The Ripple Effects of Financial Misconduct

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Declaration of Authorship

I hereby declare that this thesis entitled “The Ripple Effects of Financial Misconduct” is my own work and has not been previously submitted in as a thesis or dissertation for the award of a higher degree qualification elsewhere.

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Abstract. This thesis consists of three studies related to the wider effects of financial misconduct. In the first study, I show that information complementarities play an important role in the spillover of transparency shocks. I exploit staggered revelation of financial misconduct by S&P500 firms and find that the implied cost of capital increases for "close" industry peers relative to "distant" peers. Disclosure also increases. The effects are particularly strong when the close peers share common analysts and institutional ownership with the fraudulent firm. While disclosure remains high for the next four years, with sustained disclosure, the cost of equity starts to decrease. Firms' financing patterns tilt more towards debt financing initially at the expense of equity, but eventually revert.

In the second study, I investigate how suppliers adjust their innovation when financial fraud by a major customer is revealed. Consistent with the importance of "trust" when contracts are incomplete, suppliers reduce R&D, generate fewer patents, engage in more explorative innovation, and innovate in areas different from those of the fraudulent customer. Surprisingly, while the survival likelihood of the affected suppliers decreases in the next three years, over a ten-year period, survival likelihood is higher, and they attract more principal customers, than control firms. The results suggest that customer pressure and myopic incentives of supplier managers cause suppliers to pursue suboptimally diversified innovation strategies.

In the third study, I examine the strategic response of product market competitors when financial fraud by an industry leader is publicly revealed. I document evidence of predatory advertising and pricing. Close competitors of the leader step up advertisement spending relative to control firms. Although I do not directly observe product prices, I find that even though advertisement increases, competitors' profit margins drop, consistent with predatory pricing. Evidence of predation is stronger when rival firms have larger market share, the fraud firm has higher leverage, and when the average leverage of rival firms is lower. The effects appear mainly in industries that produce customized products and consumer switching costs are high. Increasing advertising expenditure appears to be a more potent predatory strategy in industries that experience new customer growth, whereas cutting prices appears more potent in industries with stagnant customer base. I present a switching cost model similar to Klemperer (1995) that generates implications broadly consistent with these observations.

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Chapter 1

Introduction

Serious corporate financial misconduct frequently shows in newspaper headlines. The Enron and Worldcom cases shocked the industry but more recently financial misconduct cases have become ubiquitous and an important cross-disciplinary topic in research.[1] One line of research documents negative consequences to firms of discovery of financial misconduct (e.g., negative stock price reactions and loss in sales). The cost to inflate one dollar of a firm's market value is roughly $4 (including both direct and indirect cost) when the firm's misconduct is revealed (Karpoff, Lee, and Martin, 2008).[2] However, there is little evidence of the effect of financial misconduct beyond the firm that commits to financial misconduct, such as the fraud firm's contracting partners and industry peers. If non-fraudulent firms are also affected by the news of fraud by others, then the understanding of the channel through which non-fraudulent firms are affected is informative to firms, regulators, and investors. A recent study by Giannetti and Wang (2016) find that financial misconduct erodes public confidence in the stock market resulting in a lowering of the willingness of households to participate in the stock market. Households even hold fewer stocks in non-fraudulent firms, which suggests that trust is indeed first-order importance that underlies the exchange activity in the stock market.

Although researchers have documented that some fraudulent firms rebuild reputation through improving board independence, governance, and internal control systems, there is mixed evidence that these reputation-rebuilding investments can yield positive net present value. Along the same line, one can examine how non-fraudulent firms (contracting partners, industry peers) respond to the news of the misconduct by other firms. They might also invest in activities trying to reverse the damage (if any) spilled from the fraudulent firm. In addition, business partners can try to change their relationship with the fraudulent firm if they are concerned about the opportunistic behavior of the fraudulent firm, and this is likely if the relationship goes beyond written contracts. Many studies document a loss of 20% or more in share values for firms when their financial misconduct is revealed (Karpoff, Lee, and Martin, 2008; Beneish, 1999). Declines in market values of a magnitude of 20% or similar can impair

[1] Please see Amiram, Bozanic, Cox, Dupont, Karpoff, and Sloan (2018) for reviews of studies on financial misconduct in law, accounting, and finance discipline.
[2] Direct costs can include fines, settlements, and legal expenses. Indirect costs can be loss in market value, higher cost of capital, decrease in sales.

a fraudulent firm's financial flexibility, and financing constraints are likely to be exacerbated if such stock price declines at least partially reflect trust and reputational problems. A set of firms that operate in the same product space as the fraudulent firm could compete aggressively and potentially benefit from increased sales, the fraudulent firm could not retaliate since its financial position is weakened when its financial misconduct is revealed.

In this thesis, I examine these wider effects of financial misconduct. In the second chapter, I use the public revelation of the financial misconduct as an adverse transparency shock and identify the spillover effects on peer firms. I find that a peer group of non-fraudulent firms experience an increase in the cost of capital, and this evidence is consistent with the importance of trust that underlies the stock market. This is also consistent with Giannetti and Wang (2016) who find that households reduce their holdings in firms located in the same state as the fraud firm after the revelation of financial misconduct. In addition, my results address the relatively underexplored issue of channels of disclosure spillover. I find that information complementarities between firms is an important determinant of the channel through which spillover occurs. Firms that are informationally related experience complementarities in the process of information generation by market participants for the fraudulent firm and the related firms. Industry peer group is a coarse proxy for information complementarity. Market participants are likely to regard the information environments of firms in the same close industry group as similar. Financial misconduct by a high-profile industry peer could call into question how good the quality of information for other firms in the same industry. Thus, firms that operate in the similar and/or related product space as the fraudulent firm and have similar business practices suffer more from the adverse disclosure shocks.

Ali and Hirshleifer (2020) find a strong momentum effect among analyst-connected firms, and this effect swamps all other momentum effects documented in the literature. I use analyst co-coverage and co-ownership as finer proxies for information complementarity and find that the effect of the adverse transparency shock increases in the strength of information linkage between fraudulent firms and peer firms. Finally, I argue that the equilibrium relationship between disclosure and the cost of capital can be either positive or negative, depending on the benefits and costs of disclosure. Indeed, my results show that the cost of capital of peer firms can increase when there is an adverse transparency shock, prompting more disclosure as the marginal benefit of more disclosure is increased by the shock. Thus, a positive relationship between disclosure and the cost of capital is observed. However, while firms maintain a high level of disclosure in the next four years, the cost of capital reverts to pre-shock levels in the third year after the shock. Finally, I contribute to a contentious literature that asks whether a firm's information environment is a first-order determinant of its financing choices. My finding that adverse transparency shocks cause firms to shift from equity financing to debt financing is consistent with the idea that information asymmetry matters for the types of securities firm issue.

In the third chapter, I investigate the effect of fraud on supply chain relationship in the context of the innovation strategy of upstream firms. The unexpected revelation of information also allows us to better understand the dynamics of supply chain relationship. Suppliers often make relationship-specific investments to cement long-term relationships with large customers. Investment in relationship-specific R&D cannot be specified ex-ante and complete contracts cannot be written. The customer's reputation may play an important role in motivating the suppliers to make risky long-term relationship-specific investments. I investigate how the scale and scope of supplier innovation activity change when the customer's reputation is adversely affected and trust in the customer is impaired. In particular, customers' reputation is impaired upon the revelation of financial misconduct of customers, and market participants revise their expectations for its future cash flows – in the sample, customers lose 10% of share value around the revelation of financial misconduct. The revelation of the financial misconduct of customers is likely to weaken the customers' bargaining power. Also, customer is perceived to be less likely to honor implicit contract thus the loss of trust reduces the incentive of the supplier to make relationship-specific investment. I find that while suppliers spend less on R&D and produce fewer patents, suppliers engage in more explorative innovation and improve survival likelihood over a 10-year period. In addition, if suppliers have other customers (non-fraudulent firms), they tailor their innovation towards non-fraudulent customers and shift away from the technology pool of fraudulent customers. I use the exposure to customer fraud to instrument explorative innovation and show that more explorative innovation is associated with higher survival likelihood over a 10-year period. Overall, the results suggest that suppliers prioritize relationship-specific innovation at the cost of a more diversified innovation strategy which could be more beneficial in the longer term.

In the fourth chapter, I examine the behavior of product market competitors when the financial misconduct of a major firm in the same product market is revealed to the public. Tirole (19850, Poitevin (1989), Benoit (1984), and Bolton and Scharfstein (1990) show that a rival firm's financial condition and/or financial market imperfections can encourage predatory behavior. The revelation of financial misconduct is associated with significant declines in the market values of the fraudulent firms (20%), impairing their financial flexibility. Moreover, the losses in share value include loss in reputational capital, e.g., higher cost of capital, are likely to exacerbate financial constraints. Using the product similarity group constructed by Hoberg and Phillips (2010, 2016), I find that close competitors lower their profit margin after the revelation of financial misconduct. The profit margin drops in a more concentrated industry, and when the fraud firm's leverage relative to competitors is higher, i.e., the potential market share gains are large, and the fraudulent firms cannot retaliate due to their weakened financial position. Also, I document the predatory role of advertising which is unexplored in the literature. Close competitors increase their advertising spending, and the increase is larger in concentrated industries and when the fraudulent firm's leverage relative to competitors is higher. Also, the predation is strong when consumers face higher switching costs in the industry since consumers would stay with a firm whose products they become familiar with. One example of switching cost is the uniqueness of the feature of a product, and the industry's R&D spending is a reasonable proxy in this context. Industries that spend more on R&D produce more unique or specialized products, and customer switching costs are likely to be higher (Opler and Titman, 1994; Bhattacharyya and Nanda, 2000). In low switching cost industries, I find no evidence of predation since consumers could not develop loyalty and could switch back later. I find evidence of predation in high switching cost industries since it is only profitable to spend resources to switch customers if they develop loyalties and are not easily switched back or lured away by other competitors. Finally, I examine the recent growth of the number of customers across different industries and find that advertisement and lowering price are two predatory strategies in different circumstances. Stepping up advertisement is the more effective strategy when new customer growth is significant, whereas lowering price is more effective in attracting existing customers in markets without significant new customer growth.

Chapter 2

Information Complementarities and the Dynamics of Transparency Shock Spillovers

2.1. Introduction

Firms' disclosure policies impact their information environment, thereby affecting their security issuance and real investment decisions (Goldstein and Yang, 2019; Kanodia and Sapra, 2016). One important strand of literature is concerned with the effect of disclosure on the cost of capital, and argues that more disclosure can lower the cost of capital by reducing uncertainty about firms' future cash flows. Disclosure and information transparency are also important for outside investors to monitor management, and for regulators to ensure that financial misconduct does not affect the trust of investors about the integrity of financial markets.

Despite the obvious importance of the relationship between disclosure and firms' cost of capital, and the importance of externalities associated with disclosure failures such as financial frauds, causal empirical links have been difficult to establish (Leuz and Wysocki, 2016). One reason for this is that voluntary disclosure is likely to be an endogenous decision of firms. Moreover, while changes to disclosure regulation are arguably exogenous, the effects of such changes are likely to be confounded by anticipation effects and other contemporaneous confounding events.

The fact that disclosure decisions are voluntary can complicate even the theoretical relationship between disclosure and the cost of capital. For example, exogenous events that increase uncertainty about the firm's cash flows can cause both the cost of capital and the amount of disclosure to increase, at least in the short run. Put differently, rather than more disclosure leading to a lower cost of capital, a higher cost of capital could lead to more disclosure.[3] This makes the empirical association between disclosure and the cost of capital difficult to identify—perhaps one reason why the empirical evidence has not been very convincing.[4]

[3] Clinch and Verrecchia (2015) present such a model. Leuz and Schrand (2009) present empirical evidence following the Enron scandal, which supports such a dynamic interaction. Both papers are further discussed below.
[4] Leuz and Wysocki (2016) provide an extensive discussion of the difficulties of interpreting related empirical evidence.

In this paper, I address two major issues. First, I provide evidence of this complex interaction. I show that when firms are exposed to exogenous shocks originating in other firms that are likely to raise concerns about the quality of their own disclosure and that of the available information, the cost of capital goes up, and firms increase disclosure. In other words, the contemporaneous relationship between the cost of capital and disclosure can be positive. However, as firms engage in sustained disclosure, the cost of capital eventually declines. I also show that consistent with the notion that the cost of equity is more sensitive to information asymmetry than debt, initially, when there is an increase in the demand for disclosure, firms switch to debt financing at the expense of equity financing. However, with sustained disclosure, the preference for debt is no longer present.

Second, I provide evidence on the channels through which shocks to a firm's information environment are transmitted to other firms. Spillovers or externalities are important considerations in the discussion of disclosure regulation; however, causal evidence on the existence of spillovers as well as the channels through which it is likely to occur is sparse. Of particular concern are the spillover effects of disclosure failure, e.g., financial misconduct. Giannetti and Wang (2016) show that the revelation of financial misconduct by firms can have widespread effects on the stock market. Following fraud revelation, households' stock market participation in the state where the fraudulent firm is headquartered decreases, even in firms that did not engage in fraud. This evidence is consistent with the view that trust in the stock market matters for investor participation, as highlighted in the influential work of Guiso, Sapienza, and Zingales (2008).

I establish that *information complementarity* plays an important role in the transmission of adverse disclosure shocks to other firms.[5] Specifically, I show that adverse disclosure shocks are likely to propagate to other firms that are informationally related by virtue of operating in similar product markets. Further, because analysts (blockholders) tend to cover (own) stocks with information complementarities, common analyst coverage, or common block ownership by institutional investors, identify firms that are exposed to these spillovers extremely well.

The empirical setting I exploit is as follows. I identify financial misconduct committed by S&P 500 firms and examine the effect this has on the cost of capital and disclosure activity of peer firms in the same SIC 4-digit industry (alternatively, in the same SIC 3-digit industry).

[5] As discussed in section 2.3, a group of firms are related via information complementarity if their fundamentals are perceived to be correlated.

Market participants are likely to regard the information environments of firms in the same close industry group as similar, and so financial misconduct by a high-profile industry peer such as an S&P 500 constituent could call into question how good the quality of their information is regarding other firms in the same industry. I hypothesize that information complementarity of the fraudulent firm with firms in the same 4-digit (3-digit) industry will be higher than with firms in the same 2-digit industry (but not in the same 3-digit industry). I conduct the analysis in a difference-in-difference setting with firms in the same 2-digit industry (but a different 3-digit industry) chosen as control firms. I examine how the revelation of financial misconduct by a high-profile firm in a particular 4-digit (3-digit) industry affects the peer firms' implied cost of capital, relative to that of the control group.[6] I find that the implied cost of capital of the close peer firms (same 4-digit or 3-digit firms as the fraudulent firm) goes up following the revelation of misconduct. This is consistent with the idea that the high-profile fraud revelation causes market participants to re-evaluate what they know about the fundamentals of close industry peers (for example, their likely exposure to the same industry shocks that might have affected the fraudulent firm and instigated misconduct). As a result, the cost of capital goes up for close peers relative to distant peers, who are less likely to be similarly affected.[7]

I explore how the peer firms' disclosure responds to the revelation of financial misconduct by the high-profile firm. I find that, in the same difference-in-difference setting, the peer firms significantly increase the frequency of management forecasts and the length of the Management Disclosure and Analysis (MDA) section in the 10-K filings. I find similar results for a market-based measure of revelation of firm-specific information, namely, the logarithm of one minus the market-model R^2.

I next examine whether the twin effects of the higher cost of capital and more disclosure among peer firms are stronger for firms that share common analysts and common institutional ownership with the high-profile fraudulent firm. This is plausible because even within the peer firms (which are already informationally related as they are likely to operate in similar product markets), information complementarities will be stronger when the same analyst covers a peer

[6] Following the method of Pástor, Sinha, and Swaminathan (2008) and Chava and Purnanandam (2010), I calculate the implied cost of capital for each firm as the internal rate of return, which makes the current stock price equal to the present value of the expected stream of free cash flows to equity.

[7] There could be concern that common industry or other shocks could cause the high-profile industry leaders to commit fraud and at the same time directly affect the cost of capital and disclosure of close industry peers. Since the period over which fraud is committed typically precedes the year fraud is revealed, I am able to check whether the close and distant peers' cost of capital and disclosure diverge when fraud is actually being committed (possible due to prevailing industry conditions). I find no such evidence.

firm, or the same institutional investor owns a peer firm, together with the high-profile fraudulent firm. Ali and Hirshleifer (2020) find "shared analyst coverage to be a strong and versatile proxy for fundamental linkages between firms and for relatedness of firms." They show that momentum spillover effects among stocks are stronger for firms with common analysts and that this effect swamps all other momentum spillover effects documented in the literature. They also show that this effect is due to the fact that analyst co-coverage identifies fundamental linkages between stocks more sharply.[8] As for co-ownership, the notion that information complementarities encourage investors to hold stocks with similar fundamentals has been put forward as an explanation of the home-bias puzzle (Van Nieuwerburgh and Veldkamp, 2009). Kacperczyk, Sialm, and Zheng (2005) show that funds with industry concentration yield an average return that is 1.1% per year higher than those with below-median concentration.

