% and 1.9%, respectively. In other words, the month following a regulatory disclosure, 10.8% of a standard deviation decrease is observed in fund flows. Moreover, outflows are more pronounced immediately after regulatory events, and reverse overtime (captured by the negative coefficient of variable *Past Fraud* in columns 5 and 6). Alternatively, negative future inflows have a long-lasting effect. One drawback using monthly flow data is the difficulty in pinpointing the exact time of response when regulatory disclosures occur in the middle of the month. The effect could be underestimated using specification 1.4. Nevertheless, together these results suggest that investors show an immediate response to negative regulatory signals.

Panel B of Table 1.3 reports very similar results using the CRSP sample. Columns (1) - (2) in Panel B of Table 1.3 report all equity fund share classes. Columns (3) to (4) report the fund flows for institutional shares classes, and columns (5) to (6) report those of the retail counterparts. Importantly, the results show that the effect of regulatory disclosures on fund flows is twice as strong among institutional share classes. For example, in the month following regulatory disclosure, institutional share classes experience on average a 0.6% negative flow, which is equivalent to 13.7% of a standard deviation decrease. On the contrary, retail share classes only experience a 0.3% negative flow in the month following regulatory disclosure, equivalent to 8.2% of a standard deviation decrease. Also, there is a significant delay in retail investors' response, indicated by the significant negative coefficient of the variable *Past Fraud (5-year)*.

Heterogeneity between retail investors and institutional investors has been documented in previous studies. Investors' asset allocation decisions are largely affected by factors beyond rational expectations. Individual investors are net buyers of attention grabbing stocks (Barber and Odean, 2008), and salience affects their investment decisions significantly (Cooper et al., 2001; Rashes, 2001; Cooper et al., 2005; Sialm and Tham, 2015). On the contrary, professional investors respond to proper performance signals and act (Del Guercio and Tkac, 2002). My results further confirm that institutional investors are better informed about regulatory disclosures and market discipline is a much more effective force levied at the hands of better informed investors.

Investor sophistication contributes to the discrepancy in reactions between institutional and retail investors. There are two channels through which individuals can buy and sell funds, either through a full-service broker or discount broker. With discount brokers, investors can place buy and sell orders at a reduced commission. Full-service brokers provide retail investors professional investment advices that help mitigate investment sophistication discrepancy, which are not available through discount brokers. Following ICI criteria, the direct-sold funds are defined as funds without front or rear loads, and the 12b-1 fee being less than 25 basis points.[15]

In Panel A of Table 1.4, I find that negative fund flows are only significant within broker-sold funds, and direct-sold funds show lack of flow discipline. Previous research suggests that individual investors do not always make asset allocation decisions rationally, and their trading activity can negatively impact their wealth (i.e. Barber and Odean, 2000; Cooper et al., 2005). My results suggest that with professional advice, the sub-optimal behavior of retail investors can be mitigated, at least partially. These results support the findings in Bergstresser et al. (2009), that brokers can provide intangible benefits to investors even if there is virtually no difference in fund returns between broker-sold and direct-sold retail funds.

1.3.3 Management Fees and Regulatory Disclosures

Investment advisers compete on both the size of assets under management as well as management fees. Investors could also discipline fund managers by negotiating lower fees. In addition to fund flows, it is also important to understand the impact of regulatory disclosure on mutual fund management fees. Similar to fund flow analyses, the specification below is used to analyze the effect of regulatory disclosures on future management fees paid by investors.

$$\Delta \text{Mgmt Fee}_{t,j,t+s} = \beta_1 \text{MF Fraud SEC}_{f,j,t} + \gamma_{f,j,t} \text{Controls}_{f,j,t} + \beta_t + \beta_j + \epsilon_{f,j,t} \qquad (1.5)$$

In Panel B of Table 1.4, I find a significant reduction in fund management fees of 2% to 5% within two years after a regulatory disclosure, with the only exception being direct-sold retail

[15] Also see Sun (2014).

funds. Among most investor classes, mutual fund misconduct is quite costly. Over the following six months after a regulatory action, funds experience cumulative -3.4% net flows. Based on the average fund size of $2,362 million, mutual funds lose up to $80 million of assets under management. Moreover, fund advisers have to lower management fees to prevent further negative flows; on average, each regulatory disclosure can reduce the investment advisers' profit by about 3.8%. Under necessary conditions, market discipline is rather effective. However, the effectiveness of market discipline varies among investor bases.

The weaker reaction from retail investors, especially among direct-sold funds, is noteworthy. Theory suggests that market discipline prevails only if (a) information is publicly available with no (very low) cost and (b) incentives to monitor are sufficiently high and cost of actions are sufficiently low. In the next section, I explore the possible mechanisms that contributes to the asymmetric market discipline outcome.

1.4 Effective Information Costs and Regulatory Disclosure

Heterogeneity in information costs benefits informed traders at the cost of the uninformed (Grossman, 1976). Regulatory disclosures are public signals, however, investor sophistication can affect their ability to efficiently process information (Barber and Odean, 2000). As a result, the effective cost of information increases which ultimately influence their ability to exert market discipline. In this section, I show that investor sophistication together within information quality can hinder the efficacy of market discipline.

1.4.1 Visibility

Previous studies also showed that fund flows are affected by fund family size, media attention and past performance (see Chevalier and Ellison, 1997; Sirri and Tufano, 1998; Berk and Green, 2004). A nudge, in behavior finance, influences investors' decision making, making it more likely that investor will make a particular choice with the desired outcome (Thaler and Sunstein, 2008). Investors' inertia can, therefore, be offset by increasing visibility, serving as information nudge.

To further examine its impact on market discipline, I separate the funds into groups with different search costs based on fund family visibility.

First, the funds are ranked based on the size of the families to which the funds belong, where fund family size is measured by the total net assets under management within the family. Large-fund groups represent funds with a family size that is above the median. Large-fund families are expected to have better marketing and distribution outcomes, as well as generating media attention, which results in higher investor familiarity. Information costs are much lower among large funds in comparison to that of small funds.

Table 1.5 provides results to support this hypothesis. Panel A of Table 1.5 finds a significant negative flow reaction to regulatory disclosures among the funds belonging to large-fund families. Column (2) and (4) in Panel A shows that large-fund families experience 0.5% and 1.1% negative fund flows, respectively, one and three months following the regulatory disclosures. Column (1) and (3) shows virtually no flow discipline among funds from small families during the same duration. In unreported analysis, the institutional funds are separated from retail funds. I find that the large discrepancy in fund flows reaction come mainly from retail share classes. Both large and small institution funds experience negative fund flows following regulatory disclosures (columns 1 to 4); whereas the negative fund flows are only observed among large retail shares (columns 5 to 8).

On the other hand, funds with better past performance exhibit the so-called “*star*" phenomenon. These “*winner*" funds are often subject to better exposure, which generates spillover effects to other funds within the same fund family. To identify a *star* family, the procedure in Nanda et al. (2004) is followed. First, the Fama-French three-factor adjusted return is calculated for each fund each month; then the risk-adjusted returns are ranked into percentile, and the funds are flagged that belong to the top 5^{th} percentile as a *star fund*. For each month, I name a fund family as “*star family*" if at month t any fund belonging to the fund family is identified as a *star fund*.

In Panel B of Table 1.5, the negative fund flows are found to be more significant among the funds from a *star family*, both statistically and economically. Column (4) shows that funds from a *star family* experience 1.2% negative flows after regulatory disclosures, whereas funds from non-star families experience only one-third of the effect (0.4%).

1.4.2 Severity

Investors' inattention affects investments performance (DellaVigna and Pollet, 2009; Dahlquist and Martinez, 2015; Sialm et al., 2015). Investors' inertia can be offset by external mechanisms that reduce effective search costs. For example, severe events are more attention grabbing; investors and update their information set through learning from the past. In addition, previous results suggest that media, compare to public disclosure platform, a direct way to attract investors' attention is through media coverage of mutual fund misconduct events.

In Table 1.6, I examine the possible explanation for heterogeneous information friction. First, I examine whether investors learn about misconduct from the past. If retail investors face an information disadvantage, multiple offenses should offer learning opportunity for investors; thus the search costs for the repeated offense should be substantially lower. The results in Table 1.6 are consistent with this hypothesis. For retail investors, the response to the repeated offense is about -0.6% (0.2% + 0.4%), which is comparable to the fund flow responses from their institutional counterpart. These results suggest that learning is one way to mitigate information friction. And once the effective information costs are reduced, retail investors behave very similarly to institutional investors in disciplining financial misconduct.

Investor's attention are also likely to be correlated with the severity of an event. Retail investors, in particular, have limited time and resources to obtain financial information. Severe events are more likely to attract their attention. For example, regulatory actions can be initiated by the SEC and other regulators. The median monetary fine for SEC enforcement is $8 million dollars, whereas the median monetary find for enforcement by other regulators is about $400,000. In Table 1.6, I find that retail investors are less likely to respond to less severe fraud disclosures, measured both by regulator and monetary fine. For example, negative fund flow responses are about 0.6% from retail investors if the monetary fine is above median. Also, more severe or high profile mutual fund fraud are likely to be covered by major media outlets, further reducing information friction.

Indeed, the marginal effect on fund flows from media coverage is about -0.3%, both economically and statistically significant.

1.4.3 Relevancy

Less sophisticated investors also have inferior ability to process information. Therefore information quality also affects investment decision significantly, especially for the investors with lower financial sophistication. The ability to effectively extract relevant information from noisy signals is crucial for investment decision making. Market discipline breaks down if investors under-react to relevant information or over-react to irrelevant information. Two types of financial misconduct are considered to be *irrelevant* to mutual funds. First, the group of funds whose investment management companies are affiliated with large financial institutions is analyzed.[16] The affiliation is identified through two non-mutually exclusive channels. The funds can be associated with a financial conglomerate when they share common elements in their names; funds and financial institutions can be affiliated if mutual fund management companies are registered subsidiaries of large financial institutions. When the SEC initiates regulatory actions against the financial conglomerates that mutual funds are associated with, rational investors should show no response if such misconduct has no impact on the value of mutual fund assets.

The results in Table 1.7 indicate divergent responses. In Panel A, results show that institutional investors are able to detect the fallacy; we observe no fund flow reaction following regulatory disclosure against the financial institution. On the contrary, retail investors react as if such regulatory actions are indicative of investment adviser fraud and punish the management companies. More interestingly, the magnitude, 0.4% negative flows, is even stronger than the *actual* mutual fund misconduct. One explanation for this result is that the enforcement actions against large financial institutions are much more visible via media coverage to the general population. The results convey that individual investors cannot effectively distinguish noise from signals, whereas institutional investors are less affected by information noise.

[16]Table 1.10 provide the list of large financial institutions based on their assets, I only include the top 40 largest financial institution in this study.

Second, the group of funds with misconduct not related to the mutual fund itself is analyzed. Form ADV provides information about the specific product to which the regulatory charges are related. Mutual fund investors should be concerned about misconduct related to “Mutual Fund(s)." In Panel B of Table 1.7, fund flows following all other types of regulatory disclosures initiated by the SEC are examined. Institutional investors show no response to such disclosures; whereas we observe moderate levels, or 0.4% negative flows over three months from retail investors. It suggests irrational behavior by retail investors in a noisy environment. These results are noteworthy as we have the tendency to equate information quantity to information quality. Transparency matters. The manner in which information is communicated is also important. Improving information quality so that disclosures help safeguard investors and reduce information noise deserves more attention.

1.5 Monitoring, Investor Mobility and Regulatory Disclosure

Whether market discipline is effective also depends on investor's incentive to monitor. Monitoring costs are high for retail investors, which would reduce their incentive to exert effort. However, retail investors can benefit from the ability and willingness of institutional investors to exercise market governance through reduced agency problems from greater institutional monitoring (Evans and Fahlenbrach, 2012). Also, some of the retail investors are active traders (Barber and Odean, 2000), compared to others such as retirement funds investors. Among funds where investors closely monitor fund performance, we should observe a strong disciplining effect.

1.5.1 Monitoring

First, I split the retail funds into two groups to proxy for investor monitoring, retail funds with an institutional fund twin and the rest. Retail investors can benefit from monitoring by institutional twin fund, and follow the investment strategy by their twin. In Panel A of Table 1.8, I find that the retail funds show much stronger negative fund flow responses when they have an institutional twin fund. The negative fund flows are 0.5% for the month following the regulatory disclosures for the retail funds with an institutional twin, compared to 0.2% without. The results suggest that investor's incentive to monitor has significantly impact on the effectiveness of market discipline. In Panel B of Table 1.8, I use volatility of abnormal outflows as a proxy for the intensity of investor monitoring. When funds are under high scrutiny, regulatory disclosure results in much stronger negative fund flows (1.92%). The fund flow responses are insignificant (-0.26%) among the funds with low level of monitoring intensity.

These findings suggest that professional investors monitor investments more closely than retail investors and react more strongly to negative signals. However, when retail investors who actively monitor funds, or have reduced cost to monitor through institutional twin funds, the fund flow responses become much more sensitive to regulatory disclosures.

1.5.2 Investor Mobility

Hubbard et al. (2010) argue that the mutual fund industry is price competitive only if investors can easily "fire" their investment advisers. Investor mobility facilitates competition in the mutual fund industry. Factors such as capital gains taxes, sales loads and outside investment options can affect investors' ability to switch funds. Market discipline is effective only when capital can be moved freely into and out of funds (Dangl et al., 2008). In this section of the paper, I further link how changes in investor mobility affect the efficacy of market discipline.

1.5.2.1 Capital Gains Overhang

Barclay et al. (1998) show that capital gains overhang increases expected future taxable distributions, and hence the present value of new investors' tax liabilities. Ivkovic et al. (2005) also find supportive evidence that capital gains overhang affects investors' trading behavior. Unrealized capital gains affect investor mobility; particularly for taxable investments, high levels of unrealized returns increase the investor's tax liability when redeeming assets. Next, I explore how the level of capital gains overhang can affect investors' ability to discipline funds. Following Barclay et al. (1998), capital gains overhang is calculated for each fund each month as in 1.6. Then, each month funds are ranked into terciles based on the level of capital gains overhang to examine the effect of flows following regulatory disclosure.

$$\begin{aligned} Overhang_t = Overhang_{t-1} + (NAV_t - NAV_{t-1}) \times Shares_{t-1} \\ + (Shares_t - Shares_{t-1}) \times (NAV_t - \text{Acg Price Paid for New Shares}) \end{aligned} \tag{1.6}$$

The results are presented in Panel A of Table 1.9. Outflows after regulatory disclosures increase significantly only for the funds with low level of capital gains overhang. High levels of overhang deter future inflows, that the reduced inflow is more pronounced within the group of funds with higher level of capital gains overhang. The results suggest that the level of unrealized capital gains indeed affect investors' disciplinary power, however, through different channels. Disciplining capacity of existing investors and future investors are significantly affected by the tax liabilities they face. Whether there exist conflict of interest between fund management companies, existing and future investors, and the related potential wealth transfer implications needs to be further explored.

1.5.2.2 Investment Options

Capital reallocation is associated with a considerable amount of transaction costs, both direct (loads) and indirect (outside investment options). How indirect costs (opportunity costs associated with the search for alternative investments) affect the efficacy of market discipline are examined in this section. When outside investment options are limited, investors have lower incentives to react. Therefore, the sample is split into different time periods based on outside investment options' availability.

More specifically, the *late-trading scandal* period is examined, as well as during the *financial crisis*. The financial crisis has had a major impact on investors' perceptions about the market environment as well as confidence in the asset management industry. Overall, a loss of confidence in the asset management industry results in increased search costs and reduced investor mobility. On the other hand, the *late-trading scandal* provides a strong signal for investors to filter investment opportunities and results in lower monitoring costs.

In Panel B Table 1.9, I find that fund flows are statistically insignificant from zero following regulatory disclosures during *financial crisis* era. In contrast, during the *late-trading scandal* period, it required much less effort to distinguish *good* funds from *bad* funds. I find that negative fund flows are 50% stronger (1.5% decreasing in flows compared for 0.5% of the average effect) during *late-trading* periods. More interestingly, the increased outflow is only significantly when outside options are cheap. The results suggest that elevated level of search costs for alternative investment options can significantly affect market discipline negatively.

1.5.2.3 Sales Loads

Sales loads increases transaction costs of asset redemption as well. In Panel A of Table 1.4, I show that broker-sold funds are more sensitive to regulatory disclosure, and the benefit from professional investment advice seems to outweigh the sales costs. In Table 1.12, I further investigate inflows and outflows separately to understand the role brokers play.

The results in Table 1.12 indicate that the net flow of broker-sold funds is predominantly derived from increasing outflows. Compared to direct-sold funds, broker-sold funds with either front sales loads or rear sales loads both experience high levels of outflows immediately following regulatory disclosures; decreasing inflows have a smaller magnitude. These results suggest that the benefits investors gain from professional investment advice (active investing) can outweigh the direct transaction costs. This can be one of the important intangible benefits the broker can provide to retail investors, mitigating the high cost of inattention.

Overall, evidence convey that restricted investor mobility hinders the efficacy of market discipline. More specifically, tax liabilities and search costs of alternative investment options are the primary factors affect investors' ability of asset reallocation. Transaction costs paid by the investors are somewhat offset by the reduced information friction through the investment advice provided by brokers.

1.6 Discussion and Conclusion

Enforcement mechanisms are used to deter future violations to protect the public interest. The efficacy of enforcement is determined by the costs and benefits from such actions. In particular, mutual fund enforcement deserves special attention due to the unique governance structure and lack of authoritarian role of regulators in the mutual fund market. Regulatory enforcement in mutual funds is limited to facilitating transparency, whereas market enforcement complements to a large extent through asset redemption.

By using public regulatory disclosures against mutual fund investment advisers, I find that as theory predicted, when information costs are sufficiently low and investor mobility remains unhindered, market discipline serves as an effective governance mechanism in mutual funds. However, heterogeneity among investors exists in their ability to respond to enforcement. Among possible channels, financial sophistication, information costs and investor mobility have a considerable impact on investors' disciplinary power. Both search costs and information quality contribute significantly to investors' ability to discipline investment advisers, in the presence of heterogeneous investor sophistication. Also debatable is whether mandatory information disclosure itself is sufficient to guarantee low information costs. Ultimately, regulatory disclosures should aim at improving information quality that better communicates with less informed investors. I also find that limited investor mobility can prevent the necessary disciplinary action from investors. When outside options are expensive, or when faced with high levels of penalties to transfer assets (e.g., tax liabilities), the market's disciplining mechanism is less effective.

This paper highlights the importance of this unintended outcome of segmentation in market discipline. The asymmetric investor reactions could affect the managers' incentives, ex ante, which could results in wealth transfer at the cost of certain types of investors. Moreover, investor heterogeneity exists beyond the scope of mutual funds, that shareholder characteristics could affect the corporate governance similarly. My results suggest that future research could be focus on how shareholder heterogeneity and composition could affect external corporate governance outcomes.

CHAPTER 2

CORPORATE CULTURE AND CORPORATE FRAUD

2.1 Introduction

Managers often assert that having an appropriate culture is critical to a firm's success. For example, in a recent survey of CEOs and CFOs by Graham et al. (2015), 91% of respondents said that they thought that culture was "Important" or "Very Important," and 78% think that it is a top 5 value driver for their firm. One particular dimension of culture that has received much attention from regulators, auditors, and academics, is the degree of integrity in a firm's culture and its relationship with corporate fraud.[1] Yet, there is relatively little quantitative research that tests whether an unethical culture actually predicts corporate fraud. This is perhaps because culture, by its very nature, is difficult to define and measure. Most research relies on surveys and interviews to measure culture, which provide important insights into some facets of culture. However, self-reporting biases may be particularly severe for surveys of integrity: for example, employees of firms with questionable ethics are perhaps more likely to lie or embellish their responses to surveys.

In this paper, we move beyond self-reported measures to study a measure of integrity based on individual employee actions: the decision to register for, and use, AshleyMadison.com ("AM"), a website that facilitates extramarital affairs.[2] We assign AM users to firms based on the domain name taken from their email accounts, resulting in a sample of approximately 47,000 individuals who used their corporate email account to register and actively use an AM account over the 2002-

[1] For example, in a speech to members of the financial services industry on October 20, 2014, William Dudley, President and Chief Executive of the Federal Reserve Bank of New York says: *"Supervisors simply do not have sufficient "boots on the ground" to ferret out all forms of bad behavior within a giant, global, financial institution. Moreover, regardless what supervisors want to do, a good culture cannot simply be mandated by regulation or imposed by supervision...It is up to you to address this cultural and ethical challenge.*

[2] We use anonymized data on individual users and do not conduct any analysis at the user level. Furthermore, we do not disclose in any way the names of corporations with employee email accounts in the database. We have received exemption from Institutional Review Board and approval by the universities with which we are associated because of the anonymization process, public availability of the data, and the aggregate nature of the measures that enter our analysis.

2014 period. Our key variable of interest is the number of active users at any point in time in a given firm, where *active* means the user has not only registered, but also exhibited some activity in the account (e.g. purchased credits to send a message).

We hypothesize that AM membership reflects a firm's emphasis on integrity. Because a firm is more likely to attract, select, and retain employees who match its culture (Schneider, 1987), we expect that individual employee traits provide information about corporate priorities. Firms that do not emphasize integrity in their cultures are more likely to employ individuals who display a lack of integrity. Overall, AM membership reflects both a focus on integrity at an individual employee level, and the focus on integrity in the systems and policies in place at firms.

We test whether AM membership predicts future corporate fraud. In particular, we test whether firms with greater AM membership are more likely to be subject to SEC enforcement actions due to accounting misstatements. Dechow et al. (2011) find that a host of financial variables predict SEC enforcement actions. We find that after controlling for all these variables, greater AM membership predicts a greater likelihood of *future* enforcement actions. Moreover, these results are economically significant: a one standard deviation increase in AM membership is associated with a 0.104 percentage point increase in the rate of detected fraud, which is more than double the unconditional mean.

Despite its advantages, AM membership is by no means a perfect measure of corporate ethics. One potential concern is that we can only observe the fraction of employees who use their official email account to register for AM. Besides perceptions of systems in place, there could be other reasons to use an official email account that are likely to make AM membership a noisy measure of corporate ethics.[3] Another related concern is that the number of AM users that we observe constitutes a small fraction of the firm's workforce. Around 50% of firms have zero AM membership and the mean AM membership is 5.4 employees for the firms that have at least one AM member. While the small fraction of AM membership likely adds noise to the measure, it should also bias against finding results.

[3]For example, employees could be unfamiliar with the perils of electronic communications, or may believe that their spouse does not have access to official email identification.

Nevertheless, we run a battery of tests to ensure that the relation between AM membership and corporate fraud is robust. In addition to our variable on the number of AM employees, which could be a noisy measure of culture, we also construct a dummy variable equal to one if a firm has at least one AM account for at least two consecutive years. We find that this dummy variable also predicts future misstatements after controlling for standard determinants. Another concern is that there may be heterogeneity across industries or geographies in AM usage. All our results hold after including industry and geography fixed effects. Additionally, there may be non-linearities in fraud with respect to firm size. We therefore match firms on size (number of employees) and find that AM firms are 75% more likely to have accounting misstatements than size-matched non-AM firms.

Finally, it is possible that these results may be sensitive to the choice of accounting misstatements as a measure of corporate misconduct. We therefore test whether AM membership predicts an alternative measure of corporate ethics: ratings of firms on ethical issues by external analysts at KLD. We find that a one standard deviation increase in AM membership is associated with a 2.65 percentage point increase in analysts perception of significant concerns regarding bribery and corruption. This effect is economically quite large, since the unconditional average is only 4.7%, and the average for firms with no membership is 3.39%. We also find that AM firms are more likely to be involved in tax avoidance via use of tax havens, pay lower taxes than similar companies, and are more likely to be rated as having tax-related concerns by KLD analysts.

Our results provide insight that, at a minimum, AM membership captures an important source of unobserved heterogeneity across firms, which predicts substantive firm-level outcomes. After controlling for standard variables, AM membership has incremental predictive power for future accounting misstatements and external analyst perceptions of unethical behavior. These results are consistent with the hypothesis that firm culture and ethical behavior are closely linked.

Our paper is specifically related to research that attempts to quantify corporate culture. Kim et al. (2012) use analyst ratings to examine whether socially responsible firms are also responsible along various dimensions of financial reporting. Popadak (2013) measures culture based on a textual analysis of employee reviews of firms from career intelligence websites, and finds that stronger shareholder governance causes firms to focus on observables and neglect intangibles such as collaboration and integrity. Guiso et al. (2015) and Garrett et al. (2014) measure integrity using surveys that ask employees whether they believe that senior managers in their firms are ethical. We also focus on integrity, but our measure is akin to a revealed preference. Rather than surveying employees, we infer the importance of integrity in a firm's culture using the actions of a subset of the firm's employees.

Moreover, our results are related to prior research that examines the effect of CEO personality on firm outcomes (e.g., Jia et al. (2014), Schrand and Zechman (2012), and Gormley et al. (2013)). In particular, recent work by Mironov (2015), Cline et al. (2016), and Griffin et al. (2016), shows that a CEO's personal indiscretions and corrupt behavior are associated with firm level corruption, ethical violations, and class action lawsuits. While we also document a strong association between personal and professional ethics, our analysis is broader in the sense that it includes employees from all levels of a firm, not only upper management. This is consistent with anecdotal evidence that suggests that "rank and file" employees, rather than top management, were responsible for unethical corporate behavior in a number of recent corporate scandals.[4] Moreover, the choice of CEO is endogenous with respect to firm culture; we find that firms with lax cultures are more likely to choose internal CEOs relative to firms with more ethical cultures, thereby perpetuating their current culture.

2.2 Data

2.2.1 The AshleyMadison Data

AshleyMadison.com is a dating website for people who are married or in a committed relationship. The website was created in 2002 and quickly became the world's largest online social networking

[4]For example, AIG's Joseph Cassano and Drexel Burnham, and Lambert's Dennis Levine (both employees well below the level of corporate executive), each played a large role in their firm's troubles during the financial crises of 2008 and the late 1980's, respectively. Similarly, it appears that engineers, and not top executives, at Volkswagen installed software intended to mislead emissions testing. While it is likely that Martin Winterkorn (the CEO) played a role in determining the culture, it was the ethics of "rank and file" employees that led to scandal, and ultimately a large loss in shareholder wealth.

community for people who wish to engage in extramarital affairs.[5] While signing up on Ashely-Madison is free, users must purchase credits to send custom messages, initiate chat sessions, send priority messages, or send virtual gifts. By late August 2015, information for the majority of AM accounts became available on a variety of websites and received a great deal of media attention.[6]

Many of the accounts on AM were registered using corporate email addresses. We link these email accounts to firms using corporate domains identified from the WebURL from Compustat and LexisNexis corporate affiliations. We merge the matched list to the Compustat database using ticker symbol and company name. We then hand-check each domain-company link to verify its validity. We exclude certain domains that are likely being used by people who are not employed at the firm to which the domain belongs. For example, we exclude domains such as "yahoo.com," "facebook.com," "aol.com," "google.com," and "verizon.com." After applying these filters, our final sample includes 12,687 company domains in the Compustat database from 2002-2014. Using these domains, we are able to match 46,649 employees to companies who used the corporate domain name with which they created an AM account from 3,469 different companies. We do not in any way disclose the names of individuals or corporations that have accounts in our dataset.

For each account we observe the date that the account was created, the age of the user, the gender of the user, the city (zip-code) in which the account was created, the first date that an email or message was sent, the last date that an email or message was sent, and whether the account user purchased any credits. For the majority of our analysis we restrict our focus to accounts that exhibited some level of activity (e.g., a custom message was sent, a chat session was initiated, or credits were purchased for the account). This excludes "phantom" accounts that were created by mistake, as a practical joke, or by someone who immediately appears to have had second thoughts about their actions.[7] Furthermore, since we can only observe the dates for the first and last email,

[5]http://www.prnewswire.com/news-releases/hollywood-courts-toronto-based-ashley-madison-75587257.html "Hollywood Courts Toronto-based AshleyMadison." www.prnewswire.com. Retrieved 2015-10-24.

[6]For example, on August 19, 2015 the Washington Post published that thousands of accounts were linked to the U.S. military and the U.S. government. *Inside Higher Ed* reported that more than 74,000 accounts at AshleyMadison.com were from universities and colleges with '.edu' email accounts.

[7]In unreported results, we relax this restriction to include possible "phantom" accounts and the results are largely unchanged.

or message, we assume that an account is active in the intermediate time between its inception and the last observed activity. We define the variable $activeaccount_{j,t}$ as a binary variable equal to unity for the years in which an account is active according to our definition, and zero otherwise.

We create our primary variable, $active\ AM\ accounts_{i,t}$, by summing the number of accounts with a corporate domain name that belongs to firm i and that have exhibited some level of activity on or before time t:

$$activeAMaccounts_{i,t} = ln(\sum_{\tau=0}^{t}\sum_{j=1}^{N} 1[domain(activeaccount_{j,t}) = corpdomain_i] + 1)$$

We use the natural log of the number of active AM accounts as our main variable, and not the ratio of AM accounts to the total number of employees at a firm, because the Compustat item, *emp* (i.e., the number of employees), is only a noisy approximation.[8] However, we control for the (log) number of employees and (log) market capitalization in all our specifications.

Table 2.2, Panel A, reports basic descriptive statistics. The average number of AM accounts per firm is 2, and conditional on having at least one account, the average number of accounts is 5.4. Given the relatively small number of AM accounts per firm, it is possible that the number of AM accounts is a noisy measure. In additional robustness tests, we use a dummy variable equal to one if the number of active AM accounts is greater than zero for at least two consecutive years, and zero otherwise as an alternative explanatory variable. The average age of an AM user is 39 year old. The ratio of males to females is roughly two to one.[9] In Table **??**, we report industry and geographic statistics for our sample. As Table **??** documents, AM membership services are used by high-tech industries, while low-tech and defense industries are less frequented. In later analyses, we control for heterogeneity across industries through geography and industry fixed effects transformations.

[8]The number of employees at a firm is not an audited number and firms strategically misreport employment numbers (e.g., Beatty and Liao, 2012). As a result, there is not a standard way for firms to report this number (e.g., some firms report the average number of employees and some report the number at year-end). In addition, the *emp* item typically includes part-time, seasonal, and foreign employees. Scaling by a number that includes foreign employees could potentially bias our results, since our AM membership measure is composed of only domestic employees. Finally, there are only a few AM accounts per firm, relative to the total number of employees at the firm. Taking the ratio would result in a denominator that is several orders of magnitude larger than the numerator and that exhibits a large degree of measurement error.

[9]In unreported results, we used only males or females, or the number of AM users along with the ratio of males/females as an additional control. The results are qualitatively similar to those reported in the paper.

We report regression results for the determinants of AM membership in Table 2.7. The larger (as measured by market capitalization or number of employees) and older firms have a higher rate of AM users. Firms with higher AM membership rates also tend to be headquartered in areas with large populations and relatively low incomes. For these reasons it is important that our results are robust to controlling for various measures of firm size and regional heterogeneity in AM rates.

2.2.2 Other data

For data on corporate social responsibility, we use the MSCI KLD STATS from 2002-2014. KLD data are detailed annual statistics of performance indicators developed by MSCI analysts who provide research for institutional investors. To create these performance indicators, MSCI analysts use government databases, company disclosures, and macroeconomic data to assess company performance with respect to meeting stakeholder needs regarding environmental, social, and governance factors. Mattingly and Berman (2006) and Kim et al. (2012) suggest that the KLD data is well suited for studying corporate social responsibility. Note that Kim et al. (2012) document a strong association between KLD ratings and financial reporting standards, which is reassuring for our analysis since we use both variables as proxies for corporate ethics. For the purpose of our study, we focus on the particular indicators we consider to be closely related to integrity, which is the dimension of corporate culture we intend to study. The KLD indicators are broken down into *strength* and *weakness* categories.

Our first variable, *Bribery and Fraud* is a binary variable equal to unity if a firm has experienced severe controversies related to bribery, tax evasion, insider trading, and accounting irregularities in a given year, and zero otherwise. Similarly, *Tax Disputes* indicates whether a firm has had major tax disputes within a given year. The variable *Product Quality* assesses how companies manage their risk of facing major product recalls or losing customer trust through major product quality concerns. Companies that score higher are those that proactively manage product quality by achieving certification to widely acceptable standards, undertaking extensive product testing, and building processes to track raw materials or components. The variable *Human Rights* measures the severity of controversies related to a history of involvement in human rights-related legal cases; widespread or egregious complicity in killings, physical abuse, or violation of other rights; resistance to improved practices; and criticism by NGOs or other third-party observers. Firms that are guilty of worse human rights violations have negative scores. Lastly, *profit sharing* indicates whether a company has a cash profit-sharing program through which they have recently made distributions to a significant proportion of their workforce. Note that the first two variables (*Bribery and Fraud* and *Tax Disputes*) are binary, and the other KLD variables are the sum of binary sub-components and hence can take on values other than 0 or 1. All variables are defined in detail in Table 2.1.

Data on misstatements from 2002-2014 come from the AAER data set discussed in Dechow et al. (2011). This dataset provides detailed information regarding SEC investigations of public corporations for financial misstatements and has been commonly used in accounting research to study misreporting. Feng et al. (2011) study the AAER database and provide evidence that pressure from CEO's cause CFO's to become involved in material accounting misstatements. In a closely related study to ours, Garrett et al. (2014) use the AAER database to show that trust in top management, measured at various employee ranks, strongly predicts financial reporting quality.

Our tax havens data come from Dyreng and Lindsey (2009), who download every 10-K through the SEC's Edgar database between 1994 and 2014 and search every 10-K filing (Exhibit 21) for country names. Countries are identified as tax havens if they are defined as such by by three of the four following sources: (1) Organization for Economic Cooperation and Development (OECD), (2) the U.S. Stop Tax Havens Abuse Act, (3) The International Monetary Fund (IMF), and (4) the Tax Research Organization.

Firm accounting and financial information come from Compustat from 2001-2014. We also use stock price and return data from CRSP to calculate volatility measures and portfolio returns. A full description of all variable definitions is provided in Table 2.1.

2.3 Corporate Fraud and Ethics

In this section we examine whether greater AM membership among the employees in a firm is related to unethical behavior by the firm. We consider three sets of measures of unethical behavior: SEC enforcement actions for accounting misstatements, KLD ratings on ethics-related variables, and measures of tax avoidance. The first two are clearly related to corporate ethics. Tax avoidance is more nuanced. Managers have a duty to act in the interest of shareholders, and minimizing a firm's tax bill using legal strategies is consistent with this duty. However, if managers use extremely aggressive measures such as tax havens, this suggests that they are willing to walk close to the line of legality. We therefore include tax avoidance as an additional measure, but with the qualification that it is not as clear a measure of ethics as the two other measures we consider.

First, we follow related work by Schrand and Zechman (2012), Garrett et al. (2014), and Jia et al. (2014) and use SEC enforcement actions due to misstatements as a measure of corporate ethics. We follow Dechow et al. (2011) and examine three models. Model 1 (Specifications 1 and 2) includes AM-variables and financial statement variables. Model 2 (Specifications 3 and 4) adds non-financial statement and off-balance-sheet variables, and Model 3 (Specifications 5 and 6) incorporates market-based measures. As in Dechow et. al. (2011), we use a logit specification. We augment the models in Dechow et al. (2011) by adding industry and year fixed effects transformations. We report our results in Panel A of Table 2.3.

AM membership strongly predicts the probability of accounting misstatements, after controlling for other potential determinants studied by Dechow et al. (2011). The unconditional probability of misstatements in our sample is 0.67%. Increasing *Active AM Accounts* by one standard deviation results in an increase in misstatement probability by 0.36-0.53 percentage points (54-79% of the unconditional mean). The effect is even more pronounced in specifications in which we use a dummy variable ($Dummy(AM > 0)$) as our explanatory variable. Companies with at least one AM account have a 0.84-1.06% higher probability of accounting misstatements (126-159% of the unconditional mean). Note that these effects hold after controlling for all the other determinants of fraud studied by Dechow et al. (2011). The magnitudes on the control variables are similar to the results in Dechow et. al. (2011).[10]

Specifications 5 and 6 also include the log number of employees as an additional control besides the variables from Dechow et al. (2011). The AM variables retain significance after controlling for the number of employees. It is possible that non-linearities in size drive these results. This explanation requires that AM membership is non-linear in firm size and the probability of misstatements is non-linear in size as well (perhaps due to greater monitoring of the largest firms).[11] To mitigate concerns that our results on the predictive ability of AM membership for accounting misstatements are due to non-linearities in size, we use a matching approach. The 'treatment' sample consists of firms with at least one AM account. For controls, we match firms in the same year, and same Fama-French 48 industry with the closest number of employees. We impose the restriction that the number of employees must be within 10% of the firm with AM membership to be included in our sample.[12] It is important to note that the matching criteria is quite stringent and results in a relatively high loss of observations. In the matched sample, firms with a positive number of AM accounts are 1.19% likely to misstate financials, whereas matched firms without AM membership are only 0.68% likely to misstate financials. The difference of 0.51% is economically large and statistically significant at the 10% level.

We then turn to KLD ratings as a measure of ethics in Table 2.4. We examine four categories that are related to corporate ethics: *Bribery and Fraud, Tax Disputes, Human Rights,* and *Product Quality*. The *Bribery and Fraud* variable is related to accounting misstatements, but is wider in scope. *Bribery and Fraud* (reported in Specifications 1 and 2) is based on expert opinion on a wider range of compromised ethics, not just misstated financials. A one standard deviation higher AM Membership is associated with a 56 percentage point increase in ethical concerns by analysts. When we use the dummy variable ($Dummy(AM > 0)$) as our explanatory variable, firms with positive AM membership are 51% more likely to have ethical concerns by analysts when compared

[10] We follow Dechow et al. (2011) and estimate logit specifications. Our results are qualitatively similar using probit and linear probability model specifications. We include year dummies and cluster our standard errors at the year level because the rate of enforcement actions exhibit strong heterogeneity through time.

[11] An earlier version of the paper reported results when cubic polynomials in log employees and log assets were used as controls. These results are available from the authors.

[12] In unreported analyses, we also match on market capitalization and value of assets and obtain similar results.

to firms without AM membership. Among other variables, Tobin's Q and tangibility are negatively related to ethical concerns regarding *Bribery and Fraud*, while size (log(employees)) is positively related to analysts concerns.

We observe similar results for *Tax Disputes* (Specifications 3 and 4). A one standard deviation increase in *Active AM Accounts* (positive AM membership) is associated with a 1.9 percentage point (1.66 percentage point) increase in the likelihood of tax disputes (72% (79%) of the unconditional mean). The results for human rights violations are consistent, albeit weaker (Specifications 5 and 6). Companies with higher AM Membership are more likely to be involved with regimes that violate human rights (have conflicts with indigenous people, or have labor disputes), and less likely to have proactive policies that prevent such involvement.[13] A one standard deviation increase in AM membership is associated with a 1.5percentage point increase in ethical concerns regarding human rights violations (39% of the unconditional mean). We do not find a statistically significant effect at conventional levels when we use a dummy variable as the explanatory variable ($Dummy(AM > 0)$).

In Columns 7 and 8, we report results for product quality concerns. Higher AM Membership is related to more concerns (negative values), and fewer strengths (positive values). A one standard deviation increase in the number of AM Accounts ($Dummy(AM > 0)$) is associated with a 63 (67) percentage point increase in concerns regarding product quality.[14]

Panel B of Table 2.4 reports results for the matched sample discussed previously. The results from the matched sample corroborate our initial evidence. Out of five KLD variables, only *Product Quality* is insignificant in the matched sample. The coefficient for *Bribery and Fraud* is 57% larger for firms with positive AM membership than for matched firms with zero-AM membership. This is very close to the economic effect reported in Panel A. Similarly, *Tax Disputes* are three times more likely to occur for a firm with positive AM membership than a comparable firm in the same

[13]This variable has a negative value if the company is involved with the regimes that violate human rights, violate the rights of indigenous people, or are involved in labor disputes and anti-union policies. The variable has a positive value if the company has proactive policies to prevent such involvement and has pro-indigenous and pro-labor-union policies. Unreported results showed that what we observe in Specification 5 is driven mostly by violations rather than strong anti-human rights violations policies.

[14]In unreported results we find that the effects in specifications 7 and 8 are driven mostly by product quality concerns rather than product quality strengths.

industry and with roughly the same size, but without AM membership. Additionally, AM-firms are 69% more likely to be flagged by analysts regarding Human Rights concerns. Finally, AM-firms are 66% more likely to be involved in profit sharing programs.

Finally, we examine the relation between AM membership and the use of tax havens/avoidance. Building on the work of Dyreng and Lindsey (2009), we use the proportion of tax havens among the countries the firm does business with. We assume that it is unlikely that a Compustat-listed firm would have a legitimate reason for the majority of its international business dealings to be tax havens. We use three cutoffs, 50% (i.e., more than half of the countries the firm is doing business in are tax havens), 75%, and 90%. Table 2.5 (cont'd), Panel A, reports the results for use of tax havens. Even in our weakest test (50% cutoff, Columns 1 and 2), the effect of AM membership is both economically and statistically significant. A one standard deviation increase in active AM Accounts leads to a 1.05 percentage point increase in abnormal tax haven use. Similarly, *Dummy(AM>0)* is associated with a 2.6 percentage point increase in the use of tax havens, relative to the unconditional mean. At the 90% cutoff (Columns 5 and 6), the effect is considerably larger. *For Active AM Accounts (Dummy(AM>0))* a one standard deviation increase is associated with a 4.0 (5.3) percentage point increase in the use of tax havens.[15] The presence of institutional investors, greater competition, a higher Tobin's Q, and larger market capitalization are all associated with greater use of tax havens. Family firms use tax havens less extensively.

In Table 2.5 (cont'd) Panel B, we report results on the relationship between AM membership and effective tax rates (defined as tax expense divided by pretax income). Even after controlling for the use of tax havens, the effective tax rate is about 0.16 percentage points lower for high AM companies. A one standard deviation increase in AM membership is associated with a decrease in the total taxes paid by approximately 1.1% of an average firm's total tax bill. The results using a dummy variable as our explanatory variable *Dummy(AM>0)* and reported in specifications 2, 4, and 6, are not statistically significant at the 10% level. However, the results are similar in magnitude to the estimates reported for the continuous measure of *Active AM Accounts*.

[15] For consistency with earlier tables, we report results from logit specifications in Panel A of Table 2.5 (cont'd). We obtain qualitatively similar results for probit and linear probability specifications.