I find that peer firms related to the high-profile fraudulent firm via co-coverage or co-ownership experience larger increases in the cost of capital following the revelation of financial misconduct. I also find that the subsequent increase in the frequency of management forecasts and the length of the MDA section is stronger for such firms.[9] I find no evidence that co-coverage or co-ownership affects the disclosure or cost of capital for firms in the same 2-digit SIC industry as the fraudulent firm, validating my premise that information complementarity is likely to be weaker at the 2-digit industry level.

Interestingly, I find no evidence that having an auditor in common with the fraudulent firm exposes the peer firm more to the transparency shock spillover. My findings therefore do not support the view that (a) being audited by the same auditing firm indicates particularly strong information complementarities among firms, or that (b) the revelation of financial fraud typically reflects on the quality of the auditor involved.[10] Finally, to examine the possibility that strategic product-market considerations might affect the disclosure activities of the peer firms (as well as those connected to the fraudulent firm via co-coverage and co-ownership), I explicitly control for the Hoberg and Phillips (2010) product similarity score between the fraudulent firm and the close or distant peer firm. I examine whether firms with higher

[8] See also Lee, Ma, and Wang (2016), Muslu, Rebello, and Xu (2014), and Israelsen (2016) for related findings.
[9] For close 4-digit peers that are not linked to the fraudulent firm via co-coverage or co-ownership, the effects on the cost of equity and disclosure are generally much smaller in magnitude (though still significant at conventional levels). The effects are typically insignificant for unlinked close peers are defined at the 3-digit SIC level.
[10] Minutti-Meza (2013) finds that industry specialization by auditors is unrelated to audit quality or audit fees, suggesting that the benefits from auditor specialization are not particularly significant.

similarity scores have a higher cost of capital and increase disclosure after the misconduct is revealed. I find no such evidence, and the main results for close peers and firms linked via co-coverage and co-ownership remain.

Next, I examine the dynamic behavior of the cost of capital and disclosure in my difference-in-difference setting. Consistent with Clinch and Verrecchia (2015) and Leuz and Schrand (2009), I find that the cost of capital and disclosure of the peer firms increase in the first two years after the high-profile firm's financial misconduct is revealed. However, while the level of disclosure of the peer firms continues to increase over the next two years, the cost of capital of the peer firms is no longer higher than that of the control firms after the first two years. In other words, the usual negative association between disclosure and the cost of capital manifests with sustained disclosure. While these results are not causal and merely document an association, they are consistent with the interpretation that sustained disclosure (or a commitment to more disclosure) improves transparency and succeeds in bringing down the cost of capital.

Finally, I examine the financing behavior of the peer firms. The price of equity is more subject to information asymmetry than that of debt (Myers and Majluf, 1984). Therefore, less informative signals about a firm's cash flows are likely to affect the cost of equity more than the cost of debt. This should cause the peer firms to switch more towards debt financing at the expense of equity financing following the revelation of misconduct by the high-profile firm. This is what I find. Overall, in the four-year period subsequent to the revelation of misconduct, equity issuance likelihood decreases four percentage points and is largely offset by a four percentage point increase in the likelihood of debt issuance. I also observe a dynamics consistent with the dynamic pattern of cost of equity discussed above: the switch to debt financing at the expense of equity financing occurs within the first two years, but there is no discernible difference vis-a-vis the behavior of control firms in the next two years.

2.2. Related Literature and Contribution

My paper is close in spirit to that of Leuz and Schrand (2009). The authors examine the effects of the Enron 2001 scandal on firms' betas over two windows, one immediately after the scandal (event period), and one before the next round of annual reports (pre-report period), and consider the difference of the betas estimated over each of these windows and the pre-event beta as measures of "beta shocks", or shocks to the cost of capital. They show that the extent of disclosure in the annual reports is positively related to these beta shocks. They also show that more disclosure is related to lower estimated betas for the post-reporting period. Leuz and Schrand (2009) motivate the higher disclosure in response to higher beta shocks in terms of firms' attempts to provide more information to mitigate the adverse effects of the Enron scandal on the information environment and the cost of capital.

My analysis differs from Leuz and Schrand (2009) in the following respects. First, I focus not only on one event, but on many events, and thus I can examine the effect of financial misconduct by high-profile firms on peer firms in a staggered difference-in-difference setting. I focus on the differential effect on "close peers" (peer firms in the same 4-digit SIC industry) versus "distant peers" (those in the same 2-digit industry). Drawing on control firms from the same 2-digit industry alleviates the concern that other contemporaneous sources of industry shock (at the 2-digit level) could be driving my results. Moreover, disclosure practices may reflect economic fundamentals (Leuz and Wysocki, 2016), and thus it is appropriate that my control sample is chosen to reflect at least some of these fundamentals.[11]

Second, I can directly examine the effect on the implied cost of equity, which is difficult to do by examining beta shocks on all firms in the market because the average beta must add up to one. Third, a novel feature of my analysis is that, by examining the effect of common analysts and common ownership, I provide evidence that information complementarity plays an important role in the transmission of voluntary disclosure shocks. My results also suggest that co-coverage and co-ownership by analysts and institutional investors, respectively, could make these agents important conduits for the transmission of disclosure externalities. Finally, I show that peer firms' security issuance activity is affected in a manner consistent with great information uncertainty or asymmetry surrounding the misconduct events.

My paper builds on a large theoretical literature on the relationship between disclosure and the cost of capital. An early literature that emphasized the role of disclosure in reducing estimation risk (Barry and Brown, 1985; Coles and Loewenstein, 1988; Handa and Linn, 1993; Coles, Loewenstein, and Suay, 1995) shows that more precise parameter estimates pertaining to the return-generating process can reduce the cost of capital. Another class of models (Diamond and Verrecchia, 1991; Easley and O'hara, 2004) explore the role of disclosure in

[11] As I demonstrate, there is little evidence of any spillover to distant peers. In additional tests reported in Appendix 2.4, I compare the effects on the 2-digit peers relative to a control sample drawn from firms with a different 1-digit SIC code. I find no effects of the transparency shock emanating from the high-profile financial misconduct on the cost of capital or disclosure policy of 2-digit peers.

reducing information asymmetry in models in which market-making activity is explicitly considered. In these papers, higher stock liquidity is associated with a lower cost of capital. Lambert, Leuz, and Verrecchia (2007) show that the cost of capital is related to both the variance of *future* cash flows and the sum of its covariance with the cash flows of other firms in the market. As the information quality of signals related to the firm's cash flow improves, the conditional covariances decrease, leading to a lower cost of capital. More disclosure can be regarded as improved information quality of signals, and thus is associated with a lower cost of capital.

My study contributes to the empirical literature that examines the association between disclosure and the cost of capital and stock liquidity. This literature encompasses both mandatory and voluntary disclosure. Leuz and Wysocki (2016) provide a comprehensive and insightful survey of this literature, so I do not attempt to review it here. As Leuz and Wysocki (2016) point out, this literature faces several empirical challenges. Disclosure and the cost of capital can be spuriously related due to common factors affecting both. Further, as noted, there could also be reverse causality since shocks to the cost of capital could drive disclosure. My paper contributes to the literature by exploiting a staggered difference-in-difference setting and financial misconduct events of high-profile firms, which arguably addresses some of these identification challenges commonly encountered in the literature. Specifically, by focusing on spillovers, I avoid some of the self-selection issues that typically challenge empirical investigation of the effects of voluntary disclosure on the cost of capital. Since the triggering events are not voluntary disclosures but revelations of financial misconduct, the typical "reflection problem" (Manski, 1993) that plagues identification in spillover studies is also absent in my setting.

My paper also contributes to a literature that anticipates that, in a voluntary disclosure setting, the interaction between the cost of capital and disclosure can exhibit a more complex relationship than empirical models have typically tried to test. In particular, exogenous changes to perceived riskiness of future cash flows or other parameters such as investor risk aversion could cause the relationship between disclosure and the cost of capital to be positive. Clinch and Verrecchia (2015) make a distinction between voluntary disclosure and disclosure commitment, and argue that "the chief empirical implication of my paper is that the contemporaneous relation between a change in the level of disclosure and the discount in price as a result of a change in the risk environment is positive. However, to the extent to which increased disclosure is subsequently perceived as a commitment, then the relation between a change in the level of disclosure and the discount will be negative." The results in my paper are consistent with this conjecture.[12] My results are consistent with the empirical results in Leuz and Schrand (2009), who also find a similar dynamic relationship, as well as Balakrishnan, Billings, Kelly, and Ljungqvist (2014), who find that after an exogenous decrease in analyst coverage due to brokerage closures and consequent increase in information asymmetry, firms voluntarily disclose more information in the form of management guidance. Consistent with my results, they find that after the loss of analyst coverage, liquidity initially decreases and then improves.

Third, I contribute to the important question of the spillover effects of disclosure, including disclosure failure. Xu, Najand, and Ziegenfuss (2006) examine the intra-industry effect of earnings restatement and argue that the accounting irregularities of restating firms cause a contagion effect rather than a competitive effect within the industry. They arrive at this conclusion by showing that when a restating firm reacts negatively (positively) to the announcement of a restatement, its rival firms tend to exhibit negative (positive) announcement returns. In a similar vein, Gleason, Jenkins, and Johnson (2008) find evidence that the accounting misstatement raises investors' concerns about the trustworthiness and content of financial statements previously issued by industry peers. They show that peer firms' stock prices decline in response to restating firms' announcement, and among peer firms with high earnings and high accruals, the spillover effects are concentrated for those who share the same external auditor with the restating firm. Recent empirical literature also documents a positive spillover effect of disclosure failure. Silvers (2016) documents that during the event window of the SEC enforcement targeted at foreign firms, stock returns are positive for non-target foreign firms, in general, and greater for firms from countries with weak legal environments, in particular. His findings support the view that enforcement actions reduce agency costs as investors benefit from public enforcement and decrease involvement in costly private monitoring. In contrast with my work, these papers focus on the short-term spillover effects on announcement returns while the long-term and the dynamic spillover effects on disclosure, cost of capital, and financing occupy center stage in my analysis. My results point to previously unexplored channels through which disclosure events experienced by one firm are likely to

[12] Larcker and Rusticus (2010) also note that "……firms with high risk and uncertainty in their business environment (and thus a high cost of capital) may try to increase their disclosure quality in order to reduce the cost of capital. To the extent that they are only partially successful, this causes a positive relation between disclosure quality and cost of capital."

affect the disclosure decisions of other firms. I find that information complementarities play a key role in the transmission of transparency shocks. I identify two indicators of information complementarity. Firms that are similar in terms of the types of business activity they pursue and are linked by common analyst coverage and common institutional ownership are most likely to influence and be influenced by each other's disclosure. My findings on the spillover effects of disclosure failure by fraudulent firms also complement the arguments and findings of Guiso et al. (2008) and Giannetti and Wang (2016) that mistrust in the stock market can be an important channel for the spillover effects of financial misconduct.[13]

Finally, my findings have implications for the literature studying the relationship between disclosure decisions and financing choices. Building on the well-recognized idea that information asymmetry affects financing, Healy, Hutton, and Palepu (1999) show that improved voluntary disclosure is positively associated with stock returns, stock liquidity, analyst coverage, and institutional ownership. They argue that increasing disclosure enables firms to have access to financial markets by finding that the expansion of disclosure is associated with decreases in private financing and increases in external financing. A growing body of evidence also shows that firms strategically increase disclosure during the pre-offering period to reduce information asymmetry (Lang and Lundholm, 2000; Schrand and Verrecchia, 2005; Leone, Rock, and Willenborg, 2007; Shroff, Sun, White, and Zhang, 2013). I extend these studies by focusing on how the industrywide information environment influences firms' choices between debt and equity financing. It is well recognized that more information asymmetry is associated with a preference for debt financing over equity financing. However, empirical evidence seemingly is at odds with this proposition, since small firms that are supposed to be more subject to information asymmetry than large firms rely more on equity financing (Rajan and Zingales, 1995; Frank and Goyal, 2003; Halov and Heider, 2011). My results show that a transparency shock to a high-profile peer firm affects the cost of equity capital more than that of debt, as implied by theories of adverse selection (Myers and Majluf,

[13] My paper indirectly relates to the literature on the peer effects of disclosure failure on real investment decisions. For example, Sidak (2003) studies the adverse impact of WorldCom's accounting fraud on rival firms in the telecommunication industry. He finds that the WorldCom's overstatement of internet traffic misled the government and rival firms' on the industry prospect, resulting in overinvestment problems. A related paper by Sadka (2006) builds a model in which a firm's accounting fraud influences the industry adversely. He argues that a fraudulent firm disguises its misbehaviors by increasing outputs and decreasing prices. Such suboptimal decisions made by the fraudulent firm will result in lower equilibrium prices. Durnev and Mangen (2009) develop a model in which financial reports and especially the restatements of financial reports alleviate the rival firms' uncertainties about demand and cost conditions in the restating firms' industry. In response to the announcement of restatements, rival firms update their beliefs about other firms' strategic decisions and adjust their investment decision accordingly.

1984), since the price of equity is more information-sensitive than that of debt. Consistently, firms move away from equity financing and towards debt financing; however, this pattern reverts as firms engage in more disclosure, and the cost of equity decreases. Collectively, this evidence strongly supports information-based theories of financing choice.

2.3. Hypothesis Development and Empirical Implications

My research question concerns the spillover effects of transparency shocks to firms that share information complementarities with the firms that are subject to these shocks. As explained in more detail below, following methods in Karpoff, Koester, Lee, and Martin (2017), I hand-collect the dates of trigger events that attracted the attention of the SEC and prompted informal inquiry and/or a formal letter of investigation by the SEC relating to violations of 13(b) provisions of the 1934 Securities Exchange Act and the Code of Federal Regulations.[14] I focus on high-profile financial misconduct committed by industry leaders (S&P 500 firms that were accused of 13(b) violations). I hypothesize that these trigger events are shocks to the transparency of the information environment of the high-profile firm that are likely to spill over to other firms with which the affected firm has information complementarity.

For my purposes, *information complementarity* refers to the idea that there are complementarities in the process of information generation by market participants for a group of firms, so that any new information for a member of the group has implications for how other members of the group are assessed. Fundamentals can be correlated for many reasons – for example, firms that operate in similar product or factor markets, or have similar business models, are likely to have correlated fundamentals and experience information complementarity. Transparency shocks such as the revelation of financial misconduct are likely to cause market participants to re-assess the *precision* of their signals about the fraudulent firm's fundamentals. Such shocks can spill over to the information environment of other firms with which it has information complementarities. Since the precisions of the signals are revised downwards, these shocks are essentially "beta shocks" for informationally related firms (Lambert et al. 2007; Leuz and Schrand, 2009) that are likely to affect their cost of capital. However, it is also possible that negative transparency shocks also cause the expected cash

[14] Sometimes informal inquiry is followed by a formal letter of investigation, though this is not always the case. The SEC usually sends a formal letter of investigation to a firm if they need to issue subpoenas to managers to obtain additional evidence. If the SEC can obtain all the evidence without issuing subpoenas, then the investigation remains informal. In the enforcement releases or news items, the SEC would usually state what the trigger event led to the informal and/or subsequent formal investigation.

flows of informationally related firms to be revised downwards, thereby causing the cost of capital to increase.

I proxy for the presence of information complementarity with the high-profile fraudulent firm in two ways. My coarse proxy for information complementarity is based on 4-digit (alternatively, 3-digit) SIC industry classification. This is motivated by the fact that peer firms in the same 4-digit or 3-digit industry produce similar and/or related products and have similar business practices. I benchmark the effect of the transparency shock on these close industry peers against that on distant industry peers, as represented by firms in the same 2-digit SIC industry (but not in the same 3-digit SIC industry). I pick control firms with some industry overlap to partially control for common shocks to the industry at the 2-digit level. In principle, there can be spillover effects to these control firms as well (indeed, to firms in any other industry (Leuz and Schrand, 2009)). Thus, my empirical approach is designed to test whether the spillover effects are stronger for firms with which the informational complementarities are likely to be stronger.[15] I use multiple financial misconduct events associated with high-profile firms to implement a staggered (and stacked) difference-in-difference setting; thus, the magnitudes of the coefficient estimates are always interpreted relative to the control group.

I also use two finer proxies. Recent literature (Ali and Hirshleifer, 2020; Lee, Ma, and Wang, 2016; Muslu, Rebello, and Xu, 2014; Israelsen, 2016) suggests that information complementarities are particularly strong among stocks that are covered by the same analyst. I accordingly hypothesize that within 4-digit (alternatively, 3-digit) same industry peers, the spillover effects of the transparency shock to a high-profile firm will be stronger among peer firms that are covered by analysts who also cover the high-profile firm (*Co-coverage*). While some control firms in the same 2-digit SIC industry can also be subject to co-coverage, I expect the informational complementarities between the fraudulent firm and such firms to be weaker than between the fraudulent firm and the 4-digit or 3-digit industry peers linked by co-coverage.