2.4 Internal vs. External CEOs

In this section, we ask the question: do firms make an attempt to transform a culture with low integrity? Prior literature suggests that culture is one the most difficult organizational attributes to change; it outlasts organizational products, services, founders, leadership, and the physical attributes of an organization (Schein, 1992). However, as the firm's business environment changes, its former culture may no longer be appropriate. "When basic survival is threatened in terms of an organization's ultimate mission, there is a very strong external impetus to make a radical change in culture" (Flanagan, 1995). Research in management science has suggested that such a transformation often begins when an organization has a new, strong, leader who understands the need for a major change (Kotter, 1995). This literature also recommends that such firms should hire CEOs from outside the firm—or even outside the industry—if changing the existing culture is a primary goal (Bailey and Helfat, 2003).[16]

Thus, the literature suggests that if a firm wants to change its culture, an effective way to do so is to hire an external CEO. In our context, we ask whether firms with high levels of AM membership attempt to change their culture in this manner. This would be the case if there were no tradeoffs to consider in the attempt to enforce stricter standards of integrity. We exploit CEO changes to examine whether firms with high AM membership are more likely to hire external CEOs, suggesting shareholders and directors want to purge "miscreant" cultures. We acknowledge that a firm's culture is likely difficult to change, and therefore we do not attempt to measure the success or failure of a regime shift. However, a firm's board or shareholders trying to institute a deep cultural shift are more likely to do so by appointing an external CEO rather than by hiring someone who has been a part of the very culture the firm is trying to change.

We use Boardex data from 2003-2013 to identify internal versus external CEO hires. We define internal CEOs as CEOs who were employed at the hiring firm for at least two years before their

[16]Lou Gerstner, the former IBM CEO is an example of an outsider who was brought in to change the corporate culture (and succeeded). Many attempts to replicate this story have failed. For example, Hewlett-Packard's Carly Fiorina, and Procter & Gamble's Durk Jager, are cited as examples of CEOs that tried to change too much, too soon. Research has documented that many outside CEOs have not made meaningful changes at all (Karaevli and Zajac, 2013).

appointment. Table 2.6 presents the results from our analysis. The unconditional probability of hiring internal CEOs in our sample is 0.378. After controlling for time effects, as well as industry and geography fixed effects, the probability of choosing an internal CEO is significantly higher for firms with higher AM membership. Specifically, a one standard deviation increase in the number of AM accounts leads to a 6.5-18.1% increase in the probability that a new CEO appointment comes from within the firm, or between 17.0-48.0% of the unconditional probability.

These results show that boards and shareholders of firms with more AM membership do not exhibit a strong tendency to want change their culture by hiring an external CEO. One possible explanation follows from Fiordelisi and Ricci (2014), who show that companies with more creative cultures are more likely to choose an internal CEO in order to continue their creative success. Furthermore, our evidence suggests that firms' directors and shareholders are content with a culture that supports a relatively high level of AM membership. This is consistent with our hypothesis that there are inherent tradeoffs to engineering a corporate culture. These results are also consistent with those of Parrino (1997).[17]

2.5 Conclusion

We find that individual decisions by employees provide substantial information about their employer. Firms that have a greater number of employees registered on AM are not only more likely to behave more unethically, they are also likely to be more innovative. Overall, our results suggest that the personality traits of employees vary systematically across firms. Firm culture is related to corporate outcomes, and firms and employees tend to have matching personality types.

[17] In unreported tests, we examine whether CEO characteristics can explain our results. In particular, we examine the overconfidence measure in Malmendier and Tate (2005). We construct the backward-looking measure, *Holder 67*, that describes the exercise decision of a CEO in the fifth year prior to expiration. Five years before expiration is the earliest point we can consider, since most options in our sample have a ten-year duration and are fully vested only after year four. Under Malmendier and Tate (2005) assumptions of constant relative risk aversion and diversification, the new exercise threshold in the Hall-Murphy framework is 67%. We set *Holder 67* equal to 1 if a CEO fails to exercise options with five years remaining duration despite a 67% increase in stock price (or more) since the grant date. We find no correlation between *Holder 67* and *Active AM Accounts* (it is 0.026). We do not see any significant changes in the coefficient on our variable of interest in all regressions in our paper after controlling for CEO overconfidence, age, and gender. For consistency we report results from logit specifications in Table 2.6. We obtain qualitatively similar results for probit specifications.

An interesting avenue for further research is understanding whether a causal relation extends to more general settings. Research argues that culture fits the firm's business environment, and employee personalities are selected to fit the culture. Yet, research also argues that the corporate culture is persistent. Thus, rapid changes in the firm's external environment might lead to a culture that is no longer suited to the firm's environment. Are such firms the proverbial "dinosaurs" that cannot adapt to changes in their environment and thus go extinct? Anecdotal evidence suggests that even CEOs find it difficult to change a firm's culture.

CHAPTER 3

PRODUCT COMPETITION AND CORPORATE FRAUD

3.1 Introduction

Trust is a central factor in the proper functioning of financial markets (Greenspan, 2008). In particular, distrust in the accuracy of financial statements stemming from corporate fraud can erode firm value (Karpoff et al., 2008; Dyck et al., 2010), impose negative externalities on other firms (Guiso et al., 2008; Kedia and Philippon, 2009), and influence investors' allocation decisions (Giannetti and Wang, 2016; Gurun et al., 2018). These consequences have obliged auditors, regulators, and researchers to improve their understanding of managers' incentives to commit fraud and the ability of various parties to detect financial reporting manipulations.

A survey of CFOs by Dichev et al. (2013) indicates that comparability between rival firms is an important means for identifying financial reporting abnormalities. Building on their insight, we posit that greater product market overlap can enrich financial statement comparability, thus facilitating monitoring and improving fraud detection. This benchmarking effect could discipline managers' reporting practices, leading them to commit less fraud. Alternatively, intense product market competition stemming from less differentiated products can also pressure managers to distort perceived relative performance through reporting manipulations (see Shleifer, 2004; Tirole, 2010) (see Shleifer, 2004; Tirole, 2010). Until recently, only coarse industry-level measures of competition have been available to researchers, which creates challenges in identifying the potential effect of these opposing forces. As a result, the relationship between corporate fraud and product market interactions remains underexplored.

In this paper, we move beyond traditional measures of competition to shed light on whether product market differentiation imposes discipline or increases pressure, on average, as it pertains to corporate fraud. To measure corporate fraud, we use the combination of settled Accounting and Auditing Enforcement Releases (AAER) and settled Securities Class Action Clearinghouse

(SCAC) financial lawsuits.[1] To measure product differentiation, we use pairwise product market similarity scores developed by Hoberg and Phillips (2010, 2016) that are based on the product descriptions in firms' annual financial reports. Whereas most industry classifications are binary, product similarity scores capture the degree of product market overlap between firms. Furthermore, these similarity scores improve on the accuracy of other competitor classification schemes and allow for firm-specific and time-varying competitor networks. These features enable us to move beyond identifying associations between industry-level characteristics and fraud by conducting more powerful tests using firm-level measures of product market differentiation.

Our analyses reveal a strong relationship between product market differentiation and corporate fraud. We find that the incidence of fraud is significantly lower for firms with a less differentiated product mix. Specifically, a one standard deviation higher average product similarity score for a firm is associated with a 14.8-23.7% decrease in the rate of SEC enforcement actions, relative to the unconditional sample average. This result is robust to the inclusion of control variables that have a documented relation to corporate fraud, such as measures of firm size, accounting quality, internal and external corporate governance, the number of distinct markets in which a firm operates, industry and industry-year fixed effects, and various levels of clustered standard errors. Our findings suggest that the effect of product market differentiation on the incidence of fraud has a relatively large economic effect when compared to virtually any predictor of corporate fraud previously explored in the literature.

To explore whether the apparent disciplining effect of low product differentiation stems from a benchmarking channel, we study how firm complexity impacts the relationship between product market differentiation and fraud. On the margin, it is likely more difficult to detect abnormal behavior for complex firms operating in many segments than for firms with a simple organizational structure (e.g. Cohen and Lou, 2012).[2] We proxy for complexity using the number of unique SIC codes that a firm's product mix spans, and split firms into quartiles based on this proxy. Consistent

[1]Donelson et al. (2017) suggest the combination of AAER and class action lawsuits provide the most accurate and complete measure of corporate fraud that is currently available.

[2]Cohen and Lou (2012) find that financial markets incorporate information at a slwer pace for complicated firms.

with the benchmarking channel, we find that the disciplining effect of product similarity increases monotonically across complexity quartiles, after controlling for firm size. Indeed, the coefficient estimate is more than four times as large for the most complex quartile when compared to the least complex quartile. This finding suggests that having less differentiated rivals generates a larger marginal impact on the ability to detect corporate fraud for complex firms.

To further explore the benchmarking channel, we exploit an additional feature of IPO and M&A activity. Specifically, we utilize IPOs and M&As of a firm's rivals as a shock to the firm's information environment. IPOs increase the publicly available financial information of existing competitors, which in turn enhances the ability to assess, compare, and scrutinize related firms' own financial statements (e.g. Bauguess et al., 2018). Similarly, M&A activity generates attention in the merging firms' industries due to potential spillover effects on rivals, customers, and suppliers (e.g. Fee and Thomas, 2004), and due to potential ensuing acquisition activity (Song and Walkling, 2000). We find that higher IPO and M&A activity of rival firms is associated with a higher incidence of detected fraud. Further, this increase in detection is significantly more pronounced for firms with less similar rivals, prior to the IPO (i.e., the effect is greater for ex ante less disciplined firms). This finding suggests these events enhance monitors' effectiveness in detecting fraud. In particular, IPOs or M&As by rivals increase the saliency of publicly available information in a firm's industry before a firm has time to fully wind down misreporting. A potential concern is that IPOs and M&A activity likely impact the competitive nature of the industry by injecting capital into a newly public firm or by consolidating market power in existing competitors. While we cannot entirely rule out the competition effect, we condition on the amount of capital raised in an IPO (size of M&A deal) to help isolate the effect of information saliency on fraud detection.

Product market differentiation likely reflects aspects of the competitive landscape, in addition to the amount of comparable information provided by rivals. To isolate the benchmarking aspect of product market similarity, we control for a variety of measures meant to capture various aspects of competitive intensity and market power. These measures include industry-wide profit margin, sales concentration, top-4 firm market share, the number of competitors a firm has, and product market fluidity. We find that including these measures of competition as control variables does not significantly influence the coefficient estimates for product market differentiation. Furthermore, we fail to find a robust statistical relationship between these alternative competition measures and fraud when examined separately. These results suggest that product market differentiation plays an important and unique role in determining fraudulent behavior that is not influenced by controlling for other characteristics of competition.

In summary, we find a strong and robust link between product market differentiation and corporate fraud. Indeed, our estimates suggest that product market similarity has an economically larger effect on fraud than any factor, other than firm size, previously documented in the literature. Our initial results suggest that product market similarity imposes a strong disciplining effect on financial reporting misconduct. Further, while none of our follow-up analyses provides incontrovertible evidence in isolation, the preponderance of evidence suggests that the disciplining effect stems through a benchmarking channel. These results indicate that market-based mechanisms play an important role in both the incentive to commit fraud and the ability of external parties to uncover fraudulent activities.

Our paper relates to the literature examining the effect of various measures of competition on managerial discipline. On one hand, competition can diminish conflicts of interest by incentivizing managerial effort (Nickell, 1996) or by reducing resources available for rent extraction (Ades and Di Tella, 1999; Schmidt, 1997). On the other hand, competition has been argued to pressure managers to distort the perceived performance relative to rivals (Shleifer, 2004; Tirole, 2010; Andergassen, 2016). Until recently, only coarse industry-level measures of competition have been available to researchers, which has introduced challenges in identifying the potential effect of these opposing forces. We shed light on this relationship by exploiting newly developed, firm-level measures of product differentiation that allow us to conduct more powerful tests. Consistent with a disciplining channel of competition, we document that product market similarity is strongly associated with a lower incidence of fraud.

In addition, our work suggests that benchmarking is an important factor to consider in studying competition, as it enhances information, and therefore facilitates monitoring ability. In turn, this benchmarking channel can influence managerial behavior. More specifically, as predicted by Holmstrom (1982) and Nalebuff and Stiglitz (1983), to the extent that firms are similarly impacted by the competitive landscape, more direct competition increases information about the firms that could help reduce moral hazard problems. Indeed, empirical work such as Hsu et al. (2017) indicates that a firm's competitive landscape is an important determinant of analyst coverage and forecast accuracy. We study whether this effect applies to financial fraud. Our results suggest that having similar rivals can facilitate information acquisition, which is consistent with survey evidence by Dichev et al. (2013) on factors that help detect financial misrepresentation. Thus, our work suggests an avenue through which external parties, such as short-sellers and analysts, can obtain information useful in the detection of fraud (Dyck et al., 2010).

Our analysis also complements empirical work by Wang et al. (2010) who show that industry-level information affects fraud detection. Our results indicate that information contained in firm-specific product markets and unique competitor networks leads to substantial within-industry heterogeneity in fraud detection, after controlling for the industry-wide measures of information outlined in Wang et al. (2010). Other work by Balakrishnan and Cohen (2013) investigates the interplay between traditional measures of competition and the industry-level incidence of restatements, rather than misreporting and fraud. Our results also suggest that competition matters, but we focus on a particular dimension of competition (product differentiation) that facilitates exploration of the benchmarking channel. We show that the benchmarking ability brought about by firms with a similar product mix holds after controlling for various measures of industry concentration.

3.2 Data

We follow recent empirical work of Donelson et al. (2017) by defining corporate accounting fraud as, "the intentional, material misstatement of financial statements that causes damages to investors." Donelson et al. (2017) advocate using a combination of public and private enforcement actions through AAER and class action lawsuits to capture financial reporting fraud to mitigate measurement error. While regulatory enforcement is important, other participants, such as the media, industry regulators, and employees, serve as important actors in this arena (Dyck et al., 2010).

We obtain AAER data for the sample period 1996-2010. According to the Center for Financial Reporting and Management, the U.S. Securities and Exchange Commission (SEC) issues AAERs during, or at the conclusion of, an investigation against a company, an auditor, or an officer for alleged accounting or auditing misconduct. The AAER dataset provides information on the nature of the misconduct, the named individuals, and the entities involved, as well as their effect on the financial statements. The misstatement investigations in our sample occur because of bribery, fraud, inflated assets, financial reporting related enforcement actions concerning civil lawsuits brought by the in federal court, and orders concerning the institution and/or settlement of administrative proceedings.

We construct our sample of class action lawsuits following the work of Choi et al. (2009), Griffin et al. (2004), Jayaraman and Milbourn (2009), and Thompson and Sale (2003). We start by downloading all class action lawsuits from the SCAC hosted by Stanford University for 1996 through 2011 and scan each filing to only include 10-b5 class action lawsuits, which eliminates those lawsuits that occur for non-financial reasons.[3]

We define each firm-year as an AAER year, a SCAC year, both, or neither. Our primary independent variable, fraud, is a binary variable equal to one for all firm years in which there is an AAER or SCAC. We exclude firms in the financial and utilities industries and firms headquartered outside the United States. Further, we drop ADRs, firms with assets less than $1M, and firms with missing assets or sales items in Compustat. Our final sample of corporate fraud events includes 935 firm-years that are affected by AAER misstatements in at least one quarterly or annual financial statement from 322 unique firms from 1996 to 2010. In addition, our sample includes 311 class action lawsuits affecting 299 firms from 1996 to 2011. In total, our sample contains 498 firms and

[3]Karpoff et al. (2017) note the importance of additional checks of the sources to ensure that they contain instances of fraud.

1,217 firm-years, flagged as years with fraudulent reporting. These figures are very closely in line with those of (Dyck et al., 2010). As shown in Table 3.2, the overall incidence of fraud in our sample is 1.9%.

To construct our set of control variables, we follow work in the finance and accounting literatures related to corporate fraud. We include predictors of accounting misstatements from Dechow et al. (2011). The variable RSST accruals measures the change in non-cash net operating assets, including both working capital accruals and long-term operating capital. Bergstresser and Philippon (2006) show that changes in accounts receivable (ΔAR) and change in inventory (ΔInventory) are associated with incentives to improve sales growth and gross profit margin. A firm's soft assets as a percentage of total assets (% soft assets) is associated with more discretion for earnings management. We define % soft assets as total assets minus property plant and equipment and cash and cash equivalents, all scaled by total assets. Change in cash-based sales (ΔCash Sales) excludes accrual-based sales to measure the portion of sales that are not subject to discretionary accrual management. Change in ROA (ΔROA) controls for changes in earnings growth. The variable ΔEmployees is the percentage change in employees less the percentage change in total assets. This measure is associated with labor costs and must be expensed as incurred. Reducing the number of employees can boost a firm's short-term financial performance by immediately lowering expenses. Finally, we include a dummy variable (d_Security Issue) equal to one for firm years in which a firm issues debt or equity, which can increase incentives to manage earnings Rangan (1998). We refer to specifications including only the controls from Dechow et al. (2011) as the Dechow set of controls.

We also include specifications that contain proxies for monitoring mechanisms and corporate opaqueness, which could potentially influence the marginal impact of our proposed benchmarking channel. We include Institutional Ownership, the natural log of the number of analysts covering a firm's stock (Ln Num Analysts), research and development expenses (R&D), and industry stock return r-squared (Ind R2).[4] To construct the industry r-squared, we follow Wang and Winton

[4]To handle observations with missing R&D, we follow the method outlined in Koh and Reeb (2015) and replace each missing observation with the industry year average and include a dummy variable for whether the firm has missing R&D .

(2014) and first regress each firm's daily stock returns on the weighted-average daily market return and the weighted-average daily industry return. Then, we take the average r-squared for each firm in a given three-digit SIC code. Managers may feel pressured to commit fraud when they require capital from outside sources (Teoh et al., 1998; Wang and Winton, 2014). Thus, we include the Whited and Wu (2006) Index for financial constraints.[5] We include the natural log of total assets (ln assets) as a measure of firm size. We also include the variable Book Leverage, which is defined as long and short-term debt over total assets. Highly levered firms may have greater probabilities of financial distress, which has been shown to predict fraud (e.g. Healy and Wahlen, 1999). Alternatively, debt can have a disciplining effect by either mitigating agency issues between managers and shareholders (Grossman and Hart, 1982), or providing an additional source of external monitoring vis-a-vis debtholders.

Product differentiation is likely related to relative performance evaluation (RPE). Firms with less product market differentiation might naturally have better benchmarks, and therefore, be more prone to RPE, which could pressure some managers to cut corners or misstate earnings to outperform benchmarks (e.g. Cheng, 2011). This effect would work against our hypothesis and findings. Thus, to increase the power of our tests, we control for RPE following the work of Wang and Winton (2014) who construct an indicator variable RPE. First, the authors estimate the following regression equation:

$$prob(CEOTurnover_{i,t-1}) = \gamma_1 RP^{+}_{i,t} + \gamma_2 RP^{-}_{i,t} + \epsilon_{i,t} \tag{3.1}$$

where $RP^{+}_{i,t}$ is equal to relative performance when relative performance is above 0, and zero otherwise; and $RP^{-}_{i,t}$ is equal to relative performance when relative performance is below 0, and 0 otherwise. Relative performance is measured as the difference in performance between firm i and the weighted average of firm i's rivals according to its three-digit SIC code. Following Wang and Winton (2014), we estimate equation (1) separately for each industry (three-digit SIC) and define the binary variable RPE equal one for industries where $\gamma_2 < 0$. We refer to specifications that include all our control variables as the full set of controls.

[5] In unreported analysis, we use an alternative prosy for equity finance needed, which measure a firm's asset growth rate in excess of the maximum internally financeable growth rate. We find qualitatively similar results.

Table 3.2 provides the number of observations, mean, standard deviation, 10th percentile, and 90th percentile value for our control variables. We estimate all specifications for both winsorized and non-winsorized data. Estimates obtained from winsorized data (1% in each tail) are reported.

3.3 Product Differentiation and Alternative Measures of Competition

3.3.1 Product Market Differentiation

For our measure of product differentiation, we use the product similarity score developed by Hoberg and Hoberg and Phillips (2010, 2016), who use textual analytics to capture the relatedness of a firm's product market with all other firms in that file annual reports with the U.S. Securities and Exchange Commission (SEC). The process involves vectorizing the product market vocabulary from the business description from each firm's annual 10-Ks, according to a dictionary the authors develop. They then assign pairwise similarity scores based on the cosine similarity between two firms' vectorized product market descriptions. The cosine similarity is higher when the product market descriptions between the two firms are more similar. The measure ranges from 0 (no similarity) to 1 (perfect similarity). We contend that product market overlap measures relatedness that can be used to assess both financial statements and understand the common factors that affect the performance of related firms.

We also make use of the text-based network industry competitors (TNIC) that Hoberg and Phillips define as a byproduct of their product similarity score. The TNIC competitor set includes all firms with a similarity score above a given threshold. Importantly, TNIC allows the flexibility for each firm to have its own distinct set of competitors. For example, Nike competes with Callaway in golf, and competes with Head in tennis, but Callaway and Head are not direct competitors with each other. This intransitive feature better reflects economic reality, and it allows us to exploit granularity in the data that is not possible using measures created from standard transitive industry classifications, such as SIC codes.

Furthermore, these industry classifications are updated annually, which provides more flexibility and accuracy in empirical design. For example, when Exxon sold its retail gas stations in 2008, this event was reflected by the change in its competitor set (TNIC) and average product similarity score (from 0.035 to 0.012). However, the divestment from Exon was not reflected by a change in its SIC code or other industry classifications. As a result, the level of competition that Exxon faced according to SIC code-based Hirschman-Herfindahl Index (HHI) measures did not change in response to its large divestment. These features allow us to conduct more powerful tests than transitive and time-invariant industry classifications would allow.

Using the TNIC competitor classification and product similarity scores, we create our main variable of interest; Average Similarity Score, as the average pairwise similarity score of all competitors within a firm's TNIC-3 classification in each year. As shown in Table 3.2, the firms in our sample have 49 competitors on average, with an Average Similarity Score of 0.03 above the threshold set by Hoberg and Phillips (2016).