A second finer proxy is co-ownership by the same institutional investor of the peer firm's stock and the fraudulent firm's stock (*co-ownership*). I motivate this proxy for informational complementarity by appealing to the same theoretical arguments advanced for the home-bias puzzle (Van Nieuwerburgh and Veldkamp, 2009). Kacperczyk et al. (2005) find that funds with industry concentration exhibit better performance than those with below-median concentration.

[15] However, as shown in Appendix 2.4, I find that such spillovers to the chosen control firms are absent.

Cohen and Frazzini (2008) find evidence that common institutional ownership is associated with information complementarities among vertically related stocks.

With these proxies for information complementarity in mind, my first hypothesis concerns the immediate spillover effect of the negative transparency shock on the cost of capital of close peer firms compared with more distant peers. Generally, a negative transparency shock should cause investors to question the precision or the quality of their information not only for the firm in question but any related firms, resulting in an increase in the cost of capital of those firms (Lambert et al. 2007; Clinch and Verrecchia, 2015; Leuz and Schrand, 2009). If the shock in question is very significant, such as the Enron shock, then this might apply to the entire economy (Leuz and Schrand, 2009). However, my main argument is that the effect should be stronger for firms with which the high-profile firm has greater information complementarity than for those with which that complementarity is less. Hence, I propose:

Hypothesis 1. A negative transparency shock to a high-profile firm (i) will cause the cost of capital of close peers to increase relative to distant peers. (ii) The shock will increase the cost of capital of firms with *co-coverage* and *co-ownership* in the group of close peers more than that of other firms.

The next issue is how firms are expected to respond to this increase in the cost of capital in terms of their disclosure choice. Disclosure affects the information environment of the firm and, thus, the cost of capital. Firms choose the optimal amount of disclosure by trading off the potential benefit from greater disclosure (e.g., improvement in the information environment, lower cost of capital, greater stock liquidity, etc.) against the direct and proprietary costs of more disclosure (e.g., preparation of financial statements, usage of information by competitors, etc.). My hypothesis is that the negative transparency shock increases the marginal benefit of more disclosure, and this benefit is greater the more the information complementarity with the high-profile fraudulent firm. Hence, I propose:

Hypothesis 2. A negative transparency shock to a high-profile firm (i) will cause disclosure by close peers to increase relative to that by distant peers. (ii) will increase disclosure by firms with *co-coverage* and *co-ownership* in the group of close peers more than that by other firms.

It may be noted that Hypothesis 1 and 2 together imply a positive association between the cost of capital and disclosure. Clinch and Verrecchia (2015) provide a model that formalizes a channel through which such a relationship could come about. However, theirs is a single-firm model, and the notion of information complementarity is absent. It also needs to be pointed out that my hypotheses and results concern how different degrees of information complementarity matter for the spillover effect of a negative transparency shock on the cost of capital and disclosure, which is a somewhat different comparative static exercise than envisaged in that paper.

I next turn to the dynamic relationship between disclosure and the cost of capital. Clinch and Verrecchia (2015) point out that most approaches that address the relationship between disclosure and the cost of capital or liquidity implicitly assume that firms can commit to a disclosure policy. In my setting, I argue that if the objective of stepping up disclosure following a negative transparency shock is to improve the information environment, disclosure may have to be sustained for some time. Moreover, with sustained disclosure, the effect of the negative transparency shock on the "cost of capital wedge" between close and distant peers will eventually disappear. Accordingly, I propose the following dynamic behavior for disclosure and the cost of capital:

Hypothesis 3. (i) Close peers of the high-profile fraudulent firm will continue to provide more disclosure for several periods following the negative transparency shock, relative to distant peers. (ii) After increasing immediately after the negative transparency shock (Hypothesis 1), the cost of capital wedge between close and distant peers will gradually decrease.

My final hypothesis concerns financing choices of close and distant peers. A negative transparency shock creates more adverse selection, which is likely to affect the security issuance decisions of peer firms. In particular, information-based theories of financing choice (e.g., Myers and Majluf, 1984) suggest that because the valuation of equity is more sensitive to cash flow information than that of debt, the spillover impact of a negative transparency shock will be more severe on equity than on debt. Thus, one would expect close peers to favor debt financing more than equity financing immediately after the negative transparency shock, compared to distant peers. However, if, as per Hypothesis 3, continued disclosure eventually improves the information environment and brings down the wedge in the cost of equity capital between the close and distant peers, the preference for debt financing will no longer be present.

Hypothesis 4. (i) Close peers will be more likely to issue debt than equity than distant peers after the negative transparency shock to a high-profile firm in the industry. (ii) However, the effect will be manifest only in the initial years, and subsequently, there will be no relative preference for either type of financing.

2.4. Data

The data used in the analysis fall into five major categories: (1) financial misconduct, (2) I/B/E/S analyst estimates for implied cost of capital (ICC) calculations, (3) proxies for firm disclosure, (4) equity and debt issuance, and (5) common analysts and common ownership. I describe each data source in detail and outline the construction of the variables used in this paper.

2.4.1 Financial misconduct

There are four databases commonly used in studies of financial misconduct: (1) the Securities and Exchange Commission's (SEC) Accounting and Auditing Enforcement Releases (AAERs) compiled by the University of California, Berkeley's Centre for Financial Reporting and Management (CFRM), (2) the Government Accountability Office, (3) Audit Analytics, (4) the Stanford Securities Class Action Clearinghouse database of securities class action lawsuits. Karpoff et al. (2017) compare the economic importance of four features of the databases mentioned above and show how these features impact inferences of empirical applications. Karpoff et al. (2017) indicate that CFRM is the best source to identify a comprehensive list of intentional misreporting cases. My first data source is the CFRM database, developed by Dechow, Ge, Larson, and Sloan (2011). CFRM provides firm identifier and AAERs numbers that are useful to track corresponding SEC enforcement releases. To supplement the database, I download all the enforcement releases from the SEC website and identify enforcement actions for the violations of 13(b) provisions of the 1934 Securities Exchange Act and Code of Federal Regulations[16]:

I Section 13(b)(2)(A), which requires firms to make and keep books, records, accounts, which, in reasonable detail, accurately and fairly reflect the transactions and dispositions of the assets;

[16] Many researchers have used 13(b) data (e.g., Kedia and Rajgopal, 2011; Files, 2012; Kedia, Koh, and Rajgopal, 2015; Call, Martin, Sharp, and Wilde, 2018; Parsons, Sulaeman, and Titman, 2018; Masulis and Zhang, 2019).

II Section 13(b)(2)(B), which requires firms to devise and maintain a system of internal accounting controls sufficient to provide reasonable assurances; and

III Section 13(b)(5), which states that "No person shall knowingly circumvent or knowingly fail to implement a system of internal accounting controls or knowingly falsify any book, record, or account".

IV Rule 17 CFR 240.13b2-1, which states that "No person shall directly or indirectly, falsify or cause to be falsified, any book, record or account subject to section 13(b)(2)(A) of the Securities Exchange Act".

V Rule 17 CFR 240.13b2-2, which pertains to representations and conduct in connection with the preparation of required reports and documents.

I identify 670 SEC enforcement actions against violations of 13(b) rules from 1999 to 2015 and track these firms in Compustat. My research question requires identifying reasonably accurate initial revelation dates when investors learn about the firm's financial misconduct for the first time. Karpoff et al. (2017) suggest that AAERs are on average released 1,008 days after the first public revelation. Following the method proposed by Karpoff et al. (2017), I hand-collect trigger events that attract the regulator's attention and prompt informal inquiry and possibly a formal letter of an investigation by the SEC. Most of these trigger events are documented in the enforcement proceedings. I also search for the trigger events in firms' SEC filings and misconduct-related news in LexisNexis. The trigger events include accounting irregularities, internal reviews, restatements, earnings, and losses announced by a firm or the press, and revelations of regulatory actions.

I focus on financial misconduct committed by high-profile industry leaders, i.e., S&P 500 firms that were accused of 13(b) violations.[17] These firms were in the S&P 500 when their financial misconduct was revealed to the public for the first time. I exclude financial and utility firms. In total, I identify 47 high-profile financial misconducts across 26 industries (3-digit SIC code). To define prior and post revelation periods clearly, if there is more than one high-profile financial misconduct in one industry, I only include the date when the financial misconduct of the first firm becomes known to the public as the event date for that industry.

[17] Beatty, Liao, and Yu (2013) study the effect of Fortune 500 firms' frauds on industry peers' investment during the misconduct period.

The figure in Appendix 2.3 shows the time-clustering of high-profile misconduct events and the number of distinct 4-digit, 3-digit, and 2-digit industries affected each year that enter my regression sample. While there is an expected spike in 2002, there are high-profile misconduct cases each year from 1995 to 2007 (except for 1996 and 1997, when there was no high-profile misconduct). [18]

2.4.2 Implied cost of capital

I calculate the implied cost of capital (ICC) for each firm as the internal rate of return, which makes the current stock price of a firm equal to the present value of its forecasted free cash flows.[19] I compute the ICC for each firm on June 30 each year based on the methodology from Gebhardt and Swaminathan (2001), Pástor et al. (2008), Chava and Purnanandam (2010), and Chava (2014). They highlight the advantage of ICC that it does not depend on noisy realized returns (Elton, 1999) and a particular asset pricing model. I obtain accounting data from Compustat, analyst forecasts from I/B/E/S, risk-free rate from Kenneth French data library, and the growth rate of real GDP and implicit GDP deflator from the Bureau of Economic Analysis. The details of the ICC construction are given in Appendix 2.2.[20] The ICC used in the analysis is adjusted using the risk-free rate.

2.4.3 Financial disclosure

My first measure of corporate disclosure is the number of the management forecasts of earnings. The data is available on the First Call Company Issued Guidelines (CIG) database. Prior studies have documented stock price reactions to the public release of management forecasts of earnings (Ajinkya and Gift, 1984; Waymire, 1984; Baginski, Conrad, and Hassell, 1993). A more recent study by Beyer, Cohen, Lys, and Walther (2010) also shows that management forecasts account for a large fraction of firms' quarterly return variance. Also, Brown, Call, Clement, and Sharp (2015) reveal in their interviews with 365 sell-side analysts that management forecasts of earnings is a useful input to analysts' earnings forecasts and stock recommendations. Since management forecasts are voluntary and not subject to regulation,

[18] I collect enforcement releases up to 2015. There are usually a few years between misconduct revelation and the enforcement release. From the enforcement releases collected from the SEC website, I did not find any cases of high-profile financial misconduct revelation after 2007.
[19] What I estimate is the implied cost of equity, but following the literature, I use the terms cost of capital and cost of equity interchangeably.
[20] My ICC construction closely follows the methodology described by Chava (2014).

managers have the flexibility to strategically issue their forecasts (Bergman and Roychowdhury, 2008).

My sample of management forecast begins in 1998, due to the increased coverage of firms and press releases on the CIG database starting from that year (Chuk, Matsumoto, and Miller, 2013). I collect both quarterly and annual forecasts on earnings per share. My dependent variable, *FreqMF*, measures overall disclosure in any given year, i.e., the natural logarithm of one plus the number of management forecasts of earnings issued during a given year.

My second measure of corporate disclosure is the length of the Management Discussion and Analysis (MD&A) section. The Securities and Exchange Commission (SEC) requires all public firms to incorporate an MD&A section in their annual reports since 1980. The MD&A section delivers managers' assessment of a firm's fundamental areas, such as liquidity, capital resources, and operations, enabling investors to assess a firm's past and current performance and predict its future performance. Although MD&A is mandated, managers have the flexibility to decide the breadth and depth of their discussion.

The value relevance and usefulness of MD&A has been established by previous studies. Leuz and Schrand (2009), Feldman, Govindaraj, Livnat, and Segal (2010), and Brown and Tucker (2011) find that the stock market reacts to the changes in the MD&A section because it contains information about future cash flows. Li (2010) shows that the level of optimism in MD&As is positively associated with future earnings. Lo (2014) finds that when the U.S. banks become exposed to the emerging-market financial crisis, their U.S. borrowers increase the length of their MD&A section as they seek alternative capital sources.

To obtain the MD&A content, I first download all the 10-K fillings between 1996 and 2019 from SEC EDGAR.[21] Then I use python to extract the MD&A section of these filings by searching these documents for string variations of "Item 7. Management Discussion and Analysis". Following Brown and Tucker (2011) and Li (2010), I remove all the HTML tags (i.e., tables) from the MD&A. Finally, I construct my dependent variable, *LengthMDA*, as the natural logarithm of one plus the number of words in the MD&A section in 10-K filings.

My third measure of corporate disclosure is firms' stock return synchronicity. Stock returns reflect the arrival of new market-wide and firm-specific information. Thus, the degree to which

[21] Almost all companies have filed the 10-K electronically since 1996.

a stock co-moves with the market depends on the relative amount of market-wide and firm-specific information aggregated into the stock price. Stock prices of a transparent firm move in a relatively unsynchronized manner because the stock prices of that firm aggregate more firm-specific information. I closely follow Morck, Yeung, and Yu (2000) and Jin and Myers (2006) to calculate R^2 from the market model:

$$r_{it} = \alpha_i + \beta_i r_{m,t} + \epsilon_{it} \tag{2.1}$$

where r_{it} is the return on stock *i* in week *t* (Wednesday to Wednesday), $r_{m,t}$ is the U.S. market index return proxied by the value-weighted returns of all CRSP firms. I exclude stocks that have less than 30 weekly returns in a particular year in my sample. I measure a firm's stock market synchronicity in a year by estimating the R^2 of the regression in Eqn. (1) for that year. My disclosure variable proxy is an inverse measure of synchronicity, given by log $(1 - R^2)$.

2.4.4 Equity and debt issuance

Following Leary and Roberts (2014), I use net equity issuances and net debt issuances as dependent variables to measure firms' financing activities. My measure of equity issuance, *Equity issuance indicator*, is equal to one if the net equity issuance of a firm is higher than 3% of the lagged book value of assets, zero otherwise. Net equity issuance is defined as the sale of common and preferred stock minus the purchase of common and preferred stock divided by lagged total assets. My proxy of debt issuance, *Debt issuance indicator*, is a dummy variable that equals one if net debt issuance is greater than 3% of the book value of assets. Net debt issuance is calculated as changes in long-term debts plus changes in short-term debts scaled by lagged total assets. I confirm the robustness of my results by using a 2% or 1% cutoff for equity issuance and debt issuance.

2.4.5 Common analyst coverage and common ownership

I obtain analyst earnings forecasts and recommendation information from Institutional Brokers Estimate System (IBES) detail file and recommendation file. To find firm pairs with shared analyst coverage, following Gomes, Gopalan, Leary, and Marcet (2017) and Muslu et al. (2014), I consider an analyst as covering a firm in a year if that analyst makes at least one earnings forecast (i.e., one-year or two-year EPS forecast) or issues a stock recommendation. Then I identify two firms as *"connected"* if a common analyst covers both the fraudulent firm and a peer firm for at least two years prior to the revelation of misconduct.

I construct my common large shareholder measures as follows. For each quarter in my sample period, I obtain institutional ownership information from Thomson Reuters Institutional Holdings (13F). This database covers holdings of U.S. publicly traded firms by institutional investors who manage at least $100 million in assets. I define an institutional investor as a large shareholder if it holds more than 5% of a firm's outstanding stocks. To measure a firm's status of common ownership before the revelation of financial misconduct, I follow He and Huang (2017) and define a dummy variable, *Co-ownership*, equal to one if a peer firm and a fraudulent firm are simultaneously held by the same large shareholder in any of the four quarters in the year before the revelation of misconduct and zero otherwise.

2.5. Empirical Methodology and Results

In this section, I estimate the effect of the revelation of high-profile financial misconduct on peer firms' cost of capital, disclosure choice, and financing decisions. I first discuss my empirical methodology, followed by a presentation of the empirical results.

2.5.1 Methodology

I analyze the impact of industry leaders' financial misconduct on peer firms by employing a staggered difference-in-difference (DID) setting. The staggered DID approach is ideally suited for my study as revelations of financial misconduct are multiple treatment events that occur at different times (see Gormley and Matsa (2011)). Specifically, I compare close peer (treated) and distant peer (control) firms' behavior before and after each revelation of high-profile financial misconduct (a negative transparency shock). Treated firms are those that have stronger informational complementarity with the fraudulent firm, and I categorize these as firms that share the same 4-digit SIC code (alternatively, same 3-digit SIC code) with the high-profile fraudulent firms. Control firms are those that have weaker or no information complementarity with the fraudulent firm, and I categorize these as firms that share the same 2-digit SIC code with the high-profile fraudulent firms but have a different 3-digit SIC code.[22] The control group from the same 2-digit industry is desirable to properly control for the

[22] Some control firms appear multiple times in the sample if more than one 3-digit SIC industry with the same 2-digit SIC code are involved in financial misconduct by high-profile firms in different years. Firm-year observations are removed from the control group if they are also treated by other high-profile misconduct events (i.e., share the same 4-digit SIC code with another fraudulent firm involved in a contemporaneous misconduct event).

underlying economics (at the 2-digit level).[23] I first construct a cohort of control and treated firms starting three years prior (excluding revelation year) and extending to four years after the revelation of financial misconduct.[24] I then stack the data across cohorts (i.e., across all the revelations of high-profile financial misconduct) and estimate the following firm-level OLS regression:

$$Y_{ict} = \alpha_0 + \alpha_1 Peer_{ic} * Post_{ict} + Controls_{ict} + \theta_{tc} + \gamma_{ic} + \epsilon_{ict} \quad (2.2)$$

where Y_{ict} is one of several outcome variables of interest measured for firm *i* in year *t*, $Peer_{ic}$ is a dummy variable indicating whether firm *i* in cohort *c* is a peer firm in the same 4-digit industry (*Peer* = 1) as the fraudulent firm, or in the control group of 2-digit industry firms (*Peer* = 0). $Post_{ict}$ takes a value of 1 for any of the four years after the revelation of misconduct. ϵ_{ict} is an error term, and θ_{tc} and γ_{ic} are year-cohort fixed effects and firm-cohort fixed effects, respectively. Following Gormley and Matsa (2011), I include firm-cohort fixed effects to account for time-invariant firm characteristics and use year-cohort fixed effects to control for unobserved heterogeneity that varies across time. The coefficient of interest is α_1, which measures the changes in Y_{ict} following the revelation of industry leaders' financial misconduct for treated firms relative to control firms. I cluster the standard errors at the firm level. Financial firms, utility firms, conglomerates, and government entities are excluded.

Table 2.1, Panel A reports descriptive statistics for the outcome and control variables used in my regression sample. The mean and the median values for the implied cost of equity are 6.1% and 4.2%, respectively. These estimates are broadly in line with the literature. In Panel B, I compare the mean value for the peer firms and control firms in the three years prior to the revelation of financial misconduct. The groups display statistically insignificant differences along several observable dimensions, including size, institutional ownership, past one-year stock returns, earnings volatility, and the probability of reporting a loss. Peer firms disclose more than those in the control group prior to the revelation of misconduct. The mean *FreqMF* is 0.26 for the peer firms and 0.19 for the control group. For *LengthMDA*, the mean value is

[23] An alternative classification of close and distant peers could be based on the Hoberg and Phillips (2010) TNIC classifications based on the similarity of a firm's products and those of the fraudulent firm. However, since the industry grouping changes from year to year dynamically, this presents some problems for my empirical design. In my regressions, I control for the product similarity score of sample firms and examine whether firms that are closer in product space to the fraudulent firm experience larger changes in their cost of equity and disclosure.

[24] None of my results change if I restrict the post-event window to three years. The fourth year is included to capture more extended dynamics in the post-event period. My results also do not change if I consider a 4-year pre-event window.

8.47 and 8.30 for the peer firms and control group, respectively. My univariate tests show that such differences are statistically significant for the frequency of management forecast and the length of MD&A. Consistent with the notion that higher disclosure is associated with a lower cost of equity (before transparency shock spillover), I observe that peer firms have a significantly lower cost of equity (0.053) than the control group (0.059). In addition, on average, peer firms have higher net equity issuance and lower leverage, consistent with a lower cost of equity. Peer firms also have higher market-to-book ratios and operating performance.

[Insert Table 2.1 here]

There are two important issues that need to be addressed to validate a causal interpretation of my findings. First, a key requirement of a difference-in-difference analysis is that the outcome variables corresponding to the peer firms and control firms display a parallel trend before the negative transparency shock (Bertrand, Duflo, and Mullainathan, 2004), that is, the outcome variables for the treated and control groups should not begin to diverge prior to the shock. Second, it is possible that some common shocks (e.g., industry shocks at the 4-digit or 3-digit level) hit the fraudulent firms and the close peers exactly at the same time, and simultaneously trigger fraud by the high-profile firm and cause the cost of capital and disclosure to increase for the close industry peers of the fraud firm. In section 2.5.4, I take advantage of the fact that in most of the cases of fraud in my sample, the actual period during which fraud is committed precedes the year the fraud is revealed. I show that treated and control group outcome variables do not show any divergence when the fraud was actually being committed. This exercise is conducted for a subsample of firms where the high-profile fraud was initiated at least three years prior to its revelation, so that it is unlikely that the fraud was undertaken in anticipation of a common shock to close industry peers that would materialize four years later. In section 2.5.5, I directly examine, for the full sample, whether peer group and control groups' behavior in terms of cost of capital and disclosure start to diverge prior to the revelation of misconduct, and find no such evidence.

To further investigate how the impact of the revelations of financial misconduct varies with the intensity of information complementarity, I consider two finer measures of information complementarity with the fraudulent firm, namely, *co-coverage* and *co-ownership*, indicating whether a firm in the treated or control group has a common analyst or a common institutional shareholder, respectively, with the fraudulent firm. To analyze if there is any heterogeneous treatment effect, I augment the OLS regression above by interacting the *Peer*Post* with the information complementarity dummy (using their pretreatment values) and estimate the following regression specification:

$$Y_{ict} = \eta_0 + \eta_1 Peer_{ic} * Post_{ict} + \eta_2 * Peer_{ic} * Post_{ict} * Common_{ic} + \eta_3 * Post_{ict} * Common_{ic} + Controls_{ict} + \theta_{tc} + \gamma_{ic} + \epsilon_{ict} \quad (2.3)$$

In specification (2.3), *Common* is an indicator variable that denotes either the presence of a common analyst (*co-coverage*) or a common owner (*co-ownership*) with the fraudulent firm. Since both variables are indicator variables (measured prior to the transparency shock) and invariant over time, their interaction with *Peer* is absorbed by the firm-cohort fixed effects. The variable of interest is the triple interaction term *Peer*Post*Common* that indicates the differential effect of industry leaders' revelation of misconduct on Y_{ict} for firms with information complementarity in the treated close peer firms, compared to those for other firms.

It is possible that my measures of information complementarity also reflect the potential of strategic interaction between the fraudulent firm and peer firms. Specifically, a firm that belongs to the close peer group, or that is subject to co-coverage or co-ownership, could increase its disclosure to lower its cost of capital and/or influence product market outcomes when the major industry player is unable to respond while dealing with the fallout of the misconduct. To take such strategic motives into account, I add the Hoberg and Phillips (2010) product similarity score (*Score*) between the fraudulent firm and the sample firm, the interaction of the *Score* and *Peer*, the interaction of *Score* and *Post*, and the triple-interaction between *Score*, *Post*, and *Peer*, to the specifications in Eqn. (2.3). Similarly, I also identify common auditors and in robustness tests, include interactions with the common auditor dummy.[25]

2.5.2 Cost of capital and transparency shock spillover

In this section, I examine the relation between high-profile firms' financial misconduct and peer firms' cost of capital and explore if there is any cross-sectional heterogeneity. Table 2.2 reports the results on the spillover effect of the negative transparency shock on peer firms' cost of capital. In this table and all subsequent tables, I report four sets of results (four columns). The first two columns report results for specifications that drop all firm-level controls, to ensure

[25] The results with the common auditor dummy are not reported in my tables, but are available on request. None of the interactions are significant in any of my tests.

that the estimates are not affected by the potential endogeneity of control variables. The last two columns add several firm-level controls. Following Gebhardt and Swaminathan (2001), Pástor et al. (2008), Chava and Purnanandam (2010), and Chava (2014), I control size, market-to-book, leverage, past one-year stock return, and stock return volatility in the cost of capital regression. These firm characteristics are constructed from the quarterly Compustat database and are lagged by at least six months.[26] The variable definitions are given in Appendix 2.1. Standard errors are heteroscedasticity-adjusted in columns (1) and (3), and clustered by firm in columns (2) and (4).

[Insert Table 2.2 here]

Consistent with Hypothesis 1(i), I find that the coefficient on *Peer*Post* is positive and significant at least at the 10 percent level in all four columns. This provides evidence of a more positive relation between adverse transparency shock to industry leaders and the cost of capital for close peer firms than for distant ones. The economic magnitude is large – representing a 0.6 percentage point average increase relative to the control firms. This represents a 10 percent increase over the mean value of the cost of capital in the sample. As I shall see below in section 2.5.5, the effect mainly comes from an immediate increase in the cost of capital in the first two years after the revelation of misconduct, and then the effect is attenuated. In terms of the control variables, I find significant relationships between the cost of capital and some firm characteristics, including the market-to-book ratio, leverage ratio, and past stock returns, consistent with previous studies.

Next, I test whether the cost of capital increase subsequent to the transparency shock is increasing in the strength of information linkage between fraudulent firms and peer firms (Hypothesis 1(ii)). Tables 2.3 and 2.4 examine whether the treatment effect within peer firms is stronger when a peer firm is linked through shared analyst coverage or shared ownership with the fraudulent firm. The results are quite striking and in line with Hypothesis 1(ii). The coefficient of the triple-interaction term (*Peer*Post*Common*) is large and statistically significant (suggesting a larger than one percentage point increase in the cost of capital for the peer firms with a common analyst or a common owner). There is no significant increase in the cost of capital of peer firms that do not have a common analyst, suggesting that co-coverage

[26] Following the literature, the implied cost of equity is estimated as of June 30 each year, and the control variables (computed from the quarterly Compustat database) are lagged by at least six months for the implied cost of capital regressions. In other regressions, they are lagged by one year.

and the associated information complementarity drives the results in Table 2.2. While co-ownership is also associated with a large increase in the cost of capital of the peer firms, peer firms that do not have co-ownership also experience an increase in the cost of capital, although the effect here is smaller. I verify that these results are not due to a very large percentage of close peers having common analyst or common ownership links with the fraudulent firm.[27]

Finally, I note that the product similarity score (*Score*) between the sample firm and the fraudulent firm and its interactions with *Post* for the peer firms or the control firms are all insignificant. If product market rivalry were somehow driving my results, one should expect the cost of equity of rival firms (peer firms or, within a peer group, firms that are closer to the fraudulent firm in product space) to go down. However, I see no such effect, suggesting either the absence of such effects or a zero net effect. For firms that are closest in terms of information complementarity (i.e., the co-covered and co-owned firms), the effects are opposite of what product market advantage derived from an impaired industry leader would suggest, and are highly significant.

[Insert Table 2.3 and Table 2.4 here]

To verify the robustness of my findings, in an unreported table, I estimate the regression specification (2.2) with an alternative close peer group which comprises firms in the same 3-digit industry as the fraudulent firm. My results are very similar. The coefficient of *Peer*Post* is slightly lower and implies a 0.5 percentage point increase in the cost of capital of close peers relative to distant peers. In Tables OA2.2 and OA2.3, I interact *Peer*Post* with the *Co-coverage* and *Co-ownership* dummies, respectively. Results for the 3-digit peer group are similar to those discussed above for the 4-digit peer group. Generally, the treatment effects are smaller in magnitude for the 3-digit peers than for the case of the 4-digit peers, with or without co-coverage and co-ownership.

2.5.3 Disclosure and transparency shock spillover

The results presented so far indicate a positive association between industry leaders' financial misconduct and close peer firms' cost of capital. I next examine how firms' disclosure

[27] Common analyst links are present for 32% of 4-digit peers, 25% of 3-digit peers (excluding the same 4-digit peers), and 12% for 2-digit peers (excluding the same 3-digit peers). The corresponding percentages for co-ownership are 13% at 4-digit, 3-digit, and 2-digit levels.

decisions respond to the increase in the cost of capital after the negative transparency shocks (Hypothesis 2). I test Hypothesis 2(i) in Table 2.5.

Panels A, B, and C of Table 2.5 provide the estimation results of Eqn. (2.2) in which I adopt various measures of corporate disclosure. In Panel A of Table 2.5, the dependent variable, *Freq*MF, is the natural logarithm of one plus the number of management forecasts in a given year; in Panel B, *Length*MDA is the natural logarithm of one plus the number of words in the MD&A section of the 10-K filing, and in Panel C, my dependent variable is $log\ (1\text{-}R^2)$, where R^2 measures stock-return synchronicity. In all three panels, the coefficient of the interaction term *Peer*Post* is positive and significant. The economic impact of the transparency shock is about a 9 percent increase in disclosure when the latter is measured in terms of the frequency of management forecasts, and a 5 percent increase when disclosure is measured in terms of the length of the MD&A section and the amount of firm-specific information. These results demonstrate that adverse transparency shocks to industry leaders are associated with economically large increases in the corporate disclosure by close peers relative to distant peers.

[Insert Table 2.5 here]

I next examine the effects of information complementarities by showing how the existence of common analyst and common shareholders affect the association between transparency shock and firms' disclosure choices (Hypothesis 2(ii)). The regression results are presented in Table 2.6. For the frequency of management forecasts and the number of words in the MD&A section, I find that co-coverage and co-ownership between peer firms and high-profile fraudulent firms are significantly and positively associated with the amount of disclosure for the close peers subsequent to the adverse transparency shocks. My results are consistent with the view that the spillover effects of a negative transparency shock to industry leaders on peer firms' disclosure decisions are stronger when more information linkages exist between two firms. However, I find no such effect for $log\ (1\text{-}R^2)$, which reflects the amount of firm-specific information reflected in the stock price. One possible reason is that there is greater within-peer group spillover of the impact of news, which is reflected in stock prices, compared to other channels through which the transparency shock affects the firms' information environment.

[Insert Table 2.6 here]

Strategic considerations could be relevant for peer firms' disclosure strategy in response to the revelation of financial misconduct by the high-profile industry leader. For example, if a dominant industry player is impaired, rival firms could benefit by expanding production capacity and increasing market share. If external financing is needed for the expansion of production capacity, they could increase disclosure to lower the cost of capital. In Table 2.6, I find that the product similarity score (*Score*) between the sample firms and the fraudulent firm and its interactions with *Post* and *Post*Peer* are all insignificant. It is possible that the firms subject to co-coverage and co-ownership have the closest product market interactions with the fraudulent firm, so that the higher disclosure by such firms reflects such strategic motives. However, it is difficult to argue that strategic considerations should be completely absent from other product market peers. The fact that variation in the product similarity score does not capture any effect of increased disclosure incentives suggests that strategic considerations are unlikely to be important for the disclosure response of the peer firms. I also note that the results on co-coverage and co-ownership as the channels of transmission argue against litigation risk being a reason for the increase in disclosure following the high-profile fraud.

In an unreported table, I repeat the tests based on the 3-digit classification of close peers. One noticeable difference is that once I take into account common coverage, close peer firms at the 3-digit level without common coverage no longer issue more management forecasts compared to their 2-digit controls. Again, the treatment effects are smaller in magnitude than for the case of 4-digit peers, with or without co-coverage and co-ownership.

My results so far compare the effect of transparency shock spillovers to close peers and distant peers of the fraudulent firms. To recall, close peers are from the same 4-digit or 3-digit SIC industry as the fraudulent firm, while distant peers are from the same 2-digit industry. In Appendix 2.4, I show that the spillover effects already fade away and are no longer discernible when I compare firms in the same 1-digit industry as the fraudulent firm, with one group (the "treated" group) belonging to the same 2-digit SIC industry as the fraudulent firm, and the other group (the control group) belonging to a different 2-digit SIC industry. My difference-in-difference regressions, similar to those in Tables 2.2 and 2.5, find no evidence that the cost of equity or disclosure activities of the firms in the treated group are any different after the transparency shock compared to the firms in the control group.

Overall, there are two takeaways from the results reported so far. First, I find that both the cost of capital and disclosure increase for close peers of the high-profile fraudulent firm after the adverse transparency shock relative to distant peers. Such a positive association of disclosure and the cost of equity is consistent with the models of Clinch and Verrecchia (2015), and arguments in Larcker and Rusticus (2010) and Leuz and Schrand (2009), and empirical evidence in Leuz and Schrand (2009) and Balakrishnan et al. (2014). However, such evidence is in contrast to the usual negative association that follows from an exogenous change in disclosure, which is supposed to improve information transparency and lower the cost of capital. As I show in the next section, the relationship between disclosure and cost of capital in my setting is, in fact, more nuanced than what the results discussed so far might suggest. While I cannot establish a direct causal link, I find evidence that a commitment to more disclosure does lower the cost of capital, as the literature has typically assumed.

Second, my results suggest that co-coverage and co-ownership *among close product market peers* are extremely strong indicators of information complementarity, and these linkages identify the firms that are most affected by the adverse transparency shocks. These results thus build on recent findings on the significance of information complementarities among co-covered firms (Ali and Hirshleifer, 2020; Lee et al., 2016; Muslu et al., 2014; Israelsen, 2016), and the (more limited) empirical evidence on co-owned firms (Kacperczyk et al., 2005). However, even with co-coverage and co-ownership, I find that information complementarity is weak when firms are not close product market peers.[28] These findings should, therefore, be of interest to the extensive literature that is concerned with the spillover effects of disclosure regulation (Leuz and Wysocki, 2016).