There are potential issues when using the Average Similarity Score based on the TNIC classification. First, the TNIC only includes pairs of firms over a certain threshold of similarity. While imposing this threshold allows us to focus on closely related rivals, there can be substantial variation in the number of competitors being averaged across for each firm. The wide variation in both the number of competitors each firm has, and the degree of similarity with each competitor, can obfuscate the association between fraud and product differentiation. Two firms, for example, could have the same average product market similarity scores for different reasons. One firm could have several moderately close rivals while another firm could have some nearly identical rivals and some that are barely related. While both firms could have the same average product similarity score, we would expect the firm with the near identical rivals to have stronger discipline effect through benchmarking.

To address such concerns, we implement a series of alternate methods for aggregating product similarity scores. Rather than averaging across all competitors in a firm's TNIC, we average across the top 5, 10 or 15 closest competitors. This process creates more homogeneity by utilizing the same number of competitors for each firm and focuses on each firm's closest rivals, which should provide the greatest benchmarking externalities. As an alternative approach, we count of the number of competitors each firm has that are in the top percentile (95th, 90th and 75th) of the overall distribution of similarity scores across all firms in the sample. This process allows us to count the number of rivals that each firm has that are very similar relative to the complete cross-section of firms.

In addition to the simple Average Similarity Score and rival counts, we develop a more nuanced measure that helps account for the degree of similarity between rivals. In particular, rivals provide signals about similar firms, with greater similarity between two rivals producing a less noisy signal. It follows that both the similarity with a given rival, as well as the number of rivals, impact the total signal provided by a firm's product market competitors. If signal noise is normally distributed, then there is an inverse squared relationship between product market similarity and the quality of the signal. We define a measure of precision as:

$$precision_{i,t} = \left(\frac{1}{N_i}\sum_{j=1}^{N_i}\frac{1}{(1-score_{i,j,t})^2}\right)^{0.5} \tag{3.2}$$

where N_i is the number of competitors in firm i's TNIC, and $score_{i,j,t}$ is the product similarity score between firm i and competitor j in year t.[6] Higher precision is indicative of a greater signal provided by a firm's product market rivals.

3.3.2 Alternative Measures of Competition

The similarity measures based on product market differentiation should capture the extent to which a firm's rivals provide suitable benchmarking of performance, thus facilitating the detection of fraud. However, the degree of product market differentiation also reflects the notion that competition is an endogenous outcome of market forces and that firms choose to differentiate to the greatest degree possible (e.g. Tirole, 1988). Therefore, less differentiation suggests more intense competition, all else equal. To isolate the benchmarking effect, we control for commonly used measures in the literature that are designed to capture other aspects of competition, including HHI (Herfindahl, 1950), profit margin (Bain, 1951), and the sales concentration ratio of the largest four firms in an

[6]we thank Jerry Hoberg for suggesting this measure

industry (Heflebower, 1957). The HHI based on SIC code is the most extensively used measure of competition in studies related to product market competition. The HHI for industry j is calculated as:

$$HHI_j = \sum_{i=1}^{N_j} ((MS_i)^2 \tag{3.3}$$

Where MS_i is the sales-based markets share of firm i in industry j, and N_j is the number of firms in industry j. HHI has a maximum value of 1 that is attained if a single firm makes up an entire industry, and a minimum value of $1/N_j$ if each firm has equal weight in industry j.

As additional measures of competition, we include the number of competitors according to a firm's TNIC or three-digit SIC codes. Classic models of competition suggest that the more firms there are offering similar products, the competition would be more intense (Tirole, 1988). We also include profit margin (Bain, 1951) and the sales concentration ratio of the top four firms in an industry (Heflebower, 1957) as an additional measure of market power at the industry level. Lastly, we include product market fluidity, which captures competitive pressure from potential entrants that captures each firm's ex ante competitive threats (Hoberg et al., 2014). This measure also uses textual analytics and compares the use of unique words in each firm's product descriptions to changes in the overall use of a given word by other firms in their product descriptions. This measure lies between zero and one and is higher the more the words used a firm's product description overlap with the changes in the word changes by competitor firms. The intent is to capture threats based on the actions by rival firms, rather than changes of the firm itself.

3.4 Empirical Results

In this section, we discuss results from firm-level regressions that examine the association between corporate fraud and product market differentiation. We first explore associations in a standard panel data framework before exploring an instrumental variables approach. We then move on to discuss empirical tests that are aimed to highlight the particular channel that could explain the findings of our exploratory regressions.

3.4.1 Product Market Differentiation and Corporate Fraud

We report OLS estimates for the association between average product similarity score (Average Similarity Score) and corporate fraud in Table 3.3.[7] The firm-year is the unit of observation in all reported specifications in this section. The specification in Column 1 only includes year fixed effects. Column 2 includes the Dechow set of controls (i.e. accruals, change in accounts receivable (ΔAR), change in Inventory (ΔInventory), the percentage of soft assets (% Soft Assets), change in cash sales (ΔCash Sales), change in ROA (ΔROA), change in employees (ΔEmployees), and a dummy for security issuance (d_Security Issue).

In Column 3, we also include the natural log of total assets (Ln Assets), Book Leverage, Industry Stock Return R-squared, the Whited-Wu Index, a flag for relative performance evaluation (RPE flag), R&D, an R&D flag, and Institutional Ownership. Including Institutional Ownership results in a large drop in the number of observations and does not appear to have a meaningful effect on the detection of fraud. Furthermore, inclusion of Institutional Ownership only seems to intensify the relationship between fraud and Average Similarity Score. Considering these issues, we drop Institutional Ownership from the remaining specifications and use the specification from Column 4 as our primary specification throughout the remainder of our analysis. Henceforth, we refer to the specification of control variables in Column 4 as our Full set of control variables. All explanatory variables are lagged by one year.

The granularity of our data enables us to control for unobserved heterogeneity at the industry and industry-year level. The specification in Column 4 includes industry (three-digit SIC code) and year fixed effects, and the specification in Column 5 includes industry-year fixed effects. The inclusion of fixed effects improves upon existing studies that are typically unable to account for unobserved industry heterogeneity because the variables they deploy are often constructed at the industry level. In particular, inclusion of industry or industry-year fixed effects accounts for pervasive differences in the propensity to commit fraud across industries and helps to mitigate the effects of large industry shocks explained by factors not controlled for in our initial specifications.

[7] In untabulated analysis, we estimate this relationship with probit and logit specifications and find similar results.

The t-statistics are calculated from standard errors clustered by three-digit SIC code.[8]

Throughout all specifications, the coefficient estimate for Average Similarity Score exhibits a very consistent, economically meaningful, and statistically significant, mitigating effect on fraud. A one standard deviation increase in Average Similarity Score (0.023) is associated with a roughly 0.48 percentage point decline in the rate of fraud. That is, a one standard deviation increase in average product similarity score is associated with a decline in the rate of fraud from 1.8% to 1.3%. Thus, the results suggest that product similarity has a large economic effect. Indeed, firm size is the only predictor of fraud that we have found documented in the literature to have a larger economic relation to fraud than Average Similarity Score. These results are robust to several different sample periods (i.e., before and after Sarbanes Oxley) and to the inclusion of controls that proxy for external monitoring, such as the number of analysts and the degree of institutional ownership.[9]

3.4.2 Firm Complexity and Product Differentiation

Thus far, we have documented a strong mitigating effect of product market similarity (lack of differentiation) on corporate fraud. In this section, we aim to further establish that the disciplining effect of product market similarity on earnings manipulation acts through a benchmarking channel. Tirole (2010) claims that relative performance evaluation (benchmarking) plays an important role in corporate governance because the performance of rival firms is partly governed by common shocks to production cost and demand.

To explore the benchmarking channel, we study the disciplining effects of product market similarity and a measure of firm complexity. Cohen and Lou (2012) argue "complicated firms require more complicated analysis to impound the same piece of information into the price of a firm with multiple operating segments." It stands to reason that regulators, media, and employees can more easily disseminate information for firms with a simple organizational structure, and are therefore, more likely to detect "abnormal" performance or financial reporting. Thus, for firms

[8] Our results are robust to clustering at broader industry classifications (e.g., two-digit SIC) and at the firm level.

[9] We also control for financial statement comparability developed by Franco et al. (2011) and we obtain similar results.

with a very simple organizational structure and product mix, the information provided by having similar rivals (benchmarks) would have a lower marginal effect on outsiders' monitoring ability. In contrast, complex firms can be very difficult to understand and detect "abnormal" behavior without a clear benchmark. Thus, having close rivals for complex firms should intuitively provide a larger marginal effect on the ability to detect earnings manipulations.

All else equal, a firm that operates in several product markets has greater scope to conceal financial information. Operating across a multitude of product markets reduces substantive analytic procedures that auditors can perform and will require more subjective and detailed testing. This notion is reflected in the higher audit fees for firms with many segments (Brinn et al., 1994). For example, a firm that competes in pharmaceuticals, manufacturing, and consumer durables, could hide information by shifting resources across segments or using complex transactions. Furthermore, monitors would need to understand all three industries to confidently detect reporting abnormalities.

As such, we define complexity as the unique number of industries (three-digit SIC codes) in which a firm operates each year. To calculate this value, we sum the number of distinct industries spanned by a firm's TNIC-based competitor set. For example, if a firm has three rivals that each operate in a different three-digit SIC code, then we consider that firm to be operating in three distinct markets. A higher score on complexity indicates that a firm operates in an environment where rivals are from many different industries, and thus the firm is likely more diversified and has a complex basket of products that compete across several markets. Our measure of complexity builds on the intuition provided by Cohen and Lou (2012) who measure complexity as whether a firm operates in multiple markets.

We split our sample into quartiles according complexity rankings. Then, we estimate our main specification for the relationship between corporate fraud and product market similarity separately for each quartile. The results are presented in Table 3.4. In Panel A, we report the average number of unique SIC codes and the number of competitors in each firm's TNIC. Each specification is estimated using our full set of control variables, described in Section II and in our analysis of Table 3.3. We estimate regressions separately for each complexity quartile without Institutional

Ownership (Panel B) and with Institutional Ownership (Panel C).

Consistent with the benchmarking channel, we find that the disciplining effect of product similarity increases monotonically across complexity quartiles for Panel B. The partial effect for the top quartile is more than four times as large as that for the lowest quartile. To put this finding into perspective, a one standard deviation increase in Average Similarity Score for the least complex firms leads to a decrease in propensity of fraud from 1.9% to approximately 1.6%, or a 0.3 percentage point decline. By comparison, a one standard deviation change in Average Similarity Score for the most complex firms decreases the propensity of fraud from 1.9% to 0.65%, or a 1.25 percentage point decline. This result is robust to several variations of controls for firm size. While large firms are more complex than small firms, on average, our sorts capture product market complexity beyond firm size.

In Panel C, we include a control for Institutional Ownership because institutions can serve as monitors (Hartzell and Starks, 2003). We split this specification into a separate panel because the number of observations is reduced substantially. The coefficient estimates continue to exhibit a strong monotonic relationship across complexity quartiles, and therefore, we omit institutional ownership form other specifications to maintain the larger sample size. Again, the effect of product market similarity in the top quartile is almost four times as large as the effect in the bottom quartile.

3.4.3 Change to Information Environment Associated with Rival IPOs and M&A

To further investigate the benchmarking channel, we exploit an additional feature of IPO and M&A activity by a firm's rivals. In particular, both events plausibly lead to a shock to the firm's information environment. We first study the event of IPOs by a firm's rivals. These events increase the publicly-available financial information of previously existing, private competitors, which in turn, enhances the ability to assess, compare, and scrutinize a firm's own financial statements. Consistent with this view, Bauguess et al. (2018) provide evidence that IPOs lead to in an increase EDGAR traffic for rival firms that are already publicly traded. Next, we study acquisitions by a firm's rivals. Acquisitions are material events that can draw considerable scrutiny from investors, analysts, regulators and the media, thus increasing the saliency of existing information in the industry. For instance, acquisitions often occur in waves, suggesting an increase in attention for other firms that could potentially be involved in a deal (Song and Walkling, 2000).

For the IPO tests, we take each pairwise observation of competitors, i and j, and flag whether firm j underwent an IPO in year t. We then aggregate the data to the firm-year level for firm i, counting the number of rivals that underwent an IPO in year t. For robustness we also define a dummy variable (Competitor IPO) equal to 1 if any of a firm's rivals underwent an IPO in year t, and 0 otherwise. There are 3.2 rival IPOs per firm-year in our sample, with a median of 0 (43% of firms have at least one rival IPO). Contingent on having at least one IPO rival, each firm has an average 7.6 rivals undertaking IPOs, which is consistent with the documented evidence that IPOs occur in waves (e.g. Lowry and Schwert, 2002).

A competitor's IPO is a shock to competition via two channels. First, as discussed, more information about economic conditions becomes publicly available for the rival, as well as increased attention, which enhances monitoring abilities for a firm's own investors, regulators, and auditors. Second, the IPO provides a capital injection for a firm's competitor, thus enhancing the intensity of competition with that rival. We control for the amount of funds raised by the competitor during the IPO to help separate the shock to a firm's information environment from the influence of changes in competitors' capital structure and size. While this solution is imperfect, it assists in isolating the effect of rival IPOs due to information rather than the intensity of competition.

We report OLS estimates for the relationship between fraud and rival firm IPOs in Panel A of Table 3.5. More specifically, we estimate the interaction between the natural log of the number of rival firms undergoing IPOs and a firm's product market similarity (Average Similarity Score) prior to the rivals' IPOs. A firm's pre-existing product market similarity is indicative of the level of market discipline provided by competitors prior to the rival's IPO. The coefficients on the level terms suggest that rival IPOs are positively related to fraud detection and Average Similarity Score continues to exhibit a negative relation to fraud propensity. The positive effect of rival firm IPOs on fraud suggests a shock to detection. In particular, IPOs by rivals change the available information for comparison rather abruptly, before a firm has time to fully wind down financial misconduct.

The coefficient estimate for the interaction term is negative (lower for firms with greater pre-existing product market similarity). This finding suggests that the increased detection resulting from rival IPOs is significantly more pronounced for firms with more product differentiation prior to the rival's IPO (i.e., the effect is more pronounced for ex ante undisciplined firms).[10] We also split the sample between pre-IPO year high and low Average Similarity Score firms to verify that the IPO-detection effect is greater for firms that had lower ex ante discipline due to fewer related firms before the IPO year. Due to a lack of power in the high pre-IPO regression these coefficients are not significantly different from each other. We find that these effects hold after conditioning on the amount of capital raised by rivals during the IPOs. Overall, the findings in this section are consistent with rival IPOs having a greater detection effect for firms with lower pre-existing product market discipline.

Next, for the M&A tests, for each pairwise observation of competitors, i and j, we flag whether firm j was acquired in year t. We then aggregate the data back to the firm-year level for firm i, and take the log of the number of acquired firms competing with firm i that were acquired in year t. For the average firm in our sample, there are 0.059 competitor acquisitions of rival firms each year. Conditional on at least one competitor being acquired, the average increases to 1.09 competitor acquisitions per year.

Panel B of Table 3.5 reports the results for rival firms being acquired. Much like the IPO results, we find a negative coefficient on average similarity and a positive coefficient for the number of competitors being acquired. The interaction term is also negative, indicating that the partial effect of firms that had higher score is lower when rivals are being acquired. Thus, firms that are ex ante less disciplined exhibit the greatest response to the information/interest generated around takeovers. The split by firms' pre-existing similarity scores, highlights that the effect of the acquisitions exists predominantly in the subsample with less similar product market rivals (i.e. those less disciplined ex ante).

[10] We repeat this test excluding the rival undergoing IPO from a firm's *Average Similarity Score* calculation to ensure that the similarity with the IPO firm is not driving the results.

3.4.4 Alternative Measures of Competition, Product Similarity, and Fraud

In this section, we show that product differentiation captures a particular dimension of competition not explained by traditionally used measures. Traditional competition measures include the Herfindahl-Hirschman Index (HHI) developed in Herfindahl (1950), profit margin Bain (1951), the sales concentration ratio of the top four firms in an industry Heflebower (1957), and the number of competitors. We also use the newer measure, product market fluidity, which captures the intensity of product market changes for a given firm each year (see Hoberg et al., 2014). All measures of competition are discussed in detail in Section III.

In each Column of Table 3.6, we estimate a specification that includes our full set of control variables, described in Section II. In Column 1, we control for the sales based HHI according to a firm's primary three-digit SIC code. This measure of competition is among the most widely used in academic research. In Column 2, we also control for the number of competitors that each firm has according to its primary three-digit SIC code. Classic models of competition, in which more firms offering the same product results in more competition, motivate our inclusion of the number of competitors. Additionally, in Column 3 we include profit margin (Bain, 1951) and the sales concentration ratio of the top four firms in an industry (Heflebower, 1957) to account for market power at the industry level. The effect of Average Similarity Score on fraud remains consistent in both significance and magnitude across Columns 1-3 of Table 3.3.

In Columns 1-3, sales-based HHI using three-digit SIC codes does not appear to have a meaningful relationship with Fraud. In Column 4, we include a sales-based Herfindahl-Hirschman Index (HHI) calculated from a firm's TNIC, rather than primary SIC code. This specification allows us to explore whether the apparent lack of power exhibited by the HHI in relation to corporate fraud is driven by the use of SIC codes to define competitor networks, or by the lack of a strong relationship between market concentration and fraud. In Column 5, we also control for the natural log of the number of competitors each firm has according to its TNIC. Finally, in Column 6 we include product market fluidity and sum similarity from Hoberg et al. (2014). Again, the relationship between Average Similarity Score on fraud remains consistent in both significance and magnitude in Columns 4-6.The key takeaway from this analysis is that the alternative measures do not appear to affect the association between fraud and product differentiation. We explore several combinations of control variables and different sample periods and find that these results are not sensitive to model specification.

In Table 3.7, we report estimates for the relationship between corporate fraud and traditional measures of competition, excluding Average Similarity Score. We perform this exercise to ascertain whether the traditional measures have an association in the absence of product differentiation, which could be capturing some of the variation of these traditional measures. For each measure of competition, we include our full set of control variables from Table 3.3 (described in section II).[11] We estimate the specifications in Table 3.7 without the inclusion of industry fixed effects to provide the best chance at highlighting a statistical relationship. The number of rivals in a firm's primary three-digit SIC industry (Ln NCOMP SIC3) is the only competition variable that exhibits a statistically significant relation to fraud (10% level) in Table 3.7. Interestingly, Top 4 Concentration no longer exhibits a relation to fraud when Average Similarity Score is not included in the same regression. In untabulated results, we find that including industry (SIC3) fixed effects attenuates the point estimates in Table 3.7 even further. This result highlights one potential reason for a lack of strong evidence between product market characteristics and corporate fraud documented in the literature.

At a minimum, the results reported in Tables 3.6 and 3.7 suggest that product similarity captures a dimension of competition unrelated to these alternative empirical measures. As these alternative measures are all designed to capture the degree of competition in an industry in various ways, our results suggest that there is something unique and particularly important about the relationship between fraud and the degree of similarity with rivals. While we cannot perfectly rule out that product similarity is merely capturing competition more accurately than these alternative measures, these results add confidence to the benchmarking channel highlighted throughout our analyses.

[11] We also estimate specifications without control variables and report results. The results are substantively very similar. Note, we include firm size as a control in all specifications since size is strongly related to measures of competition and is a strong predictor of fraud (see Buzby, 1975; Reynolds and Francis, 2000; Graham et al., 2005)

3.5 Conclusion

Our paper empirically examines the relationship between product market differentiation and the incidence of corporate fraud. Having rivals with significant product market overlap can have two potential effects on firms' incentives to commit fraud. On one hand, less product market differentiation could facilitate the ability to evaluate common shocks faced by firms, enhancing monitoring by external parties such as regulators, auditors, and investors. In turn, enhanced monitoring should increase the likelihood that committed fraud would be detected. Whether this effect would result in more or fewer cases of fraud being observed depends on the extent to which managers respond to enhanced detection rates by committing less fraud. On the other hand, less differentiation could foster more competition, leading firms to commit more fraud to boost their own perceived relative performance.

We also show that events that could affect the information environment of firms are associated with a greater detection effect for firms with lower pre-existing market discipline. We contend that these results are largely due to the ability to benchmark firm performance when there are more similar rivals with publicly available information. These results suggest that aspects of competition faced by a firm have a disciplining effect on the incentive to commit fraud.

Collectively, our paper provides new insight on how a particular aspect of competition, product market differentiation, influences the incentives to commit fraud via the ability to benchmark a firm against similar peers. Thus, our paper highlights the role of one market-based mechanism that can affect commission and detection of corporate fraud. Our results suggest that external parties could focus efforts on examining firms with fewer comparable rivals when looking for fraudulent reporting.

APPENDICES

APPENDIX A

INVESTOR REACTION AND MUTUAL FUND MISCONDUCT

Figure 1.1: Regulatory Disclosure

This figure reports time-series of the total number of regulatory disclosures, and total number of regulatory disclosures due to misconduct related to “Mutual Funds". Regulatory disclosures are from mandatory ADV filing by mutual fund management companies. The regulatory events only include the cases that are initially filed against investment advisers. The “Mutual Funds" related misconduct is identified using the item “Principal Product" reported in Form ADV.

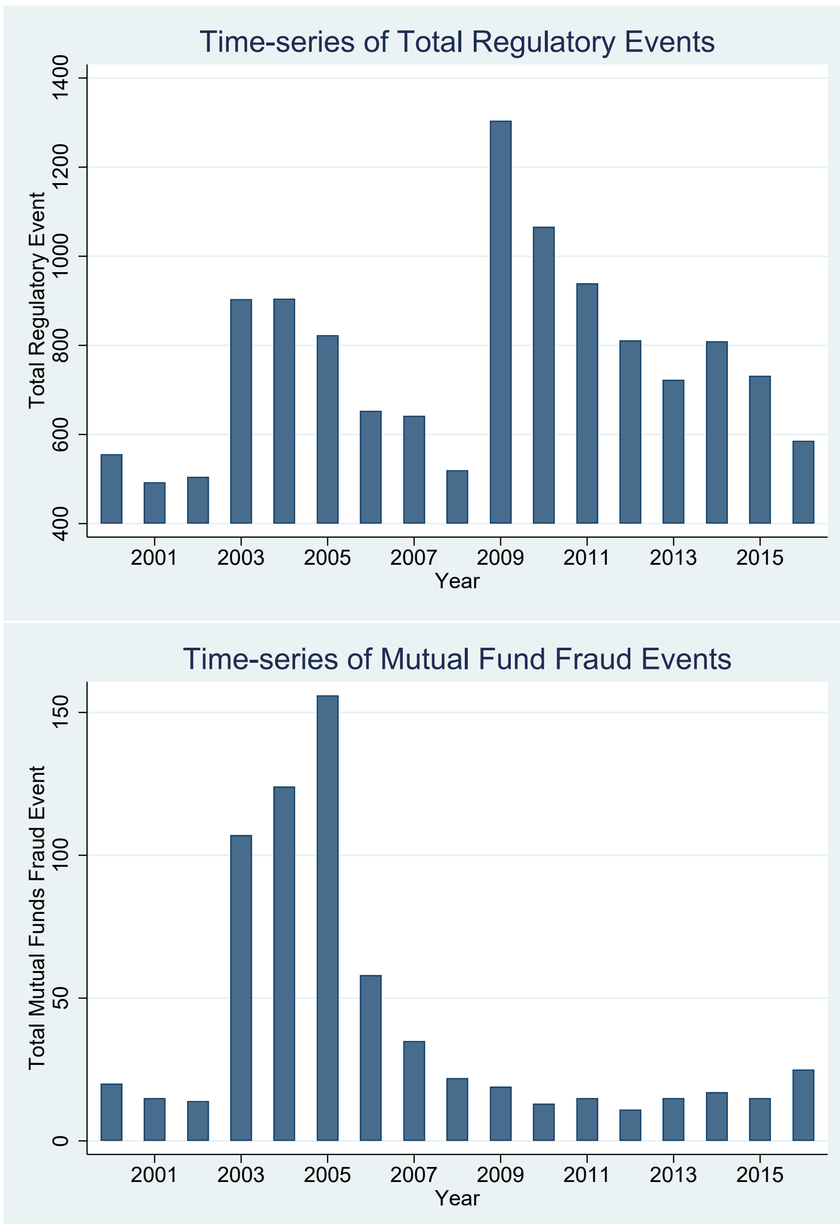

Figure 1.2: Fund Flow Before and After Regulatory Disclosure

This figure reports, at the fund level, the level of investment adjusted net flow (inflow and outflow) six months before and six months after regulatory disclosure. Fund flow data is calculated based on semi-annual N-SAR filings.

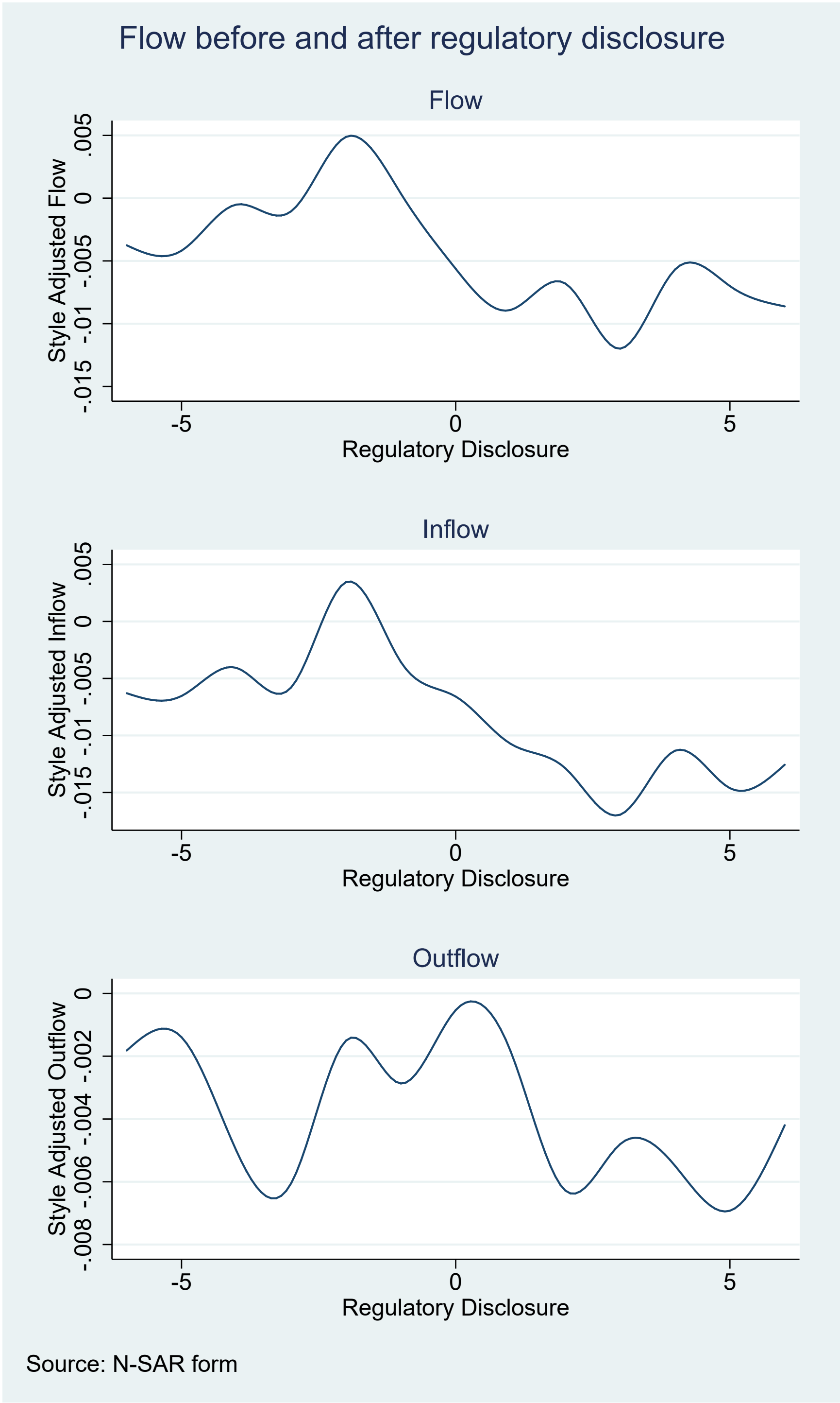

Table 1.1: Variable Description

This table provides a detailed description of all variables used in my analysis. The main variable of interest is *MF Fraud SEC*, defined as an indicator variable that takes value one if there is an SEC-initiated regulatory action taken against mutual fund investment adviser company.

Variable	Definitions
MF Fraud SEC	Indicator variable equals 1 if there is SEC initiated regulatory action taken against mutual fund investment advisor company.
MF Fraud All	Indicator variable equals 1 if there is regulatory action (by any regulator) taken against mutual fund investment advisor company.
Other SEC Regulatory	Indicator variable equals 1 if there is SEC initiated regulatory action taken against investment advisor company related to other products other than "Mutual Funds".
Institutional Funds	Indicator variable equals 1 if it's institutional share class
Retail Funds	Indicator variable equals 1 if it's retail share class.
Enforcement Bank	Indicator variable equals 1 if there is enforcement action taken against fund associated financial conglomerate at quarter t.
Fund Flow	$Flow_{f,t} = \frac{TNA_{f,t} - TNA_{f,t-1} \times (1+R_{f,t})}{TNA_{f,t-1}}$, where $TNA_{f,t}$ is a fund's total net asset at quarter t, and $R_{f,t}$ is the fund's return over the prior month.
Expense Ratio	Expense Ratio as of the most recently completed fiscal year. Represented in decimal format. Ratio of total investment that shareholders pay for the fund's operating expenses, which include 12b-1 fees exp_ratio may include waivers and reimbursements, causing it to appear to be less than the fund management fee.
TNA	Quarterly TNA is equal to total assets minus total liabilities as of quarter-end. Reported in millions of dollars.
Bank Association	Indicator variable equals 1 if there is fund is associated financial conglomerate at quarter t. The association can be established through either of the following ways, (1) fund and financial conglomerate share common name; (2) fund is a registered subsidiary of financial conglomerate.
Direct-sold Fund	Indicator variable equals 1 if fund share class has no front or rear load and the 12b-1 fee is less than 25 basis point.
Broker Fund	Indicator variable equals 1 if fund share class has either front or rear load or the 12b-1 fee is less than 25 basis point.
Age	The number of years since fund was available to investors.
Management Fee	Management fee ($)/ Average Net Assets ($). The fee is calculated using ratios based on the line items reported in the Statement of Operations. The management fee can be offset by fee waivers and/or reimbursements which will make this value differ from the contractual fees found in the prospectus. Reimbursements can lead to negative Management Fees.
Past return	The cumulative return for each fund from t-12 to t-1.
Capital Gains Overhang	Overhang of unrealized gains at the beginning of each month t, $Overhang_t = Overhang_{t-1} + (NAV_t - NAV_{t-1}) \times Shares_{t-1} + (Shares_t - Shares_{t-1}) \times (NAV_t -$ Acg Price Paid for New Shares)
Media	Indicator variable equals 1 if the regulatory disclosure is covered in major business media outlet.
Fraud Severity	Factor variable equals 0 if monetary fine is $ 0; equals 1 if monetary fine is above $ 0 and below $ 8 million dollars; equals 2 if monetary fine is above $8 million dollars.

Table 1.2: Descriptive Summary Statistics

This table reports descriptive summary statistics for primary variables of interest and other controls. These variables include fund flow, regulatory disclosures against investment management companies, regulatory disclosures related to Mutual Fund, regulatory disclosures initiated by SEC, regulatory disclosures against financial conglomerates, as well as the other fund level control variables used in our analysis. Panel A is based on the monthly level data from CRSP Survivorship-bias free database. Panel B presents the summary statistics of data from N-SAR filings. Sample period is from year 2000 to 2016. The number of observations, means, standard deviations, and the 10th and 90th percentiles for each variable are reported in this table. All definitions are provided in detail in Table 1.1.

	Num Obs	Mean	Std.Dev.	10th Percentile	90th Percentile
Panel A: CRSP Sample					
Broker Fund	581,266	0.582	0.493	0.000	1.000
Expense Ratio	558,074	0.011	0.006	0.004	0.019
ADV Regulatory	581,266	0.028	0.164	0.000	0.000
ADV MF Fraud	581,266	0.004	0.059	0.000	0.000
MF Fraud (SEC)	581,266	0.002	0.041	0.000	0.000
Insitutional Fund	581,266	0.314	0.464	0.000	1.000
Ln TNA	579,651	5.315	1.614	3.314	7.519
Past Return	544,183	0.063	0.180	-0.185	0.267
Age	581,266	11.431	9.969	2.170	21.923
Management Fee	562,895	0.588	0.333	0.082	0.989
Flow	569,354	0.003	0.040	-0.029	0.038
Enforement (Bank)	581,266	0.001	0.037	0.000	0.000

	Num Obs	Mean	Std.Dev.	10th Percentile	90th Percentile
Panel B: NSAR Sample					
MF Fraud (SEC)	608,792	0.002	0.041	0.000	0.000
Flow	608,792	0.009	0.085	-0.027	0.047
Inflow	608,792	0.051	0.135	0.000	0.112
Outflow	608,792	0.042	0.173	0.001	0.087
Overhang	590,999	0.016	0.472	-0.180	0.165
Past Return	603,789	0.062	0.127	0.000	0.191
Expense Ratio	608,792	0.008	0.006	0.003	0.015
Ln TNA	608,792	12.861	1.953	10.323	15.164

Table 1.3: Fund Flow and Regulatory Disclosure

In Panel A of this table, we report OLS estimates for fund flows from month t to month t+1 and t+3 after regulatory disclosure of investment adviser misconduct (initiated by SEC), which happens at month t, respectively. Fund inflows and outflows in Panel A are directly obtained from N-SAR filings. Specification (1) and (2)examines the effect on fund net flows, specification (3) and (4) examines fund inflows and specification (5) and (6) examines fund outflows. Our main variable of interest is the *MF Fraud (SEC)* at month t. In panel B, we report OLS estimates for fund flows from month t to month t+1 and t+3 after regulatory disclosure of investment management misconduct at time t. Panel B uses CRSP survivorship-bias free mutual fund dataset. The results are reported separately for institutional share classes and retail share classes. Specification (1) and (2) includes all equity funds, specification (3) and (4) includes only institutional share classes and specification (5) and (6) includes only retail share classes. All specifications include time fixed effects. All other variables are defined in Table 1.1. The t-statistics, calculated from standard errors clustered at the fund management company level, are reported in parentheses below coefficient estimates. Statistical significance (two-sided) at the 10% 5%, and 1% level is denoted by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
	Flow		Inflow		Outflow	
	(t, t+1)	(t, t+3)	(t, t+1)	(t, t+3)	(t, t+1)	(t, t+3)
MF Fraud (SEC)	-0.010***	-0.019***	-0.004*	-0.012*	0.006**	0.009
	(-4.258)	(-3.174)	(-1.794)	(-1.659)	(2.139)	(1.060)
Past Fraud (1-year)	-0.003**	-0.008	-0.001	-0.009*	-0.000	-0.006*
	(-1.974)	(-1.605)	(-1.218)	(-1.878)	(-0.301)	(-1.724)
Past Fraud (5-year)	-0.001	0.001	-0.001	-0.001	-0.001	-0.001
	(-0.609)	(0.156)	(-0.747)	(-0.132)	(-0.751)	(-0.421)
Past Return	-0.000***	-0.001***	-0.000**	-0.001**	-0.000**	-0.001**
	(-3.735)	(-4.550)	(-2.502)	(-2.369)	(-2.011)	(-2.264)
Ln TNA	-0.001***	-0.005***	-0.003***	-0.010***	-0.003***	-0.008***
	(-5.299)	(-6.413)	(-12.117)	(-10.062)	(-11.524)	(-10.474)
Expense Ratio	-0.150**	-0.541***	-0.316***	-1.742***	-0.171**	-0.993***
	(-2.241)	(-2.942)	(-3.543)	(-5.058)	(-2.134)	(-4.299)
Age	-0.006***	-0.013***	0.001	0.005	0.003***	0.006**
	(-6.606)	(-5.260)	(0.788)	(1.228)	(2.728)	(1.964)
Avg Style Flow	0.077	0.177	0.070	0.267	0.030	0.254
	(0.964)	(0.794)	(0.951)	(0.782)	(0.456)	(1.014)
Past Flow	Yes	Yes	Yes	Yes	Yes	Yes
Year Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	574168	523282	628194	587411	628194	587411
Panel B						
	All Equity Funds		Institutional Shares		Retail Shares	
	Flow(t,t+1)	Flow(t,t+3)	Flow(t,t+1)	Flow(t,t+3)	Flow(t,t+1)	Flow(t,t+3)
MF Fraud (SEC)	-0.004***	-0.007**	-0.006*	-0.013**	-0.003**	-0.006**
	(-2.672)	(-2.534)	(-1.977)	(-2.009)	(-2.316)	(-2.262)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	516873	487614	155694	142344	361179	345270

*Difference between flow from institutional shares and retail shares are statistically significant, p-value = 0.075

Table 1.4: Sales Channel and Regulatory Disclosures

In this table we report OLS estimates of flows from t to t+1 and t+3, and change in management fee from month t to month t+12 and t+24 after regulatory disclosure of investment management misconduct at time t, for retail share classes. The dependent variable, $\Delta MgmtFee(t+12)$ ($\Delta MgmtFee(t+24)$), is calculated using $MgmtFee(t+12) - MgmtFee(t)$ ($MgmtFee(t+24) - MgmtFee(t)$). Panel A reports the fund flow following the regulatory disclosures, separately for broker and direct sold funds. Panel B reports the change of management fees following the regulatory disclosures, separately for broker and direct sold funds. All specifications include time fixed effect and investment style fixed effects. All other variables are defined in Table 1.1. The t-statistics, calculated from standard errors clustered at the fund management company level, are reported in parentheses below coefficient estimates. Statistical significance (two-sided) at the 10% 5%, and 1% level is denoted by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)
Panel A				
	Broker		Direct	
	Flow(t,t+1)	Flow(t,t+3)	Flow(t,t+1)	Flow(t,t+3)
MF Fraud (SEC)	-0.003**	-0.006**	-0.002	-0.009
	(-2.272)	(-2.167)	(-0.334)	(-0.921)
Controls	Yes	Yes	Yes	Yes
Year Month FE	Yes	Yes	Yes	Yes
Observations	280864	268899	80315	76371
Panel B				
	Broker		Direct	
	Δ Mgmt Fee (t,t+12)	Δ Mgmt Fee (t,t+24)	Δ Mgmt Fee (t,t+12)	Δ Mgmt Fee (t,t+24)
MF Fraud (SEC)	-0.008**	-0.008*	0.004	0.014
	(-2.372)	(-1.877)	(0.547)	(0.979)
Controls	Yes	Yes	Yes	Yes
Year Month FE	Yes	Yes	Yes	Yes
Observations	219498	193198	58107	51735

Table 1.5: Fund Flow and Regulatory Disclosure - Visibility

In this table we report OLS estimates for fund flow from month t to month t+1 and t+3 after regulatory disclosure of investment management misconduct at time t. Panel A reports separately the funds depend on the size of the fund complex which fund belongs to. Size of fund complex is calculated based on total net assets of all fund share classes within each fund family. Panel B reports separately the flows of fund depend on the past performance of the fund complex which fund belongs to. Star family is calculated following Nanda et al. (2004). All specifications include time fixed effects. All other variables are defined in Table 1.1. The t-statistics, calculated from standard errors clustered at the fund management company level, are reported in parentheses below coefficient estimates. Statistical significance (two-sided) at the 10% 5%, and 1% level is denoted by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)
Panel A:				
Family Size	Small	Large	Small	Large
	Flow(t,t+1)	Flow(t,t+1)	Flow(t,t+3)	Flow(t,t+3)
MF Fraud (SEC)	0.000	-0.004**	0.005	-0.011***
	(0.024)	(-2.488)	(1.056)	(-3.164)
Controls	Yes	Yes	Yes	Yes
Year Month FE	Yes	Yes	Yes	Yes
Observations	177,326	183,485	168,033	176,895
Panel B:				
Family Performance	Non-star	Star	Non-star	Star
	Flow(t,t+1)	Flow(t,t+1)	Flow(t,t+3)	Flow(t,t+3)
MF Fraud (SEC)	-0.002	-0.004*	-0.003	-0.011**
	(-1.609)	(-1.801)	(-1.225)	(-2.143)
Controls	Yes	Yes	Yes	Yes
Year Month FE	Yes	Yes	Yes	Yes
Observations	202,131	158,882	194,006	151,117

Table 1.6: Fund Flow and Regulatory Disclosure – Fraud Severity

In this table, we report OLS estimates for fund flow from month t to month t+1 after regulatory disclosure of investment management misconduct at time t. Specification (1) and (2) report the results for the regulatory disclosure from SROs and other state and federal regulators other than the SEC. Specification (3) reports the fund flows response for repeated offenses. Specification (4) reports the fund flows response for severe fraud charges. Specification (5) reports the fund flows response for mutual fund fraud covered by the major financial media. All specifications include time fixed effects. All other variables are defined in Table 1.1. The t-statistics, calculated from standard errors clustered at the fund management company level, are reported in parentheses below coefficient estimates. Statistical significance (two-sided) at the 10% 5%, and 1% level is denoted by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)
	Flow(t,t+1)	Flow(t,t+1)	Flow(t,t+1)	Flow(t,t+1)	Flow(t,t+1)
MF Fraud SRO	0.000				
	(0.014)				
MF Fraud State		-0.003			
		(-1.584)			
MF Fraud			-0.002*	0.001	-0.000
			(-1.697)	(0.512)	(-0.080)
Repeated Offense			-0.004*		
			(-1.679)		
Severe Fraud				-0.006*	
				(-1.744)	
Media Coverage					-0.003*
					(-1.653)
Controls	Yes	Yes	Yes	Yes	Yes
Year Month FE	Yes	Yes	Yes	Yes	Yes
Observations	361,176	361,176	361,176	361,176	351,516

Table 1.7: Fund Flow and Regulatory Disclosure – Information Quality

In Panel A of this table we report OLS estimates for fund flow from month t to month t+1, and t+3 after SEC enforcement action against the financial conglomerate at time t, separately for institutional share class and retail share class. Our regressor of interest is *Enforcement Bank* at t, and we identify the fund-financial institution association through common name or subsidiary relationship. In Panel B of this table we report OLS estimates for fund flow from month t to t+1, and t+3 after regulatory disclosure at time t that is *unrelated mutual fund product*, separately for institutional share class and retail share class. Our regressor of interest is *Other SEC Regulatory* at t. All specifications include time fixed effects. All other variables are defined in Table 1.1. The t-statistics, calculated from standard errors clustered at the fund management company level, are reported in parentheses below coefficient estimates. Statistical significance (two-sided) at the 10% 5%, and 1% level is denoted by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)
Panel A:				
	Institutional		Retail	
	Flow(t,t+1)	Flow(t,t+3)	Flow(t,t+1)	Flow(t,t+3)
Enforcement Bank	0.007	0.017	-0.004**	-0.010*
	(1.040)	(1.393)	(-2.250)	(-1.748)
Bank Affiliate	-0.004**	-0.010***	-0.000	-0.000
	(-2.530)	(-2.777)	(-0.005)	(-0.115)
Controls	Yes	Yes	Yes	Yes
Year Month FE	Yes	Yes	Yes	Yes
Observations	106692	97651	255141	243570
Panel B:				
	Institutional		Retail	
	Flow(t,t+1)	Flow(t,t+3)	Flow(t,t+1)	Flow(t,t+3)
Other SEC Regulatory	0.003	-0.002	-0.001	-0.004**
	(1.335)	(-0.489)	(-0.841)	(-2.231)
Controls	Yes	Yes	Yes	Yes
Year Month FE	Yes	Yes	Yes	Yes
Observations	155694	142344	361179	345270