2.5.4 Could common (industry) shocks explain our results?

For a causal interpretation of my results, it is important to show that (i) the outcome variables do not start to diverge before the revelation of the high-profile fraud, and (ii) common industry or other shocks do not simultaneously cause fraudulent behavior by the high-profile firm and directly affect the cost of equity and disclosure behavior of the close industry peers only. To address both issues, I take advantage of the fact that the period during which fraud is committed typically precedes the year of the fraud is revealed. If industry shocks induced both fraud by the high-profile firm and affected the cost of capital and disclosure of the close industry peers, I should find that the outcome variables for the close peers begin to diverge

[28] As noted, co-coverage and co-ownership are not associated with any spillover effects to the 2-digit peers (control firms). Moreover, spillover effects in general, and especially the effect of co-coverage and co-ownership, are weaker for 3-digit peers than for 4-digit peers.

from those of the distant peers when the fraud was committed. To further rule out the possibility that the fraud was not committed *in anticipation of* future industry conditions (that materialized at the time the fraud was revealed), I focus on a sample where the first reported year that fraud was committed (as per the SEC's Accounting and Auditing Enforcement Releases (AAERs)) is three years prior to the revelation of the fraud. Since the average duration of contractions from peak to trough in the U.S. over the last forty-five years has averaged only twelve months, it seems unlikely that the fraud firms were engaging in fraud in anticipation of changing industry conditions three years ahead of time. Using the year before the commencement of fraud by the high-profile firm as the reference year, I augment the regression specification in Eqn. (2.2) by adding the interaction of *Peer* and an indicator variable "*Before*", which takes a value of one for each of the three years prior to the revelation of fraud, and zero otherwise. To ensure that the year of fraud revelation does not overlap with a fraud year, I drop the revelation year from this regression, so that the variable *Post* is one for any of the four years after the revelation year, and zero otherwise. In Appendix 2.5, I report the regression results with the cost of equity and the three disclosure measures as my dependent variables. The coefficient of *Peer*Before* is insignificant in all regressions, but that of *Peer*Post* remains positive and significant.

2.5.5 The dynamics of cost of capital and disclosure

In this section, I conduct further tests to examine how the impact of the industry leaders' financial misconduct on treated firms varies over time (Hypothesis 3). I construct a dynamic difference-in-difference model by running the same OLS regression as Eqn. (2.2), adding an indicator variable for the year before the transparency shock, and splitting the dummy variable $Peer_{ic} * Post_{ict}$ by year:

$$Y_{ic,t+\tau} = \beta_0 + \sum_{\tau=-1,1,2,3,4} \beta_\tau Peer_{ic} * I_{ic,t+\tau} + Controls_{ic,t+\tau} + \theta_{t+\tau,c} + \gamma_{ic} + \epsilon_{ic,t+\tau} \qquad (2.4)$$

In specification (2.3), τ takes the values of -1, 1, 2, 3, and 4. The indicator variable $I_{ic,t+\tau}$ identifies one year before, and one, two, three, and four years after the event that occurs at date *t*. The coefficient β_{-1} tests, for the full sample, the internal validity of my DID approach that the behavior of the treated firms and control firms does not start to diverge before the occurrence of the financial misconduct event. The coefficient β_1, β_2, β_3, and β_4 capture how treated firms' behavior relative to control firms change dynamically in response to the revelations of the industry leaders' financial misconduct.

In Table 2.7, I examine the dynamic behavior of each of my disclosure measures, and in Table 2.8, I examine the dynamic behavior of the cost of capital. Consistent with Hypothesis 3(i), my three disclosure measures remain significantly positive for at least three years after the shock. For all three measures of corporate disclosure, the β coefficients show a monotonic increasing pattern, implying that disclosure commitment of close peers caused by major transparency shocks to high-profile firms could manifest over several years after the shock. This is particularly strong for the frequency of management forecasts as my disclosure variable – for example, the number of management forecasts is higher for the close peers by 6 percent in the year after the shock, and by 16 percent four years after the shock. The β coefficients for the number of words in the MD&A section increase from the first to the third year after the shock, and then attenuate somewhat in the fourth year. The β coefficients corresponding to the inverse measure of stock return synchronicity also show a similar pattern. Across all three disclosure measures, I find that the β coefficients corresponding to the year before the shock are small and statistically insignificant, thus suggesting that there is little evidence that diverging pre-shock trends could obfuscate my results.

[Insert Table 2.7 here]

In the face of this sustained increase in disclosure activity, the cost of capital shows interesting dynamics. As shown in Table 2.8, it increases significantly (by 0.9 and 1.3 percentage points, respectively), in the first two years after the transparency shock. However, in the third and fourth years after the shock, the difference between the close and distant peers disappears, consistent with Hypothesis 3(ii). The fact that the cost of capital and disclosure initially increase together is consistent with Clinch and Verrecchia's (2015) model, as well as the idea that, as the cost of capital increases in response to the adverse transparency shock, it is optimal for firms to change their disclosure policy by committing to more disclosure. The continued increase in the disclosure subsequent to the shock is consistent with such a change in disclosure policy. Although I cannot causally associate the eventual decrease in the cost of capital with the increase in disclosure, this finding is also consistent with the hypothesis of altered benefits of disclosure brought about by the adverse transparency shock.

[Insert Table 2.8 here]

I confirm similar results for the 3-digit classification of close peers. Consistent with earlier findings, the coefficients capturing the treatment effects are generally smaller in magnitude.

2.5.6 Transparency shocks and financing

So far, my results indicate that firms exposed to the spillover effects of a transparency shock face a higher cost of capital and commit to increasing disclosure. In this section, I focus on the impact of a transparency shock on financing choices. While the impact of information asymmetry on firms' financing choice has attracted a substantial amount of research over the last four decades, the evidence is still controversial. One of the most robust stylized facts, first noted by Rajan and Zingales (1995), is that smaller firms are much more reliant on equity issuance than are larger firms. This has been subsequently put forward as evidence that information asymmetry does not explain financing behavior (e.g., Frank and Goyal, 2003) since smaller firms are likely to be much more subject to information asymmetry than larger firms.

While I do not attempt to resolve the small firm financing puzzle,[29] my setting provides an opportunity to explore how an adverse shock to transparency and an increase in information asymmetry affects firms' financing behavior. The price of equity is more sensitive to information asymmetry than the price of debt (Myers and Majluf, 1984). Therefore, I should expect that there is a stronger adverse impact of the transparency shock on the cost of equity than on the cost of debt. Accordingly, as Hypothesis 4(i) maintains, for close peers, I should expect debt issuance to increase at the expense of equity issuance following the shock. In Panel A of Table 2.9, I define debt (equity) issuance to occur if net debt (equity) issuance exceeds 3 percent of the book value of assets.[30] I report results for a linear probability model, and the specification is similar to that in Eqn. (2.2). I find that there is a 3 percent decrease in the probability of equity issuance by close peers relative to the distant peers after the shock, which is largely offset by a corresponding increase in the probability of debt issuance, confirming that close peers are more likely to prefer debt issuance to equity issuance in response to the negative transparency shock than distant peers. In Panel B, I examine the dynamics of issuance activity, in a specification similar to Eqn. (2.4). Consistent with my earlier results that the adverse effect

[29] It has been suggested that the financing behavior of small firms could be affected by considerations of debt capacity, or the risk of losing valuable growth options due to default. One interesting argument is that since the cash flows of small firms are riskier, the adverse selection could be more about the second moment than the first moment of cash flows (Noe, 1988; Halov and Heider, 2011).

[30] My results are robust to alternative cut-offs of debt (equity) issuance, such as 2% and 1% cut-offs.

on the cost of capital is mitigated after the first two years (possibly in response to consistently higher disclosure), I find that there is no longer any significant difference in the financing behavior between close and distant peers after the second year.

[Insert Table 2.9 here]

I find similar findings for the 3-digit classification of close peers. Again, the treatment effects are similar but somewhat weaker. However, one difference is that the decrease in equity financing propensity is more gradual, in contrast to the 4-digit case where the decrease mainly shows up as significant in the second year after the revelation of financial misconduct.

2.6. Conclusions

The relationship between corporate disclosure and the cost of capital is a central issue in accounting and finance. There is growing recognition that the causal nature of this relationship is not straightforward, which poses challenges for empirically identifying any relationship. Exploiting revelations of financial misconduct by high-profile firms, I attempt to identify the consequences of such adverse transparency shocks for close industry peer firms. I show that the cost of capital of peer firms can increase when there is an adverse transparency shock, prompting more disclosure. However, while disclosure remains high in the next four years, the cost of capital reverts to pre-shock levels within three years after the shock. Thus, the equilibrium relationship between disclosure and the cost of capital can be either positive or negative, depending on the benefits and costs of disclosure.

My results also address the relatively underexplored issue of channels of disclosure spillover. I find that information complementarities between firms is an important determinant of the channel through which spillover occurs. Firms that are close industry peers of another firm are strong candidates for spillover. Within close peers, firms that are covered by the same analyst or owned by the same blockholder are the most exposed to the spillover effects of changes in each other's information environment.

Finally, I contribute to a contentious literature that asks whether a firm's information environment is a first-order determinant of its financing choices. My finding that adverse shocks to transparency are associated with firms shifting towards debt financing at the expense of equity financing is consistent with the idea that information asymmetry matters for the types of securities firms issue.

Appendix 2.1 Variable definitions

Variable	Definition	Sources
Dependent Variables		
Implied cost of capital	The internal rate of return, which makes the current share price equal to the present value of future cash flows. Please refer to Appendix B.	Compustat quarterly, IBES, Kenneth French Data Library, and BEA
Stock Return Synchronicity	R^2 calculated from the market model.	CRSP
FreqMF	Natural logarithm of one plus the number of management forecasts of earnings issued by a firm in a year.	First Call CIG
LengthMDA	Natural logarithm of one plus the number of words in MD&A section in 10-K filings of a firm in a year.	EDGAR
Equity issuance indicator	An indicator variable equal to one if the net equity issuance of a firm is higher than three percent of book value of assets. Net equity issuance is the sale of common and preferred stock minus the purchase of common and preferred stock scaled by lagged total assets.	Compustat
Debt issuance indicator	An indicator variable equal to one if net debt issuance is greater than three percent of book value of assets. Net debt issuance is changes in long-term debts plus changes in short-term debts divided by lagged total assets.	Compustat
Variables of Interest		
Peer	An indicator variable equal to one if a firm has the same 4-digit SIC code with the high-profile fraudulent firm.	AAER, EDGAR, LexisNexis, and SEC Enforcement Releases
Post	An indicator variable equal to one for the four years after the revelations of high-profile financial misconduct and zero for the three years prior to the revelations (excluding revelation year).	AAER, EDGAR, LexisNexis, and SEC Enforcement Releases
Size	Natural logarithm of total assets.	Compustat
Market-to-book	Market value of total assets to the book value of total assets.	Compustat
Leverage	Short-term debt plus long-term debt, divided by total assets.	Compustat
Stock return	A firm's past one-year stock returns.	CRSP
Stock return volatility	A firm's past one-year stock return volatility.	CRSP
β (Market Factor)	Beta estimated from the market model.	CRSP
Log (Age)	Natural logarithm of number of years since the inclusion in Compustat.	Compustat
Total volatility	Standard deviation of weekly returns in a year.	CRSP

Appendix 2.1—*Continued*

Roa	Operating income before depreciation over total assets	Compustat
Idiosyncratic ROA movement	The log of the sum of squared errors estimated from regressing a firm's ROA on the market ROA and the industry ROA. Both market ROA and industry ROA are value-weighted averages, excluding the estimated firm (See Durnev, Morck, and Yeung (2004)).	Compustat
Loss	An indicator variable equal to one if income before extraordinary items of a firm in a year is negative.	Compustat
Earnings Volatility	Standard deviation of ROA over the past ten years (at least five non-missing observations are required).	Compustat
Institutional Ownership	The percentage of total institutional ownership in a firm over a year.	Thomson Reuters 13F
Sales	The natural logarithm of net sales.	Compustat
Profitability	Earnings before interest divided by total assets.	Compustat
Tangibility	Property, plant, and equipment scaled by total assets.	Compustat
Investment	Capital expenditure scaled by lagged property, plant, and equipment.	Compustat
Z score	Altman's (1968) Z-score, calculated as 3.3 times Pre-tax Income plus net sales plus 1.4 times retained earnings plus 1.2 times working capital scaled by total assets plus 0.6 times market value of equity scaled by total debt.	Compustat
Connection	An indicator variable equal to one if a firm shares the same analyst with the high-profile fraudulent firm for at least two years before the revelation of financial misconduct.	IBES
Common Owner	An indicator variable equal to one if a firm shares the common institutional ownership with the high-profile fraudulent firm in any of the four quarters in the year before the revelation of financial misconduct.	Thomson Reuters 13F
Score	Natural logarithm of one plus the product similarity score between a firm and the high-profile fraudulent firm in the same TNIC2 (text-based network industry classifications) industry in a given year.	Hoberg and Phillips Data Library

Appendix 2.2 The methodology for constructing the implied cost of capital

I closely follow Gebhardt and Swaminathan (2001), Pástor, Sinha, and Swaminathan (2008), Chava and Purnanandam (2010), and Chava (2014) to construct the implied cost of capital (*ICC*). *ICC* is the internal rate of return, which makes the current share price equal to the present value of free cash flows. *FCFE* is the free cash flow to equity, and I forecast *FCFE* over a finite horizon (*T* = 15 years). The stock price is composed of two parts: one is the present value of *FCFE* up to the terminal year *t+T*, the other is the present value of *FCFE* beyond the terminal year. The *FCFE* of firm *i* in year *t+k* is

$$E_t(FCFE_{i,t+k}) = EPS_{i,t+k} \times (1 - b_{t+k}) \tag{4}$$

where $EPS_{i,t+k}$ and b_{t+k} are the forecast of a firm's earnings per share and its plowback ratio in year *t+k*. I obtain one-year and two-year consensus forecasts on earnings per share from I/B/E/S as proxies for EPS_1 and EPS_2, respectively. I calculate a firm's EPS_3 as the product of its EPS_2 and the long-term growth rate (*Ltg*) obtained from I/B/E/S.[31] I assign a value of 100% to firms with a growth rate larger than 100% and 2% to firms with a growth rate of less than 2%. I forecast *EPS* from year *t+4* to year *t+T+1* by mean reverting the earning growth rate g_{t+3} at year *t+3* to a steady long-term growth rate by year *t+T+2* with an exponential rate of decline. I assume the steady long-term growth rate of *EPS* to be the nominal GDP growth rate (*g*) as of the previous year, and it follows:

$$g_{i,t+k} = g_{i,t+3} \times e^{(k-3)\times g_{i,mean}} \tag{5}$$

$$g_{i,mean} = \frac{ln\left(\frac{g}{g_{i,t+3}}\right)}{T-1} \tag{6}$$

The *EPS* in year *t+k* is computed as the following:

[31] If only a subset of EPS_1, EPS_2, and Ltg are available, I try to fill the missing values from the available ones. For example, if only Ltg is missing, I estimate g_{t+3} $=EPS_2/EPS_1 - 1$. If only EPS_2 is missing, I estimate $EPS_2 = EPS_1 \times (1 + Ltg)$. If only EPS_1 is missing, I estimate $EPS_1 = EPS_2/(1 + Ltg)$. If both Ltg and EPS_2 are missing, I compute $Ltg = EPS_1/most\ recent\ realized\ earnings - 1$, then $EPS_2 = EPS_1 \times (1 + Ltg)$. If both Ltg and EPS_1 are missing, I compute $Ltg = EPS_2/most\ recent\ realized\ earnings - 1$, then $EPS_1 = EPS_2/(1 + Ltg)$. If both EPS_1 and EPS_2 are missing, I drop the observation.

$$EPS_{i,t+k} = EPS_{i,t+k-1} \times \left(1 + g_{i,t+k}\right) \tag{7}$$

Next, I compute the plowback ratio *b* as one minus the payout ratio. The payout ratio is the sum of dividends (*DVC*) and share repurchases (*PRSTKC*) minus new equity issuance (*SSTK*), divided by the net income (*IB*) if *IB* is positive. If payout ratio is missing, I set it to the median payout ratio of the industry (2-digit SIC code). I set the payout ratio to the industry median payout ratio if a firm's payout ratio is above 1 or below -0.5. For the first year *t+1*, I set the plowback ratio to the ratio calculated from the above procedure. Then, I calculate the plowback ratio for the remaining years by mean reverting it to a steady-state value at year *t+T+1*. In the steady state, I assume the growth rate of earnings (*g*) equals the return on new investment times the plowback ratio. I assume in the steady-state, the return on new investment equals the implied cost of capital ($r_{i,e}$). Therefore, the plowback ratio at year *t+k* is:

$$b_{i,t+k} = b_{i,t+k-1} - \frac{b_{i,t+1} - b_i}{T} \tag{8}$$

$$b_i = \frac{g}{r_{i,e}} \tag{9}$$

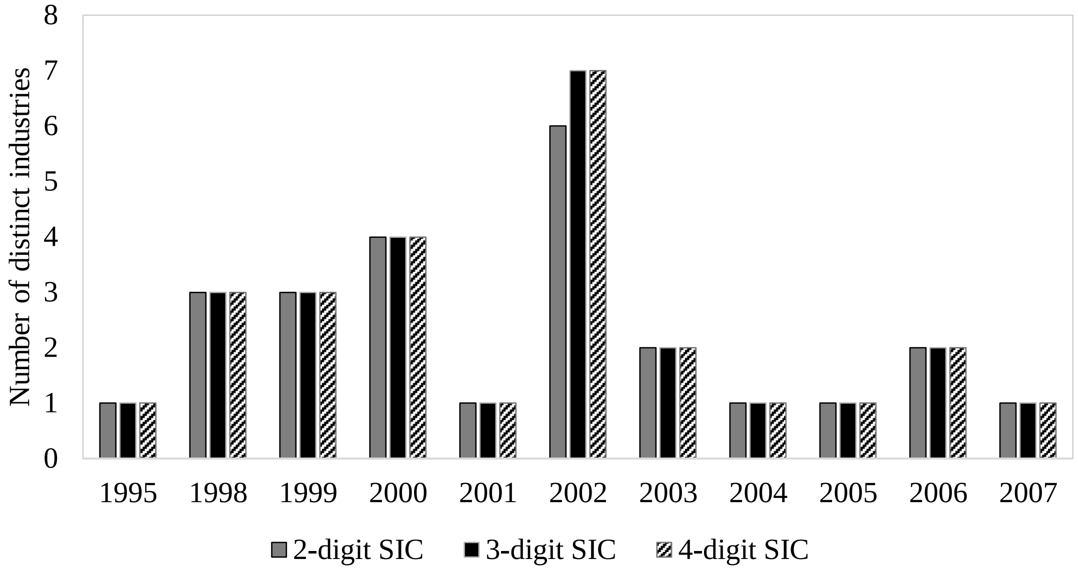
I compute the terminal value as the perpetuity:

$$TV_{i,t+T} = \frac{EPS_{i,t+T+1}}{r_{i,e}} \tag{10}$$

Then, I solve the following equation to get *ICC* (i.e., $r_{i,e}$):

$$P_{i,t} = \sum_{k=1}^{k=T} \frac{EPS_{i,t+k} \times (1 - b_{i,t+k})}{\left(1 + r_{i,e}\right)^k} - \frac{EPS_{i,t+T+1}}{r_{i,e}\left(1 + r_{i,e}\right)^T} \tag{11}$$

Appendix 2.3 Number of distinct industries associated with financial misconduct revelation of S&P 500 firms



This figure shows the time-clustering of high-profile financial misconduct events associated with high-profile firms, and the number of distinct 4-digit, 3-digit, and 2-digit industries affected each year that enter my regression sample.