Table 1.8: Fund Flow and Regulatory Disclosure - Monitoring Effort

In this table we report OLS estimates for fund flow from month t to month t+1, and t+3 after after regulatory disclosure of investment management misconduct at time t. Panel A reports the fund flows reposes of retail share classes separately for institutional twins funds and others. Institution Twin funds are defined based on the proportion of institutional assets within a fund. Retail funds are identified as having an institutional twin if the proportion of institution assets is above median. Panel B reports the fund flow responses separately for funds depends on the monitoring intensity. Monitoring intensity if calculated by the standard deviation of fund outflows. Panel B restrict the funds from N-SAR sample of funds with very limited institutional clients. All specifications include time fixed effects. All other variables are defined in Table 1.1. The t-statistics, calculated from standard errors clustered at the fund management company level, are reported in parentheses below coefficient estimates. Statistical significance (two-sided) at the 10% 5%, and 1% level is denoted by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)
Panel A:				
	Other	Twin	Other	Twin
	Flow(t,t+1)	Flow(t,t+1)	Flow(t,t+3)	Flow(t,t+3)
MF Fraud SEC	-0.002*	-0.005*	-0.005*	-0.010*
	(-1.889)	(-1.715)	(-1.850)	(-1.724)
Controls	Yes	Yes	Yes	Yes
Year Month FE	Yes	Yes	Yes	Yes
Observations	252,338	108,838	241,400	103,869
Panel B:				
Monitoring	Low	High	Low	High
	Flow(t,t+1)	Flow(t,t+1)	Flow(t,t+3)	Flow(t,t+3)
MF Fraud SEC	-0.0026	-0.0192***	-0.0011	-0.0540***
	(-1.3337)	(-2.9437)	(-0.2466)	(-3.1444)
Controls	Yes	Yes	Yes	Yes
Year Month FE	Yes	Yes	Yes	Yes
Observations	83,885	90,360	76,541	78,527

Table 1.9: Fund flow and Regulatory Disclosure – Investor Mobility

In this table we report OLS estimates for fund flows (inflows, and outflows) from month t to t+1, after regulatory disclosure of investment management misconduct at time t. Panel A reports separately for funds grouped by the capital gains overhang. Fund flow data are directly obtained from N-SAR filings. For each fund each month, capital gains overhang is calculated following the procedure described in Barclay et al. (1998). At the end of each month, we split funds into terciles based on its level of capital gains overhang. Panel B reports separately for sub-sample of *late-trading scandal* period, year 2003 - 2005, and *financial crisis* period, year 2007 - 2009. In unreported analysis, similar results are found for fund flows from t to t+3. All specifications include time fixed effects, and controls used in Table 1.3. All variables are defined in Table 1.1. The t-statistics, calculated from standard errors clustered at the fund management company level, are reported in parentheses below coefficient estimates. Statistical significance (two-sided) at the 10% 5%, and 1% level is denoted by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A:									
		Flow(t,t+1)			Inflow(t,t+1)			Outflow(t,t+1)	
Overhang	Low	Mid	High	Low	Mid	High	Low	Mid	High
MF Fraud (SEC)	-0.009***	-0.012***	-0.012**	-0.002	-0.002	-0.010***	0.008***	0.007*	0.006
	(-2.917)	(-2.896)	(-2.412)	(-0.715)	(-0.784)	(-3.273)	(3.018)	(1.786)	(0.783)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	211042	148502	206174	232151	159151	227170	232151	159151	227170
Panel B:									
		Flow(t,t+1)			Inflow(t,t+1)			Outflow(t,t+1)	
	All	Late-Trading	Financial Crisis	All	Late-Trading	Financial Crisis	All	Late-Trading	Financial Crisis
MF Fraud (SEC)	-0.010***	-0.015***	-0.003	-0.004*	-0.002	-0.011**	0.006**	0.014***	-0.006
	(-4.234)	(-4.199)	(-0.704)	(-1.791)	(-0.659)	(-2.084)	(2.124)	(3.099)	(-1.081)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	574,168	99,102	106,812	628,194	109,275	115,943	628,194	109,275	115,943

Table 1.10: List of Largest Financial Conglomerate in the U.S.

This table reports the list of financial conglomerates used in the analyses. The list includes the top 40 financial institutions in the U.S. based on total assets.

Rank	Name
1	JPMorgan Chase
2	Bank of America
3	Wells Fargo
4	Citigroup
5	Goldman Sachs
6	Morgan Stanley
7	U.S. Bancorp
8	PNC Financial Services
9	Capital One
10	TD Bank, N.A.
11	The Bank of New York Mellon
12	Barclays
13	HSBC Bank USA
14	State Street Corporation
15	Charles Schwab Corporation
16	BB&T
17	Credit Suisse
18	SunTrust Banks
19	Deutsche Bank
20	Ally Financial
21	American Express
22	Citizens Financial Group
23	MUFG Union Bank
24	USAA
25	Fifth Third Bank
26	Royal Bank of Canada
27	UBS
28	Santander Bank
29	KeyCorp
30	BNP Paribas
31	BMO Harris Bank
32	Regions Financial Corporation
33	Northern Trust Corporation
34	M&T Bank
35	Huntington Bancshares
36	Discover Financial
37	Synchrony Financial
38	BBVA Compass
39	First Republic Bank
40	Comerica

Table 1.11: Fund flow before and after Regulatory Event

This table presents t-test of style adjusted fund flows (inflows, outflows), six months before and after regulatory event disclosure. Fund flow data are from N-SAR filings for year 2000 to 2016. The sample only includes equity funds. Net fund flows are calculate by subtracting outflows from inflows. All flow measures are scaled by funds' total net assets from last period. Investment style of a fund is reported directly in N-SAR formS. For each month, style adjusted flows are calculated using the difference between fund flows and average flows of all funds within the same investment style.

	Before	After	Diff	t-stat
Style Adjusted Flow	-0.0012	-0.0079	0.0067	6.225***
Style Adjusted Inflow	-0.0040	-0.0129	0.0089	5.796***
Style Adjusted Outflow	-0.0033	-0.0048	0.0015	1.508

Table 1.12: Fund Flow and Regulatory Disclosure - Sales Loads

In this table, we report OLS estimates for fund inflow and out from t to t+1 after regulatory disclosure about investment management misconduct at time t, separately for broker-sold funds with front load, broker-sold funds with rear load, and direct-sold funds using NSAR data. Our regressor of interest is the MF Fraud SEC at month t. Specification (1) and (4) includes only broker-sold funds with positive front load, specification (2) and (5) includes only broker-sold funds with positive rear load, and specification (3) and (6) includes only direct-sold funds .. All specifications include time fixed effects. All other variables are defined in Table 1.1. The t-statistics, calculated from standard errors clustered at the fund management company level, are reported in parentheses below coefficient estimates. Statistical significance (two-sided) at the 10% 5%, and 1% level is denoted by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Inflow (t,t+1)			Outflow (t,t+1)		
	Frontload	Rearload	Direct	Frontload	Rearload	Direct
MF Fraud SEC	-0.002	-0.001	-0.003	0.013**	0.013**	0.004
	(-0.634)	(-0.292)	(-1.437)	(2.311)	(2.065)	(1.500)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Past Flow	Yes	Yes	Yes	Yes	Yes	Yes
Year Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	124080	84281	437428	124080	84281	437428

APPENDIX B

CORPORATE CULTURE AND CORPORATE FRAUD

Table 2.1: Description of the Variables

This table provides a detailed description of all variables used in our analysis. Our main variable of interest is the AshleyMadison Active Accounts (AM Active Accounts) defined as the natural log of the number of active accounts at the end of year t (plus one).

Panel A - AshleyMadison Variables		
$AMAccounts_{i,t}$	The total number of AM accounts for firm i in year t. An account does not have to have sent a message or purchased credits to be included in this calculation for a given year. That is, the account does not have to be *active*.	AshleyMadison
$ActiveAMAccounts_{i,t}$	The total number of active AM accounts for firm i in year t. An account is required to have sent a message or purchased credits to be included in this measure. If an account is deactivated, then it is excluded from the calculation in a given year, but still included up until the year of its deactivation. This is our main variable of interest throughout the text.	AshleyMadison
Panel B - Firm Financial Information		
$BookLeverage_{i,t}$	Total debt divided by book value of assets. [(dltt+dlc)/at]	Compustat
$Debt/MarketEquity_{i,t}$	Total debt divided by market value of equity. [(dltt + dlc) / (prcc_f*csho)]	Compustat
$R\&D/Sales$	R&D expenditures divided by sales. [xrd/sale]	Compustat
$Tobin'sQ_{i,t}$	Total asset minus book value of equity plus the market value of equity divided by total assets [(at - ceq + me)/at]	Compustat
$MarkettoBookratio_{i,t}$	Market value of firms' equity divided by the book value of equity, following Fama-French calculation of book equity [prcc_f * csho / teq - prefered + txditc]	Compustat
$ROA_{i,t}$	Return on Asset. [oibdp/l.at]	Compustat
$Tangibility_{i,t}$	Net Property, Plant and Equipment divided by total assets [ppent/at]	Compustat
$\#ofEmployee_{i,t}$	The natural log of the total number of employee [log(emp)]	Compustat
$FirmAge_{i,t}$	Firm age reported in Compustat or the number of years firm is observed in Compustat	Compustat
$Cash/Asset_{i,t}$	Cash and short-term investment divided by Assets [(ch + ivst)/at]	Compustat
$LogMarketCap_{i,t}$	Natural log of market cap [log(csho*prcc_f)]	Compustat
$HHI(sic4)_{i,t}$	Herfindahl index based on sales within 4-digit SIC industries in year t	Compustat
$\Delta_{OROA_{i,t}}$	Difference between the average operating income scaled by total assets 3 years before and after New CEO was appointed	Compustat
$\Delta_{OROS_{i,t}}$	Difference between the average operating income scaled by sales 3 years before and after New CEO was appointed	Compustat
WC Accruals	((ΔCurrent Assets - ΔCash and Short-term Investments)- (ΔCurrent Liabilities - ΔDebt in Current *Liabilities* - ΔTaxes Payable) / Average total assets	Compustat
RSST Accruals	(ΔWC + ΔNCO + ΔFIN) /Average total assets, where WC = (Current Assets - Cash and Short-term Investments) - (Current Liabilities - Debt in Current Liabilities); NCO = (Total Assets - Current Assets - Investments and Advances) - (Total Liabilities - Current Liabilities -Long-term Debt); FIN=(Short-term Investments +Long-term Investments) - (Long-term Debt+Debt in Current Liabilities+Preferred Stock)	Compustat
Change in receivables	ΔAccounts Receivable / Average total assets	Compustat
Change in inventory	ΔInventory / Average total assets	Compustat
% Soft assets	(Total Assets - PP&E - Cash and Cash Equivalent) /Total Assets	Compustat
Change in cash sales	Percentage change in cash sales (Sales - ΔAccounts Receivable)	Compustat
Change in cash margin	Percentage change in cash margin, where cash margin is measured as: 1 - ((Cost of Good Sold - ΔInventory + ΔAccounts Payable) / (Sales - Δ Accounts Receivable))	Compustat
Change in ROA	(Earnings $_t$ / Average total assets $_t$) - (Earnings$_{(t-1)}$ / Average total assets$_{(t-1)}$)	Compustat
Actual issuance	A dummy variable takes value to be 1 if the firm issued securities during year t (i.e., A dummy variable takes value to be 1 if "Sale of Common and Preferred Stock" > 0 or "Long-Term Debt - Issuance" > 0)	Compustat

Table 2.1 (cont'd): Description of the Variables

Panel C - Misstatement Variables		
Misstatement	A dummy variable takes value to be 1 if firm is during or at the conclusion of an investigation against a company, an auditor, or an officer for alleged accounting and/or auditing misconduct	AAER
Panel D - Ethics Variables		
Bribery and Fraud	A discrete variable that indicates the severity of controversies related to a firm's business ethics practices, including bribery, and fraud.	KLD
Tax Disputes	A discrete variable that indicates whether companies have recently been involved in major tax disputes involving Federal, state, local or	KLD
Cash/StockSharing	A discrete variable that indicates whether companies have a cash profit-sharing program through which it has recently made distributions to a significant proportion of its workforce. This variable also indicates whether companies encourage worker involvement via generous employee stock ownership plans (ESOPs) or employee stock purchase plans (ESPPs)	KLD
Human Rights	A discrete variable that is the net measure of positive features and negative features regarding human rights for a corporation. Positive features include quality labor rights, a strong relationship with indigenous peoples in foreign operations, and other human rights strengths. Negative features include human rights violations, including freedom of expression and censorship concerns, indigenous peoples relations concerns, labor rights concerns, operations in Sudan, Mexico , Burma, Norther Ireland and South Africa, and other human rights concerns.	KLD
Product Quality	A discrete variable that is the net measure of positive features and negatives features regarding product category. Positive features include insuring health and demographic risk, responsible investment, strong privacy and data security, financial product safety, chemical safety, opportunities in nutrition and health, access to communications, access to capital, benefits to economically disadvantaged, R&D innovation, and other product strengths. Negative features include customer relations concerns, antitrust concerns, marketing-contracting concerns, product safety concerns, and other product concerns.	KLD

Table 2.2: Descriptive Statistics

This table presents summary statistics for AshleyMadison (AM) variables (panel A), as well as the other variables used in our analysis. The AM data cover the sample period 2002-2014. We report the number of observations, means, standard deviations, and the 10th and 90th percentiles for each variable. All definitions are provided in detail in the Internet Appendix.

Variable	Mean	σ	10pct	90pct	N	Variable	Mean	σ	10pct	90pct	N
Panel A: AshleyMadison						Panel D: Governance					
Active AM Accounts						Bribery and Fraud	0.047	0.212	0.000	0.000	3428
all firms	2.052	12.130	0.000	4.000	34961	Human Rights Violations	-0.032	0.262	0.000	0.000	18090
firms $\geq$ 1 account	5.391	19.200	0.000	10.000	13306	Tax Disputes	0.021	0.143	0.000	0.000	11168
Average Years of Activity	0.712	1.105	0.000	2.000	13303	Cash/Stock Sharing	0.144	0.403	0.000	1.000	14095
Average Age of AM User	39.238	7.659	30.000	49.000	13231	Product Quality	-0.018	0.294	0.000	0.000	18096
Average Credits	0.169	6.562	0.000	0.000	11560						
Panel B: Firm and Industry Characteristics						Panel G: AAER data					
Book Leverage	0.227	0.254	0.000	0.577	40712	Misstatement	0.007	0.083	0.000	0.000	40712
Debt/Market Equity	0.645	1.923	0.000	1.281	40712						
Log Market Cap	5.490	2.312	2.420	8.416	40712	Panel F: Patent data					
Tobin's Q	2.575	3.131	0.953	4.631	40712	Ln(R&D)	1.400	1.798	0.000	4.108	40712
Market to Book Ratio	2.719	6.465	0.202	6.665	40712	Patent Cites	0.079	0.307	0.000	0.000	32666
ROA	-0.042	0.593	-0.385	0.259	40712	Patents	0.051	0.259	0.000	0.000	32666
Tangibility	0.233	0.228	0.025	0.604	40712	Pat/R&D	0.006	0.137	0.000	0.000	14121
# of Employee	8.564	38.860	0.035	17.000	40712	Top 10	0.023	1.455	0.000	0.000	32666
Firm Age	17.306	12.266	5.000	37.000	39056	Pdiv	0.043	0.182	0.000	0.000	32666
Log Sales	5.152	2.626	1.694	8.333	40712	Cdiv	0.043	0.183	0.000	0.000	32666
Cash/Asset	0.227	0.235	0.014	0.604	40712	ACdiv	0.043	0.183	0.000	0.000	32666
Volatility-3 Factor adjusted	0.027	0.012	0.013	0.044	34247						
HHI(sic4)	0.122	0.176	0.001	0.337	40712	Panel G: Tax Avoidance					
Stock Return	0.126	0.533	-0.486	0.753	34252	MostlyTxh50	0.296	0.456	0.000	1.000	10779
Skewness	0.368	0.742	-0.372	1.119	33982	MostlyTxh75	0.279	0.449	0.000	1.000	10779
CDS Spread	0.026	0.076	0.003	0.050	4262	MostlyTxh90	0.278	0.448	0.000	1.000	10779

Table 2.3: AAER Misstatements and AshleyMadison Membership

In Panel A, we report coefficient estimates and marginal effects for logistic regressions of accounting misstatements on the number of active AshleyMadison (AM) accounts. Data on misstatements and specifications from 2002-2014 come from the AAER dataset discussed in Dechow, Ge, Larson, and Sloan (2011). This dataset provides detailed information regarding misstatement investigations for public corporations. Specification 1, 3, and 5 reports estimates for the natural logarithm of the number of AM accounts, while Specifications 2,4, and 6 use a dummy variable equal to one if number of AM accounts is greater than or equal to one, and zero otherwise. We include four accruals-related measures. *WC accruals*, focuses on working capital accruals and is described in Allen, Larson, and Sloan (2009). *RSST accruals* are defined in Richardson et. al. (2005) and Dechow et. al. (2011) and extends the definition of WC accruals to include changes in long term operating assets and liabilities. *Change in receivables* (*Change in inventory*) is defined in Dechow et. al. (2011) as the change in accounts receivables (inventory) normalized by average Total Assets. *Soft Assets* is defined in Barton and Simko (2002) as Total Assets minus PP&E minus Cash and Cash Equivalent, all normalized by Total Assets. Performance variables include *Change in cash sales* (defined as the percentage change in sales minus the change in Accounts receivables), *Change in cash margin* (percentage change), and *Change in ROA* (defined as ROA(t) minus ROA(t-1)). *Actual Issuance* is a dummy equal to one if the firm issued securities during year t, and zero otherwise. Specifications (3) and (4) include the *Abnormal change in employees* (defined as the percentage change in the number of employees minus the percentage change in assets), and *Dummy Lease* (defined as one if future operating lease obligations are greater than zero, and zero otherwise). Specifications (5) and (6) also include current and lagged market-adjusted stock return and logarithm of number of employees. The *(Lagged) market-adjusted stock return* is the (previous year) annual buy-and-hold return minus the buy-and-hold CRSP value-weighted index return. All specifications include year and industry fixed effects, and all explanatory variables are lagged by one year. The t-statistics, calculated from standard errors clustered at the year level, are reported in parentheses below coefficient estimates. Statistical significance (two-sided) at the 1% 5%, and 10% level is denoted by *, **, and ***, respectively. Marginal effects are reported below the estimates in square brackets, and are multiplied by 100. In Panel B, we report estimates from a matched-sample for the difference in the number of misstatements between firms with positive AM membership and firms with zero AM accounts. We report the means for both the AM-sample (number of accounts greater than zero) and the matched sample, as well as the difference, the number of matched pairs, the t-statistic, and the p-value. We detail our matching procedure in section 3.1

Table 2.3 (cont'd): AAER Misstatements and AshleyMadison Membership

Panel A: Regression						
	(1)	(2)	(3)	(4)	(5)	(6)
Active AM Account	0.390 ***		0.383 ***		0.249 *	
	[0.658]		[0.656]		[0.447]	
	(4.18)		(4.02)		(1.87)	
Dummy(AM>0)		0.628 ***		0.612 ***		0.469 ***
		[1.06]		[1.048]		[0.842]
		(7.98)		(7.49)		(4.66)
RSST Accruals	0.139 ***	0.139 ***	0.139 ***	0.140 ***	-0.515 *	-0.535 *
	[0.234]	[0.235]	[0.238]	[0.24]	[-0.924]	[-0.959]
	(4.1)	(4.38)	(4.72)	(4.81)	(-1.82)	(-1.85)
Change in receivables	-1.435	-1.420	-1.595	-1.571	-2.365 ***	-2.374 ***
	[-2.422]	[-2.397]	[-2.73]	[-2.69]	[-4.241]	[-4.256]
	(-1.31)	(-1.24)	(-1.4)	(-1.32)	(-3.49)	(-3.35)
Change in inventory	2.156	2.149	2.153	2.142	3.336 ***	3.364 ***
	[3.64]	[3.629]	[3.684]	[3.666]	[5.982]	[6.03]
	(1.47)	(1.46)	(1.45)	(1.44)	(3.1)	(3.23)
% Soft assets	1.931 ***	1.977 ***	1.945 ***	1.990 ***	2.407 ***	2.451 ***
	[3.259]	[3.338]	[3.328]	[3.407]	[4.317]	[4.394]
	(6.65)	(7.13)	(6.66)	(7.09)	(5.7)	(6)
Change in cash sales	0.000 ***	0.000 ***	0.000 ***	0.000 ***	0.000 ***	0.000 ***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
	(-3.96)	(-3.95)	(-3.81)	(-3.82)	(-5.29)	(-5.2)
Change in ROA	-0.010 ***	-0.009 ***	-0.010 ***	-0.010 ***	0.115	0.119
	[-0.016]	[-0.016]	[-0.018]	[-0.017]	[0.207]	[0.213]
	(-2.92)	(-2.79)	(-3.04)	(-2.81)	(0.47)	(0.48)
Change in cash margin	0.000 ***	0.000 ***	0.000 ***	0.000 ***	0.000 ***	0.000 ***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
	(4.52)	(4.31)	(4.1)	(4.08)	(4.92)	(4.84)
Actual issuance	0.802 *	0.790 *	0.817 *	0.808 *	0.720	0.724
	[1.353]	[1.334]	[1.398]	[1.383]	[1.291]	[1.297]
	(1.87)	(1.79)	(1.88)	(1.81)	(1.04)	(1.00)
Abnormal change in employees			-0.071	-0.065	-0.287 *	-0.301 *
			[-0.121]	[-0.111]	[-0.515]	[-0.54]
			(-0.55)	(-0.54)	(-1.84)	(-1.92)
Existence of operating leases			-0.449	-0.466	-2.555 ***	-2.603 ***
			[-0.768]	[-0.797]	[-4.583]	[-4.667]
			(-0.38)	(-0.39)	(-2.79)	(-2.78)
Market adjusted stock return $_t$					0.297	0.286
					[0.532]	[0.513]
					(0.92)	(0.88)
Market adjusted stock return $_{t-1}$					0.306	0.295
					[0.549]	[0.529]
					(1.26)	(1.24)
Log # of employees					0.119 ***	0.127 ***
					[0.213]	[0.227]
					(2.63)	(4.00)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R-Square	0.194	0.194	0.196	0.196	0.219	0.220
Observations	27775	27775	27325	27325	22557	22557

Panel B: Matched Sample						
	Mean (AM)	Mean (Matched)	Δ_{Mean}	Number of Matched Pairs	t-stat	P-Value
Misstatement	1.186	0.678	0.508	4132	1.780	0.075

Table 2.4: KLD Ethics Ratings and AshleyMadison Membership

In this table we report OLS estimates for KLD ratings of firm behavior on the number of active AshleyMadison (AM) accounts. KLD ratings are annual company performance indicators with respect to meeting stakeholder needs regarding environmental, social, and governance factors. The indicators are developed by MSCI analysts who provide research for institutional investors. The KLD data are described in greater detail in section 2.2. As the dependent variable we use the number of positive ratings minus the number of negative ratings within a given KLD category. Our regressor of interest is the natural logarithm of one plus the number of active AM accounts for a given firm year. All regressors are lagged one year relative to our KLD measures. All other variables are defined in the Internet Appendix. The t-statistics, calculated from standard errors clustered at the firm level, are reported in parentheses below coefficient estimates. Statistical significance (two-sided) at the 1% 5%, and 10% level is denoted by *, **, and ***, respectively.

Panel A: Regression

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	Business Ethics	Business Ethics	Tax Disputes	Tax Disputes	Human Rights	Human Rights	Product Quality	Product Quality	Cash/Stock Sharing	Cash/Stock Sharing
Active AM Account	0.0331 ***		0.0190 ***		-0.0155 **		-0.0141 *		0.0303 ***	
	(4.672)		(2.759)		(-2.333)		(-1.919)		(2.594)	
Dummy(AM>0)		0.0241 ***		0.0166 **		-0.00332		-0.0120 **		0.0285 *
		(2.735)		(2.025)		(-0.517)		(-2.082)		(1.798)
Book Leverage	-0.0235	-0.0234	0.00344	0.00247	0.000621	0.000357	-0.00297	-0.00317	-0.0342	-0.0235
	(-1.599)	(-1.577)	(0.264)	(0.191)	(0.0552)	(0.0316)	(-0.196)	(-0.304)	(-1.416)	(-1.058)
Tobin's Q (t-1)	-0.00442 **	-0.00564 **	-0.000185	-0.000238	-0.000766	-0.000469	0.00113	0.00136	-0.00513	-0.00247
	(-2.023)	(-2.524)	(-0.127)	(-0.162)	(-0.498)	(-0.307)	(0.511)	(0.695)	(-1.145)	(-0.588)
EBITDA/Assets (t-1)	-0.00967	-0.0171	-0.01000	-0.0128	0.0110	0.0140	0.0137	0.0162	-0.0480	-0.0427
	(-0.553)	(-0.954)	(-1.117)	(-1.414)	(1.191)	(1.517)	(0.950)	(1.221)	(-1.465)	(-1.453)
Annual Return	-0.0234 **	-0.0235 **	0.00231	0.00302	-0.0109 **	-0.0115 **	0.00360	0.00325	0.0106	0.0102
	(-2.508)	(-2.468)	(0.855)	(1.148)	(-2.029)	(-2.135)	(0.666)	(0.542)	(1.261)	(1.277)
Volatility	1.101	1.320	0.829 *	0.918 **	-0.0782	-0.160	-0.868	-0.948 **	1.632	1.779 *
	(1.255)	(1.500)	(1.777)	(1.990)	(-0.175)	(-0.360)	(-1.448)	(-1.993)	(1.630)	(1.896)
Log # of Employee	0.0262 ***	0.0321 ***	0.00553 *	0.00813 ***	-0.0168 ***	-0.0199 ***	-0.0132 ***	-0.0151 ***	-0.00952	0.000185
	(4.383)	(5.063)	(1.815)	(2.666)	(-3.798)	(-4.257)	(-2.670)	(-4.572)	(-1.201)	(0.0240)
Market Cap (t-1)	0.00514	0.0104 *	0.0144 ***	0.0164 ***	-0.00470	-0.00703	0.00897	0.00705 *	0.0801 ***	0.0772 ***
	(0.834)	(1.721)	(3.855)	(4.305)	(-1.026)	(-1.552)	(1.627)	(1.932)	(7.601)	(7.663)
Tangibility (t-1)	-0.0917 **	-0.0948 ***	0.0173	0.0126	0.0738 ***	0.0777 ***	0.0736 **	0.0759 ***	0.110 **	0.0820 *
	(-2.506)	(-2.587)	(0.681)	(0.504)	(2.966)	(3.128)	(2.127)	(3.579)	(2.304)	(1.844)
EA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry(SIC2) FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,168	8,203	8,203	14,649	14,649	14,655	10,935	10,935	10,935	10,935
R-squared	0.219	0.208	0.202	0.151	0.149	0.140	0.241	0.227	0.241	0.227

Panel B: Matched Sample

	Mean (AM)	Mean (Matched)	Δ_{Mean}	Number of Matched Pairs	t-stat	P-Value
Business Ethics	5.323	3.387	1.935	620	1.71	0.0887
Tax Disputes	3.512	1.171	2.342	1452	2.57	0.0104
Human Rights	-3.952	-2.343	-1.609	2859	-1.73	0.0843
Product Quality	-1.084	-0.874	-0.21	2859	-0.17	0.8659
KLD_sharing	22.969	13.864	9.105	1933	3.67	0.0003

Table 2.5: Tax Policy and AshleyMadison Membership

In Panel A, the dependent variable is a dummy variable equal to one if the proportion of tax havens among countries mentioned in Exhibit 21 of a firm's 10-K filing exceeds 50% (75%, 90%) (see Dyreng and Lindsey (2009) for a detailed description). Countries are identified as tax havens if they are defined as such by three of the four following sources: (1) the Organization for Economic Cooperation and Development (OECD), (2) the U.S. Stop Tax Havens Abuse Act, (3) The International Monetary Fund (IMF), and (4) the Tax Research Organization. We define mostly_txh50 (mostly_txh75, mostly_txh90) as a dummy variable equal to one if more than 50% (75%, 90%) of the countries mentioned in 10-K filings are listed as tax havens, and zero otherwise. All other variables are defined in the Internet Appendix All reported estimates are marginal effects (multiplied by 100) from logit regression specifications. z-statistics, computed from standard errors clustered by year, are reported in parentheses below coefficient estimates. All specifications include industry, year, and EA fixed effects transformations. In Panel B, we report OLS estimates for the dependent variable *Effective Tax Rate*, calculated using income tax divided by pretax income excluding special items. All other variables are defined in the Internet Appendix. t-statistics, computed from standard errors clustered by year, are reported in parentheses below coefficient estimates. All specifications include industry, year, and EA fixed effects transformations. Statistical significance (two-sided) at the 1% 5%, and 10% level is denoted by *, **, and ***, respectively.

Panel A: Use of Tax Havens						
	(1)	(2)	(3)	(4)	(5)	(6)
	MostlyTxh50	MostlyTxh50	MostlyTxh75	MostlyTxh75	MostlyTxh90	MostlyTxh90
Active AM Account	0.0136***		0.0145***		0.0146***	
	(4.062)		(4.470)		(4.444)	
Dummy(AM>0)		0.00757		0.0138***		0.0147***
		(1.575)		(3.346)		(3.494)
Institutional Investor	0.0263*	0.0258*	0.0127	0.0123	0.0121	0.0117
	(1.843)	(1.722)	(1.029)	(1.007)	(0.955)	(0.968)
HHI (SIC4)	-0.0803	-0.0806	-0.0458	-0.0460	-0.0496	-0.0496
	(-1.558)	(-1.547)	(-1.123)	(-1.131)	(-1.189)	(-1.207)
Market Cap (t-1)	-0.0139	-0.0132	-0.0200***	-0.0193**	-0.0206**	-0.0199**
	(-1.619)	(-1.423)	(-2.581)	(-2.456)	(-2.354)	(-2.548)
Log # of Employee	-0.0505***	-0.0474***	-0.0467***	-0.0441***	-0.0462***	-0.0437***
	(-7.866)	(-9.804)	(-9.095)	(-10.26)	(-11.74)	(-10.23)
EBITDA/Assets	0.0608**	0.0563*	0.0925***	0.0873***	0.0946***	0.0894***
	(2.375)	(1.952)	(3.294)	(3.063)	(2.893)	(3.104)
Tobin's Q (t-1)	0.0127***	0.0128***	0.0112**	0.0113**	0.0118***	0.0118***
	(3.082)	(3.155)	(2.487)	(2.512)	(2.727)	(2.680)
Family Firm	0.00812	0.00838	0.0117	0.0119	0.0131	0.0133
	(0.648)	(0.659)	(1.024)	(1.036)	(1.134)	(1.167)
GIndex	-0.00400***	-0.00371**	-0.00222*	-0.00199*	-0.00216*	-0.00194
	(-2.823)	(-2.520)	(-1.843)	(-1.697)	(-1.702)	(-1.636)
EA FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry(SIC2) FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,169	10,169	10,169	10,169	10,169	10,169
Pseudo R2	0.3353	0.3348	0.3422	0.3417	0.3426	0.3421

Table 2.5 (cont'd): Tax Policy and AshleyMadison Membership

Panel B: Effective Corporate Tax Rate						
	(1)	(2)	(3)	(4)	(5)	(6)
Active AM Account	-0.208 ***		-0.203 ***		-0.203 ***	
	(-2.64)		(-2.51)		(-2.51)	
Dummy(AM>0)		-0.307		-0.378		-0.379
		(-1.57)		(-1.59)		(-1.59)
MostlyTxh50	-0.960 **	-0.988 **				
	(-2.08)	(-2.16)				
MostlyTxh75			-1.625 ***	-1.639 ***		
			(-2.43)	(-2.46)		
MostlyTxh90					-1.616 **	-1.632 ***
					(-2.31)	(-2.35)
international	-0.738 ***	-0.741 ***	-0.745 ***	-0.742 ***	-0.749 ***	-0.746 ***
	(-2.79)	(-2.8)	(-2.96)	(-2.95)	(-2.98)	(-2.97)
Institutional Investor	-0.574	-0.564	-0.580	-0.593	-0.582	-0.595
	(-0.93)	(-0.92)	(-0.94)	(-0.97)	(-0.94)	(-0.97)
R&D/Assets	-32.615 ***	-32.989 ***	-32.657 ***	-32.779 ***	-32.655 ***	-32.776 ***
	(-10.36)	(-10.43)	(-10.36)	(-10.4)	(-10.36)	(-10.41)
HHI (SIC4)	0.282	0.316	0.277	0.225	0.276	0.224
	(0.6)	(0.68)	(0.58)	(0.47)	(0.57)	(0.47)
Market Cap (t-1)	0.561 ***	0.510 ***	0.564 ***	0.552 ***	0.563 ***	0.551 ***
	(3.86)	(3.61)	(3.91)	(3.82)	(3.9)	(3.81)
Log # of Employee	-0.499 ***	-0.571 ***	-0.502 ***	-0.520 ***	-0.501 ***	-0.519 ***
	(-3.42)	(-3.79)	(-3.48)	(-3.6)	(-3.47)	(-3.59)
EBITDA/Assets	31.439 ***	31.575 ***	31.490 ***	31.557 ***	31.503 ***	31.570 ***
	(21.13)	(20.84)	(20.95)	(20.78)	(20.92)	(20.75)
Family Firm	0.852	0.870	0.846	0.864	0.849	0.867
	(1.33)	(1.36)	(1.31)	(1.34)	(1.32)	(1.35)
Tobin's Q (t-1)	-0.135	-0.129	-0.137	-0.136	-0.136	-0.135
	(-1.2)	(-1.15)	(-1.22)	(-1.21)	(-1.21)	(-1.2)
Gindex	0.023	0.020	0.026	0.029	0.026	0.029
	(0.8)	(0.7)	(0.89)	(0.99)	(0.89)	(0.99)
EA FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry(SIC2) FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7970	7970	7970	7970	7970	7970
Pseudo R2	0.344	0.344	0.344	0.344	0.344	0.344

Table 2.6: AshleyMadison Membership and the Choice of Internal vs. External CEO

In this table we report the marginal effects estimates from logit specifications of choosing an internal CEO (1) vs. external CEO (0) on the number of *active AshleyMadison (AM) accounts* and a dummy variable equal to 1 if *active AshleyMadison (AM) accounts* > 0. The data on CEOs come from Boardex for 2003-2014. We define a CEO as internal if he/she was employed at a given company for at least one full year before being appointed as CEO. Our regressor of interest is the natural logarithm of one plus the number of active AM accounts for a given firm year. Specifications 1-4 include year fixed effects, column 3 includes industry (2 digit sic code) fixed effects, and column 4 includes industry and EA fixed effects. All regressors are lagged one year relative to our CEO appointment variables. All variables are defined in the Internet Appendix. The t-statistics, calculated from standard errors clustered at the firm level, are reported in parentheses below coefficient estimates. Statistical significance (two-sided) at the 1% 5%, and 10% level is denoted by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)
Active AM Account	0.0874***	0.0844***	0.178***	0.235***	
	(11.60)	(9.576)	(13.91)	(7.890)	
Dummy(AM>0)					0.178***
					(9.112)
Dummy: Institutional Investor	0.0954***	0.0507	0.215***	-0.399***	-0.451***
	(3.138)	(1.536)	(4.114)	(-8.460)	(-9.677)
Shares held by insiders	-0.645***	-0.638***	-0.558***	-0.182*	-0.319**
	(-7.869)	(-7.851)	(-5.133)	(-1.899)	(-2.548)
HHI (SIC4)	0.218***	0.205***	0.335***	0.871***	0.918***
	(4.490)	(4.025)	(5.034)	(6.666)	(7.500)
Log Market Cap(t-1)	-0.0815***	-0.0800***	-0.0748***	0.0154	0.0868***
	(-11.21)	(-8.556)	(-4.234)	(0.789)	(5.146)
Log # of Employee	0.0450***	0.0458***	-0.0275**	0.0515**	0.0534**
	(5.712)	(5.605)	(-2.453)	(2.392)	(2.246)
Family Firm	0.0735	0.0783	-0.0974	0.133**	0.105
	(1.478)	(1.537)	(-1.461)	(2.414)	(1.604)
ROA	-0.107	-0.188*	0.137	-0.212	-0.554***
	(-1.253)	(-1.849)	(1.047)	(-1.017)	(-2.644)
Governance Index (Gompers, Ishii, Metrick)	-0.00614	-0.00563	-0.0142**	-0.0246***	-0.0112**
	(-1.463)	(-1.352)	(-1.978)	(-4.077)	(-2.546)
Founder is director	0.00779	0.0138	-0.0396	-0.0510	0.0189
	(0.306)	(0.524)	(-1.284)	(-1.003)	(0.450)
Tobin's Q (t-1)	0.0568***	0.0777***	0.00770	-0.0524*	-0.0751**
	(4.427)	(4.628)	(0.380)	(-1.662)	(-2.211)
Δ_{OROA}		-0.0172***	-0.00708***	-0.00441	-0.00630**
		(-5.916)	(-2.776)	(-1.519)	(-2.403)
Δ_{OROS}		0.0767	-0.276*	-0.169	-0.0494
		(0.400)	(-1.759)	(-1.264)	(-0.356)
Year FE	Yes	Yes	Yes	Yes	Yes
2-digit SIC FE			Yes	Yes	Yes
EA FE				Yes	Yes
Observations	955	955	850	766	766
Pseudo-R2	0.077	0.083	0.206	0.502	0.445

Table 2.7: Determinants of AshleyMadison Membership

In this table we report estimates for determinants of the number of active AshleyMadison (AM) accounts at the firm-level. We use Tobit specifications because the dependent variable, the natural logarithm of one plus the number of active AM accounts, is truncated at zero and continuous to the right of zero. Industry covariates are defined using four-digit SIC codes and geography covariates are defined at the zipcode level. Detailed variable definitions are provided in the appendix. All specifications have year fixed effects, specifications (2-6) include industry (three-digit SIC) fixed effects, and specifications (3-6) include EA fixed effects. The t-statistics, calculated from standard errors clustered at the firm level, are reported in parentheses below coefficient estimates. Statistical significance (two-sided) at the 1% 5%, and 10% level is denoted by *, **, and ***, respectively. We also report sigma and pseudo r-squared from the Tobit regressions. In unreported analyses we find qualitatively similar and statistically significant results using a linear probability model specification.

VARIABLES	(1) Active AM Accounts	(2) Active AM Accounts	(3) Active AM Accounts	(4) Active AM Accounts	(5) Active AM Accounts	(6) Active AM Accounts
Log Market Cap	0.214***	0.148***	0.122***	0.118***	0.121***	0.117***
	(8.60)	(5.58)	(39.68)	(36.00)	(35.65)	(33.40)
Firm Age	0.003	0.003	0.005***	0.005***	0.004***	0.005***
	(0.99)	(1.08)	(6.53)	(6.25)	(6.07)	(5.94)
Log # of Employee	0.306***	0.409***	0.420***	0.425***	0.420***	0.425***
	(13.66)	(13.78)	(89.94)	(87.99)	(88.13)	(86.28)
Volatility-3 Factor adjusted	0.999	3.621	3.180***	3.008***	3.092***	2.915***
	(0.30)	(1.22)	(4.68)	(4.15)	(4.07)	(3.73)
Population Density					0.024	1.965
					(0.00)	(0.16)
Population					0.036***	0.037***
					(9.97)	(9.85)
Median Population Age					-0.028***	-0.028***
					(-41.44)	(-40.37)
Avg Income per Household					-4.300***	-5.093***
					(-10.05)	(-11.57)
HHI (SIC4)				-0.044		-0.046
				(-0.91)		(-0.91)
Market to Book (SIC4)				-0.002		-0.002
				(-0.38)		(-0.35)
R&D intensity (SIC4)				0.659***		0.673***
				(5.13)		(5.05)
Sales growth rate (SIC4)				0.003		0.002
				(0.06)		(0.06)
sigma	1.503***	1.368***	1.314***	1.313***	1.314***	1.312***
	(46.06)	(45.42)	(179.15)	(171.34)	(168.38)	(164.61)
Observations	28,374	28,374	27,824	27,754	27,792	27,722
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE		Yes	Yes	Yes	Yes	Yes
EA FE			Yes	Yes	Yes	Yes
Pseudo-R2	.12	.174	.198	.198	.198	.198

APPENDIX C

PRODUCT COMPETITION AND CORPORATE FRAUD

Table 3.1: Variable Description

Variable	Definitions
AAER Misstatement	Equal to 1 for firm-years for which firms have settled with the SEC for corporate Fraud.
SCAC	Securities and Class Action Equal to 1 for all firm-years for which firms settle a securities class action lawsuit for an alleged 10B-5 fraud allegation.
Fraud	Equal to 1 for all firm-years with an AAER or SCA.
SIC3 HHI	Herfindahl-Hirschman index based on firm sales and three-digit SIC code industry classifications .
TNIC HHI	Herfindahl-Hirschman index based on firm sales Text-based Network Industry classifications (TNIC) from Hoberg and Phillips.
Avg Similarity Score	Mean Hoberg and Phillips Similarity Score for all rivals within each firm-year's TNIC.
Precision	Defined as $(\frac{1}{Ncomp_{TNIC}} \times \sum \frac{1}{(1-score)^2})^{0.5}$
Profit Margin	Average EBITDA/sales ratio for firms within each three-digit SIC code.
Top 4 Concentration	Proportion of sales within a three-digit SIC code attributable to the four largest firms within an industry.
Age	Number of years the firm has been in Compustat.
Analyst Num	Number of analysts covering the firm in each year from IBES (0 if missing).
Inst Ownership	Percentage of shares outstanding held by 13-F institutions.
Assets	Total Assets.
Capex	Capital Expenditures / log Assets.
Book Leverage	(Total Long-Term Debt +Debt in Current Liabilities)/ log Assets
ROA	Net Income / Assets
EFN	Equity Finance Needed defined as ROA/(1 âĹŠ ROA.
RSST Accruals	See Rachardson et. al (2005)
Dummy Security Issue	An indicator variable equal to 1 if the firm issued securities during year.
Change AR	Change in Accounts Receivable/Total Assets.
Change Inventory	Change in Inventory/Total Assets.
Pct Soft Assets	(Total Assets - PP&E- Cash and Cash Equivalent)/Total Assets.
Change in Cash Sales	Percentage change in Cash Sales - Change in Accounts Receivable.
Change in ROA	Change in Return on Assets.
Change in Employee	Percentage change in the number of employees - percentage change in assets.
R&D	Research and Development scaled by assets. Missing observations are filled with either the firm average, if a time series exists, or the industry average if not.
R&D (dummy)	Equal to 1 if R&D is missing and 0 otherwise.
NCOM SIC3	Number of competitors within the three-digit SIC Code.
Ind R2	Following Wang and Winton (2014), we first regress each firm's daily stock returns on the weighted-average daily market return and the weighted-average daily industry return. Then, we take the average r-squared for each firm in each three-digit SIC code.
RPE Flag	See Page 8.
NCOMP TNIC	Number of competitors according to Text-based Network Industry classifications (TNIC) from Hoberg and Phillips.