Appendix 2.4 Cost of equity and disclosure decisions of distant industry peers

This table presents estimates of the effect of the revelation of high-profile financial misconduct on the implied cost of equity and disclosure decisions of distant industry peers of the fraudulent firm, in a difference-in-difference setting. A firm is defined as a high-profile fraudulent firm if it was an S&P 500 constituent when its misconduct was revealed. Peer (treated) firms share the same 2-digit SIC code, but a different 3-digit SIC code, with the high-profile fraudulent firm. For each peer industry, control firms are those in a different 1-digit industry as the peer-industry. If there is more than one misconduct event in the same 2-digit SIC industry, I only keep the first event. *Post* is equal to one for any of the four years after the revelation of misconduct and zero for any of the three years before the financial misconduct is revealed (excluding the year of misconduct revelation). Detailed variable definitions are in Appendix 2.1. In all specifications, firm-cohort and year-cohort fixed effects are included. *t*-statistics are reported in parentheses. In column (1) and (3), standard errors are adjusted for heteroskedasticity (White, 1980). In column (2) and (4), standard errors are clustered by firm. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	Panel A: Implied cost of equity			
	(1)	(2)	(3)	(4)
*Peer * Post*	0.0038	0.0038	0.0033	0.0033
	(1.10)	(1.04)	(0.95)	(0.90)
Observations	8,267	8,267	8,267	8,267
Adjusted R^2	0.695	0.695	0.697	0.697
Control Variables	No	No	Yes	Yes
Cohort*Firm FE	Yes	Yes	Yes	Yes
Cohort*Year FE	Yes	Yes	Yes	Yes
	Panel B: Management forecast (Dependent variable: *FreqMF*)			
	(1)	(2)	(3)	(4)
*Peer * Post*	0.0323	0.0323	0.0306	0.0306
	(1.04)	(0.83)	(1.00)	(0.81)
Observations	9,641	9,641	9,641	9,641
Adjusted R^2	0.705	0.705	0.712	0.712
Control Variables	No	No	Yes	Yes
Cohort*Firm FE	Yes	Yes	Yes	Yes
Cohort*Year FE	Yes	Yes	Yes	Yes
	Panel C: MD&A (Dependent variable: *LengthMDA*)			
	(1)	(2)	(3)	(4)
*Peer * Post*	-0.0075	-0.0075	-0.0081	-0.0081
	(-0.51)	(-0.40)	(-0.56)	(-0.45)
Observations	7,037	7,037	7,037	7,037
Adjusted R^2	0.841	0.841	0.848	0.848
Control Variables	No	No	Yes	Yes
Cohort*Firm FE	Yes	Yes	Yes	Yes
Cohort*Year FE	Yes	Yes	Yes	Yes

Appendix 2.4—*Continued*

	Panel D: Stock return synchronicity (Dependent variable: *log (1-R^2)*)			
	(1)	(2)	(3)	(4)
*Peer * Post*	0.0082	0.0082	0.0088	0.0088
	(1.16)	(1.12)	(1.50)	(1.47)
Observations	9,669	9,669	9,669	9,669
Adjusted R^2	0.602	0.602	0.684	0.684
Control Variables	No	No	Yes	Yes
Cohort*Firm FE	Yes	Yes	Yes	Yes
Cohort*Year FE	Yes	Yes	Yes	Yes

Appendix 2.5 "Fraud years", cost of equity, and disclosure

I select high-profile misconduct cases with a maximum of three years of misconduct prior to the revelation of misconduct. The reference year is the year before the start of the high-profile misconduct. *Post* is equal to one for any of the four years after the revelation of misconduct and zero for the years before the misconduct is revealed. *Before* is equal to one for the years before the revelation of misconduct (excluding the reference year) and zero otherwise. The dependent variables are implied cost of equity in column (1), the natural logarithm of one plus the number of management forecasts in a year in column (2), the logarithm of one plus the number of words in the MD&A section in 10-K filing in column (3), and log (1-R^2) where R^2 is stock return synchronicity in column (4). *Peer* equals one if a firm shares the same 4-digit SIC code with the high-profile fraudulent firms. *Peer* equals zero if a firm shares the same 2-digit SIC code, but a different 3-digit SIC code, with the high-profile fraudulent firm. Detailed variable definitions are in Appendix 2.1. Firm-cohort and year-cohort fixed effects are included. t-statistics are reported in parentheses. Standard errors are clustered by firm. ***, **, and * indicate significance at the 1%, 5% , and 10% level, respectively.

	(1)	(2)	(3)	(4)
	CoE	*FreqMF*	*LengthMDA*	*log (1-R^2)*
*Peer * Before*	-0.0018	-0.0121	-0.0159	0.0018
	(-0.38)	(-0.42)	(-0.64)	(0.22)
*Peer * Post*	0.0066**	0.0664***	0.0530**	0.0425***
	(2.00)	(2.81)	(2.29)	(4.19)
Observations	5,183	8,763	5,492	8,098
Adjusted R^2	0.642	0.610	0.794	0.645
Control Variables	Yes	Yes	Yes	Yes
Cohort*Firm FE	Yes	Yes	Yes	Yes
Cohort*Year FE	Yes	Yes	Yes	Yes

Table 2.1 Descriptive statistics

Panel A reports summary statistics for the outcome and control variables used in my empirical analysis. Detailed variable definitions are in Appendix 2.1. Panel B shows the univariate comparisons between peer and control firms prior to the revelation of high-profile financial misconduct. A firm is defined as a high-profile fraudulent firm if it was an S&P 500 constituent when its misconduct was revealed. In Panel B, Peer firms have the same 4-digit SIC code as the high-profile fraudulent firms. Control firms share the same 2-digit SIC code with the high-profile fraudulent firms but have a different 3-digit SIC code. The first two columns present the pre-treatment mean of the peer and the control group. The last column reports the mean difference, with *, **, and *** indicating significance at the 10%, 5%, and 1% level, respectively, from a mean difference test assuming unequal variance across two groups.

Panel A: Summary statistics

				Percentile		
Variables	Obs.	Mean	SD	25th	50th	75th
Cost of equity	11,110	0.0612	0.0860	0.0136	0.0421	0.0748
FreqMF	18,428	0.3649	0.7702	0.0000	0.0000	1.0094
LengthMDA	10,928	8.6108	0.6783	8.1455	8.6487	9.1084
R^2	17,266	0.1287	0.1372	0.0200	0.0785	0.1970
Net equity issuances	19,480	0.0490	0.2105	0.0000	0.0006	0.0134
Net debt issuances	19,480	0.0352	0.1745	-0.0226	0.0000	0.0388
Size	19,480	5.0566	1.9079	3.6332	4.8441	6.3213
Institutional ownership	19,480	0.4005	0.2884	0.1335	0.3642	0.6490
Market-to-book	19,480	2.3089	1.9847	1.1804	1.6591	2.6027
Leverage	19,480	0.2252	0.2694	0.0102	0.1494	0.3323
β	19,480	1.0419	0.8710	0.4404	0.9346	1.5432
Earnings volatility	19,480	0.2657	0.5849	0.0439	0.0949	0.2226
Stock return	19,480	0.0228	0.1917	-0.0794	0.0047	0.1000
Loss	19,480	0.3699	0.4827	0.0000	0.0000	1.0000
ROA	19,480	0.0590	0.2286	-0.0140	0.1031	0.1869

Panel B: Ex ante characteristics

Variables	Peer	Control	Difference
Cost of equity	0.0538	0.0597	-0.0059**
FreqMF	0.2589	0.1927	0.0661***
LengthMDA	8.4796	8.3071	0.1725***
R^2	0.1103	0.1081	0.0022
Net equity issuances	0.0775	0.0533	0.0242***
Net debt issuances	0.0437	0.0510	-0.0073
Firm size	4.7932	4.8496	-0.0564
Institutional ownership	0.3545	0.3452	0.0093
Market-to-book	2.8666	2.3367	0.5299***
Leverage	0.2200	0.2530	-0.0329***
β	1.0048	0.9726	0.0322
Earnings volatility	0.2635	0.2584	0.0051
Stock return	0.0367	0.0319	0.0048
Loss	0.3610	0.3494	0.0116
ROA	0.0693	0.0577	0.0115**

Table 2.2 Cost of equity

This table presents estimates of the effects of the revelation of high-profile financial misconduct on the implied cost of equity of close industry peers of the fraudulent firm (treated firms), in a difference-in-difference setting. A firm is defined as a high-profile fraudulent firm if it was an S&P 500 constituent when its misconduct was revealed. The dependent variable is the implied cost of equity and is constructed following Chava and Purnanandam (2010). *Peer* equals one if a firm shares the same 4-digit SIC code with the high-profile fraudulent firms. *Peer* equals zero if a firm shares the same 2-digit SIC code, but a different 3-digit SIC code, with the high-profile fraudulent firm. *Post* is equal to one for any of the four years after the revelation of misconduct and zero for any of the three years before the misconduct is revealed (excluding the year of misconduct revelation). Detailed variable definitions are in Appendix 2.1. In all specifications, firm-cohort and year-cohort fixed effects are included. *t*-statistics are reported in parentheses. In column (1) and (3), standard errors are adjusted for heteroskedasticity (White, 1980). In column (2) and (4), standard errors are clustered by firm. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	Dependent variable: Implied cost of equity			
	(1)	(2)	(3)	(4)
*Peer * Post*	0.0064**	0.0064*	0.0057*	0.0057*
	(2.07)	(1.83)	(1.84)	(1.72)
Size			0.0030	0.0030
			(1.56)	(1.42)
Market-to-book			-0.0012***	-0.0012***
			(-2.72)	(-2.86)
Leverage			0.0225***	0.0225***
			(3.36)	(3.02)
Stock return			-0.0141***	-0.0141***
			(-3.20)	(-3.13)
Stock return volatility			0.0072	0.0072
			(0.64)	(0.67)
Observations	11,110	11,110	11,110	11,110
Adjusted R^2	0.638	0.638	0.640	0.640
Cohort*Firm FE	Yes	Yes	Yes	Yes
Cohort*Year FE	Yes	Yes	Yes	Yes

Table 2.3 Cost of equity and common analysts

This table reports the coefficients from firm-panel regressions of the implied cost of equity on *Peer*Post* and its interactions with common analyst dummy (*Co-coverage*). *Co-coverage* is an indicator variable that takes on a value of one if a common analyst covers both the fraudulent firm and a peer firm for at least two years before the revelation of financial misconduct. *Peer* equals one if a firm shares the same 4-digit SIC code with the high-profile fraudulent firms. *Peer* equals zero if a firm shares the same 2-digit SIC code, but a different 3-digit SIC code, with the high-profile fraudulent firm. *Post* is equal to one for any of the four years after the revelation of misconduct and zero for any of the three years before the misconduct is revealed (excluding the year of misconduct revelation). Score measures product similarity between a firm and a fraudulent firm in the same TNIC2 industry in a given year (Hoberg and Phillips (2010, 2016)). Detailed variable definitions are in Appendix 2.1. In all specifications, firm-cohort and year-cohort fixed effects are included. *t*-statistics are reported in parentheses. In column (1) and (3), standard errors are adjusted for heteroskedasticity (White, 1980). In column (2) and (4), standard errors are clustered by firm. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	Dependent variable: Implied cost of equity			
	(1)	(2)	(3)	(4)
*Post * Co-coverage*	0.0030	0.0030	0.0030	0.0030
	(1.11)	(0.88)	(1.10)	(0.88)
*Peer * Post*	0.0022	0.0022	0.0025	0.0025
	(0.52)	(0.48)	(0.58)	(0.54)
*Peer * Post*	0.0127***	0.0127***	0.0113***	0.0113**
**Co-coverage*	(2.97)	(2.61)	(2.63)	(2.33)
Score	-0.1021	-0.1021	-0.1093	-0.1093
	(-1.43)	(-1.24)	(-1.54)	(-1.33)
*Post*Score*	0.0944	0.0944	0.0950	0.0950
	(1.23)	(1.22)	(1.25)	(1.23)
*Peer*Score*	0.0029	0.0029	-0.0076	-0.0076
	(0.05)	(0.04)	(-0.12)	(-0.11)
*Peer*Post*Score*	-0.1022	-0.1022	-0.1108	-0.1108
	(-1.08)	(-1.01)	(-1.18)	(-1.09)
Observations	11,110	11,110	11,110	11,110
Adjusted R^2	0.638	0.638	0.640	0.640
Control Variables	No	No	Yes	Yes
Cohort*Firm FE	Yes	Yes	Yes	Yes
Cohort*Year FE	Yes	Yes	Yes	Yes

Table 2.4 Cost of equity and common ownership

This table reports the coefficients from firm-panel regressions of the implied cost of equity on *Peer*Post* and its interactions with common ownership dummy (*Co-ownership*). *Co-ownership* equals one if a firm and a fraudulent firm in the same industry are held by the same large shareholder in the year before the revelation of financial misconduct. *Peer* equals one if a firm shares the same 4-digit SIC code with the high-profile fraudulent firms. *Peer* equals zero if a firm shares the same 2-digit SIC code, but a different 3-digit SIC code, with the high-profile fraudulent firm. *Post* is equal to one for any of the four years after the revelation of misconduct and zero for any of the three years before the misconduct is revealed (excluding the year of misconduct revelation). Score measures product similarity between a firm and a fraudulent firm in the same TNIC2 industry in a given year (Hoberg and Phillips (2010, 2016)). Detailed variable definitions are in Appendix 2.1. In all specifications, firm-cohort and year-cohort fixed effects are included. t-statistics are reported in parentheses. In column (1) and (3), standard errors are adjusted for heteroskedasticity (White, 1980). In column (2) and (4), standard errors are clustered by firm. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	Dependent variable: Implied cost of equity			
	(1)	(2)	(3)	(4)
*Post * Co-ownership*	0.0020	0.0020	0.0014	0.0014
	(0.64)	(0.49)	(0.45)	(0.34)
*Peer * Post*	0.0078*	0.0078*	0.0076*	0.0076*
	(1.90)	(1.71)	(1.85)	(1.67)
*Peer * Post*	0.0111***	0.0111**	0.0107***	0.0107***
**Co-ownership*	(2.73)	(2.58)	(2.61)	(2.61)
Score	-0.0911	-0.0911	-0.0912	-0.0912
	(-1.28)	(-1.11)	(-1.28)	(-1.13)
*Post*Score*	0.1014	0.1014	0.1018	0.1018
	(1.33)	(1.33)	(1.34)	(1.34)
*Peer*Score*	-0.0260	-0.0260	-0.0340	-0.0340
	(-0.42)	(-0.37)	(-0.55)	(-0.55)
*Peer*Post*Score*	-0.0627	-0.0627	-0.0763	-0.0763
	(-0.67)	(-0.64)	(-0.82)	(-0.82)
Observations	11,110	11,110	11,110	11,110
Adjusted R^2	0.638	0.638	0.640	0.640
Control Variables	No	No	Yes	Yes
Cohort*Firm FE	Yes	Yes	Yes	Yes
Cohort*Year FE	Yes	Yes	Yes	Yes

Table 2.5 Corporate disclosure

This table presents estimates of the effect of the revelation of high-profile financial misconduct on the disclosure decisions of close industry peers of the fraudulent firm, in a difference-in-difference setting. The dependent variable includes the natural logarithm of one plus the number of management forecasts in a year (Panel A), the logarithm of one plus the number of words in the MD&A section in 10-K filing (Panel B), and log (1-R^2) where R^2 is stock return synchronicity (Panel C). *Peer* equals one if a firm shares the same 4-digit SIC code with the high-profile fraudulent firms. *Peer* equals zero if a firm shares the same 2-digit SIC code, but a different 3-digit SIC code, with the high-profile fraudulent firm. *Post* is equal to one for any of the four years after the revelation of misconduct and zero for any of the three years before the misconduct is revealed (excluding the year of misconduct revelation). Detailed variable definitions are in Appendix 2.1. In all specifications, firm-cohort and year-cohort fixed effects are included. *t*-statistics are reported in parentheses. In column (1) and (3), standard errors are adjusted for heteroskedasticity (White, 1980). In column (2) and (4), standard errors are clustered by firm. ***, **, and * indicate significance at the 1%, 5% , and 10% level, respectively.

Panel A: Management forecast				
	(1)	(2)	(3)	(4)
	Dependent variable: *FreqMF*			
*Peer * Post*	0.0914***	0.0914***	0.0915***	0.0915***
	(5.53)	(3.31)	(7.43)	(3.38)
Observations	18,428	18,428	18,428	18,428
Adjusted R^2	0.629	0.629	0.640	0.640
Control Variables	No	No	Yes	Yes
Cohort*Firm FE	Yes	Yes	Yes	Yes
Cohort*Year FE	Yes	Yes	Yes	Yes
Panel B: MD&A				
	(1)	(2)	(3)	(4)
	Dependent variable: *LengthMDA*			
Peer Post*	0.0544***	0.0544**	0.0474***	0.0474**
	(3.26)	(2.27)	(2.90)	(2.05)
Observations	10,928	10,928	10,928	10,928
Adjusted R^2	0.813	0.813	0.820	0.820
Control Variables	No	No	Yes	Yes
Cohort*Firm FE	Yes	Yes	Yes	Yes
Cohort*Year FE	Yes	Yes	Yes	Yes
Panel C: Stock return synchronicity				
	(1)	(2)	(3)	(4)
	Dependent variable: *log (1-R^2)*			
Peer Post*	0.0594***	0.0594***	0.0433***	0.0433***
	(8.82)	(7.51)	(7.78)	(6.61)
Observations	17,266	17,266	17,266	17,266
Adjusted R^2	0.488	0.488	0.653	0.653
Control Variables	No	No	Yes	Yes
Cohort*Firm FE	Yes	Yes	Yes	Yes
Cohort*Year FE	Yes	Yes	Yes	Yes

Table 2.6 Corporate disclosure, common analyst, and common ownership

This table reports the coefficients from firm-panel regressions of the disclosure decisions on *Peer*Post* and its interactions with common analyst dummy (*Co-coverage*) and common ownership dummy (*Co-ownership*). *Co-coverage* is an indicator variable that takes on a value of one if a common analyst covers both the fraudulent firm and a peer firm for at least two years before the revelation of financial misconduct. *Co-ownership* equals one if a firm and a fraudulent firm in the same industry are held by the same large shareholder in the year before the revelation of misconduct. The dependent variable includes the natural logarithm of one plus the number of management forecasts in a year (Panel A), logarithm of one plus the number of words in the MD&A section in 10-K filing (Panel B), and log (1-R^2) where R^2 is stock return synchronicity (Panel C). *Peer* equals one if a firm shares the same 4-digit SIC code with the high-profile fraudulent firms. *Peer* equals zero if a firm shares the same 2-digit SIC code, but a different 3-digit SIC code, with the high-profile fraudulent firm. *Post* is equal to one for any of the four years after the revelation of misconduct and zero for any of the three years before the misconduct is revealed (excluding the year of misconduct revelation). Score measures product similarity between a firm and a fraudulent firm in the same TNIC2 industry in a given year (Hoberg and Phillips (2010, 2016)). Detailed variable definitions are in Appendix 2.1. In all specifications, firm-cohort and year-cohort fixed effects are included. *t*-statistics are reported in parentheses. In column (1), (3), (5), and (7), standard errors are adjusted for heteroskedasticity (White, 1980). In column (2), (4), (6), and (8), standard errors are clustered by firm. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Table 2.6—*Continued*

Panel A: Management forecast (Dependent variable: *FreqMF*)								
	Co-coverage				*Co-ownership*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
*Post * Common*	0.0017	0.0017	0.0031	0.0031	0.0017	0.0017	0.0015	0.0015
	(0.04)	(0.03)	(0.08)	(0.06)	(0.05)	(0.05)	(0.05)	(0.04)
*Peer * Post*	0.0337**	0.0337*	0.0341**	0.0341*	0.0517***	0.0517**	0.0573***	0.0573**
	(1.97)	(1.69)	(2.00)	(1.69)	(2.95)	(2.04)	(3.05)	(2.19)
*Peer * Post*	0.1135***	0.1135**	0.1114***	0.1114**	0.2098***	0.2098***	0.1860***	0.1860***
**Common*	(3.94)	(2.28)	(3.95)	(2.27)	(3.76)	(3.54)	(3.53)	(3.15)
Score	-0.1418	-0.1418	0.1089	0.1089	0.0252	0.0252	0.2187	0.2187
	(-0.16)	(-0.13)	(0.12)	(0.10)	(0.03)	(0.02)	(0.27)	(0.20)
*Post*Score*	1.8572**	1.8572	1.1722	1.1722	1.9142**	1.9142	1.2296	1.2296
	(2.24)	(1.51)	(1.46)	(0.98)	(2.28)	(1.55)	(1.58)	(1.03)
*Peer*Score*	1.3224	1.3224	0.7570	0.7570	1.0821	1.0821	0.5305	0.5305
	(1.61)	(1.27)	(0.90)	(0.60)	(1.27)	(0.86)	(0.61)	(0.42)
*Peer*Post*Score*	0.2364	0.2364	0.7523	0.7523	0.4373	0.4373	0.9678	0.9678
	(0.37)	(0.17)	(1.11)	(0.56)	(0.63)	(0.32)	(1.36)	(0.73)
Observations	18,428	18,428	18,428	18,428	18,428	18,428	18,428	18,428
Adjusted R^2	0.632	0.632	0.643	0.643	0.632	0.632	0.643	0.643
Control Variables	No	No	Yes	Yes	No	No	Yes	Yes
Cohort*Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 2.6—*Continued*

Panel B: MD&A (Dependent variable: *LengthMDA*)								
	Co-coverage				*Co-ownership*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
*Post * Common*	-0.0069	-0.0069	-0.0064	-0.0064	0.0031	0.0031	-0.0033	-0.0033
	(-0.14)	(-0.11)	(-0.13)	(-0.10)	(0.10)	(0.08)	(-0.11)	(-0.09)
*Peer * Post*	0.0377**	0.0377*	0.0320**	0.0320	0.0521***	0.0521**	0.0403**	0.0403*
	(2.12)	(1.70)	(2.01)	(1.60)	(2.60)	(2.04)	(2.06)	(1.69)
*Peer * Post*	0.0924***	0.0924**	0.0807**	0.0807*	0.0528**	0.0528*	0.0519**	0.0519*
**Common*	(2.86)	(2.07)	(2.56)	(1.86)	(2.06)	(1.71)	(2.02)	(1.66)
Score	0.8123	0.8123	0.7473	0.7473	0.8112	0.8112	0.7412	0.7412
	(1.31)	(1.06)	(1.23)	(1.01)	(1.31)	(1.05)	(1.22)	(1.00)
*Post*Score*	-0.3235	-0.3235	-0.4760	-0.4760	-0.3207	-0.3207	-0.4691	-0.4691
	(-0.56)	(-0.40)	(-0.85)	(-0.60)	(-0.56)	(-0.39)	(-0.83)	(-0.59)
*Peer*Score*	-0.2321	-0.2321	-0.4474	-0.4474	-0.4366	-0.4366	-0.6213	-0.6213
	(-0.32)	(-0.26)	(-0.63)	(-0.52)	(-0.59)	(-0.48)	(-0.87)	(-0.72)
*Peer*Post*Score*	0.6502	0.6502	0.8829	0.8829	1.0338	1.0338	1.2315*	1.2315
	(0.94)	(0.68)	(1.30)	(0.96)	(1.50)	(1.09)	(1.82)	(1.34)
Observations	10,928	10,928	10,928	10,928	10,928	10,928	10,928	10,928
Adjusted R^2	0.815	0.815	0.822	0.822	0.815	0.815	0.822	0.822
Control Variables	No	No	Yes	Yes	No	No	Yes	Yes
Cohort*Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 2.6—*Continued*

Panel C: Stock return synchronicity (Dependent variable: *log (1-R^2)*)								
	Co-coverage				*Co-ownership*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
*Post * Common*	-0.0038	-0.0038	-0.0038	-0.0038	-0.0059	-0.0059	-0.0048	-0.0048
	(-0.32)	(-0.30)	(-0.38)	(-0.37)	(-0.59)	(-0.52)	(-0.58)	(-0.51)
*Peer * Post*	0.0603***	0.0603***	0.0441***	0.0441***	0.0609***	0.0609***	0.0426***	0.0426***
	(8.19)	(7.15)	(7.05)	(6.10)	(8.14)	(7.04)	(6.74)	(5.74)
*Peer * Post*	0.0114	0.0114	0.0001	0.0001	0.0142	0.0142	0.0125	0.0125
**Common*	(0.93)	(0.81)	(0.01)	(0.01)	(0.91)	(0.86)	(0.99)	(0.92)
Score	0.3475	0.3475	0.2579	0.2579	0.3428	0.3428	0.2565	0.2565
	(1.48)	(1.34)	(1.27)	(1.16)	(1.46)	(1.32)	(1.27)	(1.16)
*Post*Score*	0.0397	0.0397	0.0635	0.0635	0.0438	0.0438	0.0657	0.0657
	(0.15)	(0.15)	(0.29)	(0.29)	(0.17)	(0.17)	(0.30)	(0.30)
*Peer*Score*	0.0411	0.0411	0.0974	0.0974	0.0239	0.0239	0.0975	0.0975
	(0.14)	(0.13)	(0.40)	(0.36)	(0.08)	(0.08)	(0.40)	(0.30)
*Peer*Post*Score*	-0.3082	-0.3082	-0.2165	-0.2165	-0.2813	-0.2813	-0.2267	-0.2267
	(-1.03)	(-0.97)	(-0.84)	(-0.79)	(-0.94)	(-0.88)	(-0.88)	(-0.83)
Observations	17,266	17,266	17,266	17,266	17,266	17,266	17,266	17,266
Adjusted R^2	0.488	0.488	0.653	0.653	0.488	0.488	0.653	0.653
Control Variables	No	No	Yes	Yes	No	No	Yes	Yes
Cohort*Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 2.7 Corporate disclosure dynamics

This table reports the effect of the revelation of high-profile fraudulent firms' financial misconduct on the disclosure decisions of close industry peers of the fraudulent firm, in a difference-in-difference setting. The dependent variable includes the natural logarithm of one plus the number of management forecasts in a year (Panel A), logarithm of one plus the number of words in the MD&A section in 10-K filing (Panel B), and log (1-R^2) where R^2 is stock return synchronicity (Panel C). *Peer* equals one if a firm shares the same 4-digit SIC code with the high-profile fraudulent firms. *Peer* equals zero if a firm shares the same 2-digit SIC code, but a different 3-digit SIC code, with the high-profile fraudulent firm. $Year_t$ is the year of misconduct revelation. Detailed variable definitions are in Appendix 2.1. In all specifications, firm-cohort and year-cohort fixed effects are included. *t*-statistics are reported in parentheses. In column (1) and (3), standard errors are adjusted for heteroskedasticity (White, 1980). In column (2) and (4), standard errors are clustered by firm. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: Management forecast (Dependent variable: *FreqMF*)

	(1)	(2)	(3)	(4)
$Peer * Year_{t-1} = 1$	0.0088	0.0088	0.0120	0.0120
	(0.93)	(0.26)	(1.07)	(0.36)
$Peer * Year_{t+1} = 1$	0.0666***	0.0666*	0.0684***	0.0684*
	(3.71)	(1.82)	(3.19)	(1.90)
$Peer * Year_{t+2} = 1$	0.1031***	0.1031***	0.1041***	0.1041***
	(5.10)	(2.63)	(7.55)	(2.69)
$Peer * Year_{t+3} = 1$	0.1258***	0.1258***	0.1328***	0.1328***
	(4.88)	(3.05)	(6.74)	(3.27)
$Peer * Year_{t+4} = 1$	0.1640***	0.1640***	0.1581***	0.1581***
	(4.30)	(3.21)	(5.08)	(3.17)
Size			0.0941***	0.0941***
			(10.94)	(7.17)
Market-to-book			-0.0219***	-0.0219***
			(-8.29)	(-5.89)
Loss			-0.0338**	-0.0338***
			(-2.06)	(-2.66)
Roa			0.0567***	0.0567**
			(4.19)	(1.97)
Earnings volatility			0.0607***	0.0607**
			(5.72)	(2.35)
Stock return			0.0152***	0.0152**
			(3.50)	(2.55)
Institutional ownership			0.5088***	0.5088***
			(16.31)	(9.23)
Observations	18,428	18,428	18,428	18,428
Adjusted R^2	0.629	0.629	0.640	0.640
Cohort*Firm FE	Yes	Yes	Yes	Yes
Cohort*Year FE	Yes	Yes	Yes	Yes

Table 2.7—*Continued*

Panel B: MD&A (Dependent variable: *LengthMDA*)				
	(1)	(2)	(3)	(4)
$Peer * Year_{t-1} = 1$	0.0247	0.0247	0.0116	0.0116
	(1.25)	(1.33)	(0.61)	(0.64)
$Peer * Year_{t+1} = 1$	0.0714***	0.0714***	0.0501**	0.0501*
	(3.32)	(2.64)	(2.39)	(1.91)
$Peer * Year_{t+2} = 1$	0.0709***	0.0709**	0.0608***	0.0608**
	(3.15)	(2.40)	(2.77)	(2.12)
$Peer * Year_{t+3} = 1$	0.0569**	0.0569*	0.0530**	0.0530
	(2.24)	(1.68)	(2.13)	(1.61)
$Peer * Year_{t+4} = 1$	0.0495*	0.0495	0.0444	0.0444
	(1.76)	(1.34)	(1.63)	(1.25)
Size			0.1226***	0.1226***
			(11.39)	(9.24)
Market-to-book			-0.0006	-0.0006
			(-0.25)	(-0.23)
Loss			0.0247***	0.0247**
			(2.65)	(2.58)
Roa			-0.2317***	-0.2317***
			(-10.40)	(-8.98)
Earnings volatility			0.0383**	0.0383**
			(2.46)	(1.97)
Stock return			-0.0091**	-0.0091***
			(-2.45)	(-2.64)
Institutional ownership			-0.0062	-0.0062
			(-0.17)	(-0.13)
Observations	10,928	10,928	10,928	10,928
Adjusted R^2	0.813	0.813	0.820	0.820
Cohort*Firm FE	Yes	Yes	Yes	Yes
Cohort*Year FE	Yes	Yes	Yes	Yes

Table 2.7—*Continued*

Panel C: Stock return synchronicity (Dependent variable: *log (1-R^2)*)				
	(1)	(2)	(3)	(4)
$Peer * Year_{t-1} = 1$	-0.0021	-0.0021	-0.0017	-0.0017
	(-0.25)	(-0.27)	(-0.25)	(-0.26)
$Peer * Year_{t+1} = 1$	0.0270***	0.0270**	0.0214***	0.0214**
	(2.80)	(2.58)	(2.72)	(2.48)
$Peer * Year_{t+2} = 1$	0.0620***	0.0620***	0.0348***	0.0348***
	(6.46)	(5.90)	(4.33)	(3.88)
$Peer * Year_{t+3} = 1$	0.0788***	0.0788***	0.0625***	0.0625***
	(8.38)	(7.59)	(8.06)	(7.29)
$Peer * Year_{t+4} = 1$	0.0820***	0.0820***	0.0647***	0.0647***
	(7.65)	(7.27)	(7.54)	(7.17)
Size			-0.0227***	-0.0227***
			(-8.80)	(-7.97)
Market-to-book			0.0002	0.0002
			(0.37)	(0.37)
Leverage			0.0531***	0.0531***
			(4.78)	(4.53)
β			-0.1179***	-0.1179***
			(-51.67)	(-42.52)
Age			0.0133***	0.0133**
			(2.59)	(2.50)
Log of total volatility			0.0743***	0.0743***
			(17.56)	(15.77)
Idiosyncratic ROA movement			-0.0026***	-0.0026***
			(-3.54)	(-3.19)
Observations	17,266	17,266	17,266	17,266
Adjusted R^2	0.489	0.489	0.654	0.654
Cohort*Firm FE	Yes	Yes	Yes	Yes
Cohort*Year FE	Yes	Yes	Yes	Yes