Table 3.2: Summary Statistics

This table reports summary statistics of firm characteristics at the firm-year level. Variable definitions are provided in the Table 3.1. Our sample spans 1996 through 2011.

VARIABLES	No. Obs	Mean	Std. Dev.	10th Percentile	90th Percentile
AAER Misstatement	55,381	0.014	0.119	0	0
SCAC	55,381	0.006	0.074	0	0
Fraud	55,381	0.019	0.135	0	0
Avg Similarity Score	55,381	0.03	0.023	0.012	0.055
Avg Top5 Similarity	55,381	0.08	0.058	0.017	0.156
Avg Top10 Similarity	55,381	0.066	0.05	0.014	0.135
Avg Top15 Similarity	55,381	0.059	0.047	0.013	0.123
Avg Score Precision	55,381	1.002	0.103	0.924	1.053
Sum Similarity	55,381	2.847	4.999	0.074	7.659
Product Market Fluidity	50,402	7.182	3.292	3.292	11.685
SIC3 HHI	55,381	0.176	0.145	0.062	0.332
SIC3 Profit Margin	55,381	-0.039	0.272	-0.346	0.156
TNIC HHI	55,381	0.235	0.197	0.064	0.518
NCOMP TNIC	55,381	74.204	90.52	5	204
NCOMP SIC3	55,381	121.607	170.694	6	351
RSST accruals	51,487	0.024	0.24	-0.182	0.22
Change AR	55,381	0.01	0.065	-0.045	0.07
Change Inventory	55,060	0.006	0.049	-0.028	0.05
Pct Soft Assets	55,377	0.541	0.245	0.175	0.852
Change in Cash Sales	51,888	0.195	0.71	-0.214	0.574
ROA	51,497	-0.005	0.195	-0.205	0.141
Change in ROA	54,671	-0.007	0.175	-0.149	0.12
Change in employee	54,053	-0.08	0.469	-0.365	0.241
Dummy Security Issue	55,381	0.92	0.272	1	1
Whited-Wu Index	54,954	-0.196	0.198	-0.389	0.012
Book Leverage	55,237	0.299	0.294	0	0.733
Capex	55,381	0.06	0.093	0	0.14
R&D	55,381	0.069	0.117	0	0.184
R&D dummy	55,381	0.627	0.484	0	1
Age	53,295	15.353	11.825	4	35
Inst Ownership	43,018	0.516	0.315	0.068	0.922
Number of Analysts	55,381	5.837	7.008	0	15
Stock Industry Return R2	53,238	0.342	0.173	0.121	0.58
Relative Perf Eval Flag	55,179	0.677	0.467	0	1
Ln Asset	55,381	5.618	1.937	3.155	8.181

Table 3.3: Product Market Differentiation and Corporate Fraud

This table reports OLS estimates for the incidence of fraud on the average similarity of each firmâĂŹs rivals. Our proxy for corporate fraud includes a combination of AAER misstatements from the AAER dataset and Securities Class Actions from the Stanford University Lawsuit Database. The specification in Column 1 does not include control variables. The specification in Column 2 includes controls used in Dechow et al. (2011). In Columns 3-5 we include our full set of controls as described in Section II and Column 3 also includes Institutional Ownership. All specifications are run at the firm-year level, include year fixed effects, and include explanatory variables are lagged by one year. Column 4 also includes three-digit SIC code (SIC3) fixed effects, Column 5 adds year × SIC3 fixed effects. The t-statistics, calculated from standard errors clustered at SIC3 level, are reported in parentheses below coefficient estimates. Statistical significance (two-sided) at the 10% 5%, and 1% level is denoted by *, **, and ***, respectively.

	(1) Fraud	(2) Fraud	(3) Fraud	(4) Fraud	(5) Fraud
Avg. Similarity Score	-0.113**	-0.180***	-0.220***	-0.171***	-0.163***
	(-2.089)	(-3.517)	(-3.915)	(-3.946)	(-3.244)
R&D			0.009	-0.011	-0.012
			(0.740)	(-1.008)	(-1.143)
R&D dummy			-0.000	-0.003	-0.004
			(-0.011)	(-0.914)	(-1.135)
Ln number analysts			0.001	0.001	0.001
			(0.227)	(0.267)	(0.392)
Inst Ownership			0.007		
			(0.792)		
Whited-Wu Index			0.005	-0.000	0.033*
			(0.859)	(-0.057)	(1.892)
RSST accruals		0.002	-0.003	0.001	0.002
		(0.538)	(-0.636)	(0.342)	(0.572)
Change AR		0.022*	0.023	0.016	0.023*
		(1.791)	(1.460)	(1.296)	(1.808)
Change Inventory		0.016	0.026	0.020	0.027
		(0.756)	(1.258)	(0.937)	(1.225)
Pct. Soft Assets		0.019***	0.022***	0.019***	0.019***
		(4.241)	(4.186)	(3.825)	(3.733)
Change in Cash Sales		0.005**	0.004**	0.005**	0.005**
		(2.250)	(2.057)	(2.227)	(2.367)
Change in ROA		-0.023***	-0.017***	-0.021***	-0.018***
		(-6.132)	(-3.336)	(-5.887)	(-5.212)
Change in employee		-0.004**	-0.004*	-0.003	-0.003
		(-2.101)	(-1.868)	(-1.456)	(-1.414)
Ln Age		-0.010***	-0.009***	-0.008***	-0.007***
		(-3.413)	(-3.040)	(-3.513)	(-2.974)
Dummy Security Issue		0.003	-0.001	0.002	0.001
		(1.327)	(-0.258)	(0.737)	(0.275)
Stock Industry Return R2			-0.009	0.017	
			(-0.822)	(1.515)	
Relative Perf Eval Flag			0.007**		
			(2.124)		
Ln NCOMP TNIC			0.001	0.002	0.001
			(1.205)	(0.945)	(0.756)
Ln Asset		0.006***	0.005***	0.006***	0.007***
		(4.617)	(3.245)	(2.913)	(3.546)
Observations	50,526	39,519	28,912	38,916	37,144
R-squared	0.005	0.015	0.014	0.034	0.079
FE	Year	Year	Year	Year+SIC3	Year×SIC3

Table 3.4: Product Differentiation and Fraud by Complexity Quartiles

This table reports OLS estimates for the incidence of fraud on the average similarity of each firm's rivals split into complexity quartiles. Our proxy for corporate fraud includes a combination of AAER misstatements from the AAER dataset and Securities Class Actions from the Stanford University Lawsuit Database. We define complexity as the number of unique SIC codes spanned by a firm's set of competitors according to the TNIC developed by Hoberg and Phillips, 2016. Panel A reports competitor and fraud classifications for each quartile. Panel B reports OLS estimates for each quartile including our full set of control variables described in Section II, and Panel C includes Institutional Ownership. The t-statistics, calculated from standard errors clustered at the three-digit SIC code (SIC3) level, are reported in parentheses below coefficient estimates. Statistical significance (two-sided) at the 10% 5%, and 1% level is denoted by *, **, and ***, respectively.

Complexity	Low Q1	Q2	Q3	High Q4
Panel A				
Unique SICs in TNIC	3.3	8.2	13	22.5
Competitors in TNIC	13	49	117	150
% Fraud	1.6	1.7	1.9	2.1
Avg Similarity Score	2.7	2.8	3.3	3.5
Panel B				
Avg Similarity Score	-0.168***	-0.197**	-0.201	-0.683***
	(-3.418)	(-2.029)	(-1.508)	(-5.053)
Observations	9,995	9,628	9,018	8,503
R-squared	0.016	0.017	0.024	0.026
FE	Year	Year	Year	Year
Controls	Full	Full	Full	Full
Panel C				
Avg Similarity Score	-0.191***	-0.169	-0.166	-0.677***
	(-3.217)	(-1.631)	(-1.029)	(-4.081)
Inst Ownership	0.010	0.004	0.009	0.011
	(0.769)	(0.225)	(0.729)	(1.012)
Observations	7,707	7,469	7,048	6,688
R-squared	0.015	0.021	0.023	0.023
FE	Year	Year	Year	Year
Controls	Full	Full	Full	Full

Table 3.5: IPOs and Acquisitions of Rivals as Change to Information Environment

This table reports OLS estimates for the association between fraud and rival IPOs (M&A) activity. The specifications are the same as model (4) of Table 3, but also include rival firm IPO (M&A) activity in Panel A (Panel B). For each firm-year, include the natural log of the number of firms that compete with firm i and that underwent an IPO or were acquired in year t, and an interaction term Ln Num Competitor IPO × Avg Similarity Score or Ln Num Competitor Target × Avg Similarity Score. In Column 2 of Panel A (B), we control for IPO (M&A) Size ($) which is the sum of all-capital raised by IPO rivals (total market capitalization of Target rivals). In Columns 4 and 5 of Panel A (Panel B), we split the data by high and low non-IPO (non-acquired) similarity scores in year t-1. All specifications include year fixed effects and all control variables are lagged one year. Columns 3-5 also include three-digit SIC code (SIC3) fixed effects. The t-statistics, calculated from standard errors clustered at the SIC3 level, are reported in parentheses below coefficient estimates. Statistical significance (two-sided) at the 10% 5%, and 1% level is denoted by *, **, and ***, respectively.

Panel A : Rival IPO					
	(1)	(2)	(3)	(4)	(5)
				Low Non-IPO Rival Score	High Non-IPO Rival Score
	Fraud	Fraud	Fraud	Fraud	Fraud
Avg Similarity Score	-0.149***	-0.149***	-0.134***		
	(-3.488)	(-3.473)	(-3.382)		
Ln Num Competitor IPO	0.009***	0.009***	0.008***	0.005***	0.004
	(4.767)	(4.909)	(4.565)	(2.82)	(1.44)
Avg Score × Ln Num Comp IPO	-0.119***	-0.123***	-0.109***		
	(-3.370)	(-3.460)	(-2.660)		
IPO Size ($)	-0.000		-0.000	-0.000	
		(-0.498)		(-0.02)	(-0.72)
Observations	37,144	37,144	37,144	18,858	18,279
R-squared	0.017	0.017	0.034	0.050	0.037

Panel B: Rival M&A					
	(1)	(2)	(3)	(4)	(5)
				Low Non-M&A Rival Score	High Non-M&A Rival Score
	Fraud	Fraud	Fraud	Fraud	Fraud
Avg Similarity Score	-0.185***	-0.185***	-0.162***		
	(-4.060)	(-4.061)	(-3.927)		
Ln Num Competitor Target	0.052***	0.061**	0.048***	0.103***	-0.000
	(3.373)	(2.436)	(3.380)	(3.141)	(-0.024)
Avg Score × Ln Num Comp Target	-0.844***	-0.874**	-0.685**		
	(-2.693)	(-2.593)	(-2.430)		
Ln Target MarketCap	-0.001		-0.008***	0.002	
		(-0.588)		(-2.829)	(1.380)
Observations	37,144	37,144	37,144	18,672	18,449
R-squared	0.017	0.017	0.035	0.052	0.039
Controls	Full	Full	Full	Full	Full
SIC3 FE	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Table 3.6: Product Market Differentiation and Corporate Fraud (Controlling for Alternative Measures of Competition)

This table reports OLS estimates for the incidence of fraud on Average Similarity Score, while controlling for alternative measures of competition. Our proxy for corporate fraud includes a combination of misstatements from the AAER dataset and Securities Class Actions from the Stanford University Lawsuit Database. Column 1 includes sales based Herfindahl-Hirschman Index (HHI) according to three digit SIC code (SIC3). Column 2 also includes the number of competitors (logged) in the same SIC3. Column 3 also includes the profit margin and an industry concentration measure. In Column 4 we include the sales based HHI according to the firmâĂŹs TNIC. Column 5 also includes the number of competitors within a firm's TNIC. Column 6 also includes the sum similarity score. The specifications include the full set of controls as described in Section II. All specifications are run at the firm-year level, include year fixed effects, and explanatory variables lagged by one year. The t-statistics, calculated from standard errors clustered at SIC3 level, are reported in parentheses below coefficient estimates Statistical significance (two-sided) at the 10% 5%, and 1% level is denoted by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Fraud	Fraud	Fraud	Fraud	Fraud	Fraud
Avg Similarity Score	-0.158***	-0.160***	-0.158***	-0.160***	-0.169***	-0.173***
	(-3.318)	(-3.305)	(-3.250)	(-3.519)	(-3.938)	(-3.712)
SIC3 HHI	0.017	0.033	0.009			
	(0.831)	(1.458)	(0.393)			
NCOMP_SIC3	0.016**	0.016***				
		(2.164)	(2.699)			
SIC3 PM sale		-0.007				
			(-0.719)			
Top 4 Concentration	0.041**					
			(2.140)			
TNIC HHI				-0.002	0.006	0.006
				(-0.276)	(0.985)	(1.091)
NCOMP_TNIC				0.002	0.002	
					(1.113)	(1.299)
Product Market Fluidity				0.000		
						(1.062)
Sum Similarity					0.000	
						(0.123)
Observations	37,335	37,335	37,335	37,335	37,335	37,335
R-squared	0.034	0.034	0.034	0.034	0.034	0.034
SIC3 FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 3.7: Alternative Measures of Competition and Corporate Fraud

This table reports OLS estimates for the incidence of fraud on commonly used industry-level proxies for competition. Our proxy for corporate fraud includes a combination of AAER misstatements from the AAER dataset and Securities Class Actions from the Stanford University Lawsuit Database. Our measures of competition include: sales based HHI according to TNIC, sales based HHI according to SIC3, sum similarity, product market fluidity, average (SIC3) profit margin, top-4 sales concentration and number of competitors constructed using three-digit SIC code. Columns 1-7 include the full set of controls as described in Section II. The firm-year is the unit of observation in this analysis. All specifications include year fixed effects, and control variables lagged by one year. The t-statistics, calculated from standard errors clustered at the SIC3 level, are reported in parentheses below coefficient estimates. Statistical significance (two-sided) at the 10% 5%, and 1% level is denoted by *, **, and ***, respectively.

	(1) Fraud	(2) Fraud	(3) Fraud	(4) Fraud	(5) Fraud	(6) Fraud	(7) Fraud
TNIC HHI	0.002						
	(0.397)						
SIC3 HHI		-0.001					
		(-0.108)					
Sum Similarity			-0.000				
			(-0.770)				
Product Market Fluidity				0.000			
				(0.800)			
SIC3 Profit Margin					-0.008		
					(-1.272)		
Top 4 Concentration						0.006	
						(0.719)	
Ln NCOMP SIC3							0.003*
							(1.871)
Observations	37,335	37,335	37,335	36,180	37,335	37,335	37,335
R-squared	0.014	0.014	0.014	0.014	0.014	0.014	0.015
Controls	Full	Full	Full	Full	Full	Full	Full
FE	Year	Year	Year	Year	Year	Year	Year

Table 3.8: Product Market Differentiation and Corporate Fraud âĂŞ Standardized Variables

This table reports OLS estimates for the incidence of fraud on standardized RHS variables. Our proxy for corporate fraud includes a combination of AAER misstatements and Securities Class Actions from the Stanford University Lawsuit Database. The specification in Column 1 does not include control variables. The specification in Column 2 includes control variables used in Dechow et al. (2011). In Columns 3-6 we include our full set of controls as described in Section II and Column 3 also includes Institutional Ownership. The unit of observation in this analysis is the firm-year. All specifications include year fixed effects, and control variables are lagged by one year. Column 4 includes three-digit SIC code (SIC3) fixed effects, Column 5 year x SIC3 fixed effects, and Column 6 firm fixed effects. All continuous RHS variables are standardized. The t-statistics, calculated from standard errors clustered at SIC3 level, are reported in parentheses below coefficient estimates. Statistical significance (two-sided) at the 10% 5%, and 1% level is denoted by *, **, and ***, respectively.

	(1) Fraud	(2) Fraud	(3) Fraud	(4) Fraud	(5) Fraud	(6) Fraud
Avg. Similarity Score	-0.003**	-0.004***	-0.005***	-0.004***	-0.004***	-0.002*
	(-2.089)	(-3.502)	(-3.915)	(-3.946)	(-3.230)	(-1.691)
R&D			0.001	-0.001	-0.001	-0.001
			(0.740)	(-1.008)	(-1.134)	(-0.906)
R&D dummy			-0.000	-0.003	-0.004	0.008
			(-0.011)	(-0.914)	(-1.171)	(1.306)
Ln number analysts			0.001	0.001	0.001	0.009***
			(0.227)	(0.267)	(0.378)	(3.995)
Inst Ownership			0.002			
			(0.792)			
Whited-Wu Index			0.001	-0.000	0.007*	0.000
			(0.859)	(-0.057)	(1.889)	(0.554)
RSST accruals		0.000	-0.001	0.000	0.000	0.000
		(0.550)	(-0.636)	(0.342)	(0.567)	(0.537)
Change AR		0.001*	0.001	0.001	0.002*	-0.000
		(1.805)	(1.460)	(1.296)	(1.817)	(-0.275)
Change Inventory		0.001	0.001	0.001	0.001	0.000
		(0.762)	(1.258)	(0.937)	(1.228)	(0.219)
Pct. Soft Assets		0.005***	0.005***	0.005***	0.005***	0.003**
		(4.227)	(4.186)	(3.825)	(3.735)	(2.214)
Change in Cash Sales		0.003**	0.003**	0.003**	0.004**	0.002*
		(2.236)	(2.057)	(2.227)	(2.359)	(1.776)
Change in ROA		-0.004***	-0.003***	-0.004***	-0.003***	-0.003***
		(-6.131)	(-3.336)	(-5.887)	(-5.182)	(-3.775)
Change in employee		-0.002**	-0.002*	-0.001	-0.001	-0.000
		(-2.105)	(-1.868)	(-1.456)	(-1.421)	(-0.343)
Ln Age		-0.007***	-0.007***	-0.006***	-0.005***	-0.010*
		(-3.402)	(-3.040)	(-3.513)	(-2.967)	(-1.859)
Dummy Security Issue		0.003	-0.001	0.002	0.001	0.003
		(1.479)	(-0.258)	(0.737)	(0.284)	(0.855)
Stock Industry Return R2			-0.002	0.003		0.004*
			(-0.822)	(1.515)		(1.800)
Relative Perf Eval Flag			0.003**			-106.330
			(2.124)			(-0.000)
Ln NCOMP TNIC			0.002	0.002	0.002	0.002
			(1.205)	(0.945)	(0.756)	(0.740)
Ln Asset		0.011***	0.009***	0.011***	0.014***	0.018***
		(4.607)	(3.245)	(2.913)	(3.547)	(4.653)
Observations	50,526	39,465	28,912	37,144	38,910	36,380
R-squared	0.005	0.015	0.014	0.034	0.078	0.437
FE	Year	Year	Year	Year Sic3	Year × Sic3	Year Gvkey

Table 3.9: Product Market Differentiation and Corporate Fraud Alternate Similarity Scores (Standardized)

This table reports estimates for the incidence of fraud on alternative standardized similarity scores of each firm's rivals using ordinary least squares (OLS) regressions. Our proxy for corporate fraud includes a combination of AAER misstatements from the AAER dataset and Securities Class Actions from the Stanford University Lawsuit Database. All variables of interest (not controls) are standardized for purposes of comparison. In Column 1, we report results for the main dependent variable used throughout our analysis. In Columns 2-4, we replace Average Similarity Score with a firm's product market similarity score averaged across its closest 5, 10, and 15 competitors, respectively. In Column 5, we replace Average Similarity Score with the Precision measure outlined in section III. In Columns 6-8, Average Similarity Score is replaced with the (natural log of) number of a firm's rivals in the top 75th, 90th and 95th percentile of similarity scores, respectively in the full cross section of firms in year t. All specifications are run at the firm-year level, include year fixed effects, and explanatory variables are lagged by one year. The t-statistics, calculated from standard errors clustered at SIC3 level, are reported in parentheses below coefficient estimates. Statistical significance (two-sided) at the 10% 5%, and 1% level is denoted by *, **, and ***, respectively.

	(1) Fraud	(2) Fraud	(3) Fraud	(4) Fraud	(5) Fraud	(6) Fraud	(7) Fraud	(8) Fraud
Std Avg Similarity Score	-0.005***							
	(-4.248)							
Std Avg Top5 Similarity		-0.007***						
		(-3.753)						
Std Avg Top10 Similarity			-0.007***					
			(-3.383)					
Std Avg Top15 Similarity				-0.006***				
				(-3.202)				
Std Precision					-0.005***			
					(-4.231)			
Std Ln NCOMP TNIC 75th						-0.004*		
						(-1.778)		
Std Ln NCOMP TNIC 90th							-0.004**	
							(-2.265)	
Std Ln NCOMP TNIC 95th								-0.004**
								(-2.314)
cline2-9								
Observations	37,144	37,144	37,144	37,144	37,144	37,144	37,144	37,144
R-squared	0.016	0.016	0.016	0.016	0.016	0.015	0.015	0.016
Controls	Full	Full	Full	Full	Full	Full	Full	Full
FE	Year	Year	Year	Year	Year	Year	Year	Year

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