Table 2.8 Implied cost of equity dynamics

This table presents the estimates of the effects of the revelation of high-profile financial misconduct on the implied cost of equity of close industry peers of the fraudulent firms, in a difference-in-difference setting. The dependent variable is the implied cost of equity and is constructed following Chava and Purnanandam (2010). *Peer* equals one if a firm shares the same 4-digit SIC code with the high-profile fraudulent firms. *Peer* equals zero if a firm shares the same 2-digit SIC code, but a different 3-digit SIC code, with the high-profile fraudulent firm. $Year_t$ is the year of misconduct revelation. Detailed variable definitions are in Appendix 2.1. In all specifications, firm-cohort and year-cohort fixed effects are included. *t*-statistics are reported in parentheses. In column (1) and (3), standard errors are adjusted for heteroskedasticity (White, 1980). In column (2) and (4), standard errors are clustered by firm. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	Dependent variable: Implied cost of equity			
	(1)	(2)	(3)	(4)
$Peer * Year_{t-1} = 1$	-0.0011	-0.0011	-0.0011	-0.0011
	(-0.27)	(-0.26)	(-0.26)	(-0.26)
$Peer * Year_{t+1} = 1$	0.0092**	0.0092**	0.0088**	0.0088**
	(2.27)	(2.18)	(2.18)	(2.08)
$Peer * Year_{t+2} = 1$	0.0128***	0.0128***	0.0120***	0.0120***
	(3.08)	(2.84)	(2.88)	(2.66)
$Peer * Year_{t+3} = 1$	0.0019	0.0019	0.0014	0.0014
	(0.47)	(0.42)	(0.35)	(0.31)
$Peer * Year_{t+4} = 1$	-0.0010	-0.0010	-0.0022	-0.0022
	(-0.23)	(-0.20)	(-0.50)	(-0.45)
Size			0.0031	0.0031
			(1.60)	(1.45)
Market-to-book			-0.0012***	-0.0012***
			(-2.71)	(-2.84)
Leverage			0.0226***	0.0226***
			(3.37)	(3.03)
Stock return			-0.0142***	-0.0142***
			(-3.20)	(-3.13)
Stock return volatility			0.0061	0.0061
			(0.54)	(0.57)
Observations	11,110	11,110	11,110	11,110
Adjusted R^2	0.638	0.638	0.640	0.640
Cohort*Firm FE	Yes	Yes	Yes	Yes
Cohort*Year FE	Yes	Yes	Yes	Yes

Table 2.9 Firms' financing decisions

The table presents the estimates of the effects of the revelation of high-profile financial misconduct on the equity and debt issuance of close industry peers of the fraudulent firms, in a difference-in-difference setting. In Column (1) and (2), the dependent variable is one if net equity issuance is greater than three percent of book value of assets. In Column (3) and (4), the dependent variable is one if net debt issuance is greater than three percent of book value of assets. *Peer* equals one if a firm shares the same 4-digit SIC code with the high-profile fraudulent firms. *Peer* equals zero if a firm shares the same 2-digit SIC code, but a different 3-digit SIC code, with the high-profile fraudulent firm. In Panel A, *Post* is equal to one for any of the four years after the revelation of misconduct and zero for any of the three years before the misconduct is revealed (excluding the year of misconduct revelation). In Panel B, $Year_t$ is the year of misconduct revelation. In all specifications, firm-cohort and year-cohort fixed effects are included. t-statistics are reported in parentheses. Standard errors are clustered by firm. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	Panel A: Equity and debt issuance			
	(1)	(2)	(3)	(4)
	Equity issuance indicator	*Equity issuance indicator*	*Debt issuance indicator*	*Debt issuance indicator*
*Peer * Post*	-0.0410**	-0.0352*	0.0414**	0.0389**
	(-2.13)	(-1.84)	(2.11)	(2.02)
Sales		-0.0016		0.0660***
		(-0.17)		(6.40)
Market-to-Book		0.0519***		-0.0103***
		(17.00)		(-3.64)
Profitability		0.0227		-0.1894***
		(0.60)		(-5.33)
Tangibility		-0.3296***		0.0692
		(-5.25)		(1.07)
Investment		0.0764***		0.1883***
		(7.68)		(18.08)
Z score		-0.0005		-0.0155***
		(-0.16)		(-5.73)
Observations	19,480	19,480	19,480	19,480
Adjusted R^2	0.354	0.380	0.120	0.150
Cohort*Firm FE	Yes	Yes	Yes	Yes
Cohort*Year FE	Yes	Yes	Yes	Yes

Table 2.9—*Continued*

	Panel B: Dynamics of equity and debt issuance			
	(1) *Equity issuance indicator*	(2) *Equity issuance indicator*	(3) *Debt issuance indicator*	(4) *Debt issuance indicator*
$Peer * Year_{t-1} = 1$	-0.0009	-0.0017	0.0218	0.0203
	(-0.04)	(-0.08)	(0.84)	(0.79)
$Peer * Year_{t+1} = 1$	-0.0302	-0.0249	0.0569**	0.0490*
	(-1.18)	(-0.98)	(2.20)	(1.93)
$Peer * Year_{t+2} = 1$	-0.0789***	-0.0716***	0.0652**	0.0608**
	(-2.99)	(-2.73)	(2.48)	(2.35)
$Peer * Year_{t+3} = 1$	-0.0381	-0.0332	0.0395	0.0443*
	(-1.39)	(-1.22)	(1.51)	(1.74)
$Peer * Year_{t+4} = 1$	-0.0130	-0.0009	0.0277	0.0219
	(-0.47)	(-0.34)	(0.95)	(0.77)
Sales		-0.0016		0.0656***
		(-0.17)		(6.36)
Market-to-Book		0.0518***		-0.0119***
		(16.97)		(-4.45)
Profitability		0.0230		-0.1858**
		(0.61)		(-5.23)
Tangibility		-0.3284***		0.0664
		(-5.23)		(1.03)
Investment		0.0763***		0.1891***
		(7.68)		(18.12)
Z score		-0.0005		-0.1591***
		(-0.16)		(-5.81)
Observations	19,480	19,480	19,480	19,480
Adjusted R^2	0.354	0.380	0.120	0.150
Cohort*Firm FE	Yes	Yes	Yes	Yes
Cohort*Year FE	Yes	Yes	Yes	Yes

Chapter 3

Innovation, Exploration, and Survival: The Effect of Customer Fraud on Suppliers

3.1. Introduction

A firm's success in the competitive product market crucially depends on its stakeholder relationships. For example, suppliers often make non-transferable investments to meet their customers' specific requirements, and such relationship-specific investments are most common in the development of technology and products designed to exclusively serve the needs of the customer. Supplier innovation is of first-order importance for most firms, and customer-specific innovation by upstream firms is increasingly becoming the norm (Huston and Sakkab, 2006; Henke and Zhang, 2010). Suppliers, in turn, benefit from the knowledge transfer and are able to cement longer-term relationships (Chu, Tian, and Wang, 2019; Chen, Dasgupta, Huynh and Xia, 2020).

While the benefits of relationship-specificity and long-term relationships are well understood, the costs are less well-documented. Unlike in Japanese manufacturing, customer-supplier relationships are not very long-term for U.S. firms – lasting between five-to-seven years on average.[32] For supplier firms that are younger and much smaller in size than their customers, securing a contract from a large customer, engaging in knowledge-sharing, and sustaining the relationship for a number of years may confer considerable benefits.[33] However, relationship-specificity also subjects the suppliers to hold-up problems and customer opportunism (Williamson, 1975, 1979, 1985; Klein, Crawford, and Alchian, 1978). One aspect of this is that the customer could impose formal or informal restrictions on the type of innovation the supplier is able to do, or how it is allowed to use the knowledge that is transferred. The supplier management, concerned about losing the customer if the supplier does not do enough relationship-specific innovation, is likely to prioritize such innovation over a

[32] Cen, Dasgupta, and Sen (2016) identify customer-supplier relationships based on Compustat segment files and report that the mean (median) relationship duration is 5.6 (4.9) years. Costello (2013) extracts contracts that have to be filed by a contracting party with the SEC (as per Section 10(ii)(b) of Regulation S-K) if they are material for its business. Her random sample of 1,500 contracts (among 5,000) for the period January 1996 to May 2010 exhibits a mean contract duration of 6.85 years.

[33] For example, Cen, Dasgupta, Elkamhi, and Pungaliya (2016) show that the certification effect of establishing a long-term relationship with a principal customer leads to better loan contract terms for the supplier.

more diversified innovation strategy. As a result, "exploitative" innovation that is more incremental and directly beneficial to the customer (e.g., improvements specific to a product manufactured by the customer) could be prioritized at the expense of "explorative" innovation that is valuable outside the relationship, or a broader customer base. If the customer is opportunistic and cannot commit to a long-term relationship, the supplier firm's long-term growth and survival prospects could be adversely affected.

Customer opportunism is especially problematic for the suppliers regarding their relationship-specific R&D and innovation activities, since these deliverables cannot be specified ex ante and complete contracts cannot be written. Suppliers may still enter into relationships based on the belief that implicit contracts will be honored. In this situation, "trust" or reputation may play an important role in motivating the suppliers to make risky long-term investments on behalf of their customers. For example, Dasgupta, Zhang, and Zhu (2020) find that prior social links between the managers and board members of the suppliers and customers facilitate investment in relationship-specific innovation activities.

In this paper, I investigate how the scale and scope of supplier innovation activity changes when the customer's reputation is adversely affected, and trust in the customer is impaired. In particular, I examine the consequences of an adverse shock to trust or reputation due to the revelation of financial fraud of a customer. Serious financial misconducts are regularly picked up by the securities and exchange commission (SEC) either through its own investigation or using inputs from other agents like the media, auditors, or whistle-blowers. I hypothesize that the resulting loss of trust will (a) weaken the customer's bargaining power (as it may not be able to attract new suppliers and become even more dependent on its existing suppliers) and (b) reduce the incentive of the supplier to make relationship-specific investments, since implicit contracts are perceived as less likely to be honored. As a result, the supplier will engage in more explorative innovation and less exploitative innovation. This is what I find. In fact, subsequent to customer fraud revelation, suppliers spend less on R&D and generate fewer patents. However, while their sales to the fraudulent customer flattens out, they add new customers and outperform the industry in terms of overall sales. Most strikingly, compared to suppliers in the same industry with principal customers, their survival likelihood is higher over a 10-year period.[34] The survival effect is nuanced: while in the first three years, more of the

[34] The effect is quantitatively important. Univariate comparisons show that while the failure rate of the affected suppliers over the ten-year period is 8.17%, that for the control group is 12% -- this almost 4% differential is substantial in the context of an overall failure rate of the two groups combined of 11%.

suppliers of fraudulent customers exit, the *cumulative* survival rate for the suppliers of fraudulent customers is above that of the control group after the first three years.

I show that engaging in more explorative innovation improves survival likelihood. First, I show that in a sample of suppliers with at least one principal customer, those generating more explorative innovation generally are more likely to survive than those generating more exploitative innovation. Second, for a matched sample of suppliers whose customers commit fraud and other suppliers, I instrument explorative innovation by the exposure to customer fraud, and show that more explorative innovation is associated with higher survival likelihood over a 10-year period.[35] Overall, these results support the view that customer bargaining power and the myopic incentive of managers to prolong an on-going relationship with a principal customer leads to over-investment in customer-specific innovation (and in overall R&D and innovation activity), at the cost of a more diversified innovation strategy which could be more beneficial in the longer term. In other words, myopic supplier management is likely to assign higher weight to short-term profits, and the impact that losing a large customer might have on those profits, than is dictated by discount rates that are relevant for shareholders, leading to suboptimal innovation strategies.

My results are different from those documented in a contemporaneous paper by Selvam and Tan (2020), who examine the effect of covenant violations by customers on the suppliers' innovation. The authors find that suppliers innovate more, cite the customer patents more, and increase the overlap with the customer's innovation areas. This is attributed to the "bonding hypothesis", namely, due to its weakened bargaining power, the customer provides monetary and non-monetary incentives (e.g., in the form of more information sharing) to retain its supplier relationships. Financially impaired customers may also have an incentive to outsource innovation to suppliers. In contrast, I find that following financial fraud by the customer, suppliers innovate less, and move their innovation away from the affected customer by engaging in more diversified innovation. One possible reason why my results are different is that the expectation that implicit contracts would be honored is necessary for suppliers to engage in relationship-specific innovation. However, it is this crucial component of the relationship that is most called into question when the customer's reputation is affected.

[35] Customer fraud revelation could directly affect supplier survival through the effect it has on the supplier's sales. With this in mind, I control for the percentage change in the supplier's sales to fraud customers. It is worth noting, however, that such a channel, if not fully controlled for, would bias against my finding that the suppliers of fraudulent customers are more likely to survive.

Related, the magnitude of the shock to the customer's reputation and the implications for its future cash flows in my case is also substantial – in my sample, the customers suffer abnormal returns of -10% around the revelation of fraud.

My results are consistent with the arguments in Manso, Balsmeier, and Fleming (2019), who present a model that is based on the tension between exploration and exploitation that is inherent in innovation activity. When future sales are likely to be lower, the return from exploitation (e.g., process innovation that lowers production costs) declines. At the same time, the cost of failure from exploration is lower, since profits are low anyway. As a consequence, more explorative innovation occurs at the expense of exploitative innovation. A similar mechanism is likely to be at work in my context, reinforced by the fact that the affected customer might need to scale down its operations and even exit if the consequences of the fraud are serious enough. This reduces the return from exploitative innovation and encourages explorative innovation. Moreover, as Balsmeier, Fleming, and Manso (2017) suggest, firms in general (and not only the suppliers) may be innovating sub-optimally due to other types of frictions, and their survival might improve when the tradeoff changes against exploitative innovation. For example, managers may have incentives to generate more patents that are incremental rather than aim for riskier, higher-impact patents, especially when boards over-scrutinize managers; both boards and managers may be myopic and sensitive to the fact that the stock market does not properly recognize the long-term value of new types of innovation. As Manso, Balsmeier, and Fleming (2019) observe, there may exist "inherent biases towards exploitation, for example, due to the imperfect protection of property rights, or the difficulty of commercializing new technologies and appropriating their profits for the inventing firm". If the return to exploitation decreases, such biases are less likely to be important, and firms can be better off. Consistent with the argument that the suppliers benefit from diversifying their innovation, I find that the number of identifiable customers that the suppliers sell to increases after fraud revelation relative to the control group.

My results contribute to several strands of literature. First, I add to a growing literature on innovation in the supply chain. Using mutual fund flow-driven price pressure to identify exogenous negative shocks to stock prices, Williams and Xiao (2016) find that suppliers decrease subsequent R&D investment and produce fewer patents following declines in their key customers' market values. Chu, Tian, and Wang (2019) demonstrate that geographical proximity of customers and suppliers facilitates knowledge spillover through interaction among employees and researchers and leads to more customer-specific innovation. Dasgupta, Zhang, and Zhu (2020) demonstrate that prior social connections among high-rank executives and directors of the trading partners mitigates opportunism and hold-up. Chen, Dasgupta, Huynh, and Xia (2020) examine how upstream competition causes suppliers to relocate plants closer to their principal customers in order to cooperate more on innovation and forge closer ties with the customer. Selvam and Tan (2020) examine how customer financial distress affects supplier innovation. My paper focuses on how the *nature* of supplier innovation changes following an adverse reputational shock to the customer and how this affects the supplier's survival likelihood. My results suggest that supplier innovation is suboptimally diversified, possibly reflecting customer bargaining power and supplier managerial myopia.

Second, I contribute to the understanding of the wider real effects of corporate fraud going beyond the firms that commit financial misconduct. Giannetti and Wang (2016) show that the revelation of financial misconduct by firms can have widespread effects on the stock market. Following fraud revelation, households' stock market participation in the state where the fraudulent firm is headquartered decreases, even in firms that did not engage in fraud. Kedia and Philippon (2007) show that firms that manipulate earnings invest and hire more than levels warranted by their productivity to signal to the market that earnings are consistent with their real decisions; however, they do not examine peer effects. Beatty, Liao, and Yu (2013) find that peers of fraudulent firms mistakenly increase investment during the fraud periods and equity analysts potentially contribute to this spillover effect. Their results indicate that even close peers do not suspect financial fraud and adjust their investment decisions in response to their fraudulent competitor's perceived overperformance. To the best of my knowledge, there is no study on the changes in investment decisions of stakeholders after the revelation of financial misconduct.

Finally, there is also a growing literature on the propagation of shocks through vertical linkages in the economy (Acemoglu, Carvalho, Ozdaglar, and Tahbaz-Salehi, 2012; Acemoglu, Ozdaglar, and Alireza Tahbaz-Salehi, 2017), as well as the effect of supply chain disruptions (mostly in operations management). Several authors leverage natural disasters to study the propagation of shocks to upstream as well as downstream firms and find large (and sometimes asymmetric) effects (Barrot and Sauvagnat,2016; Boehm, Flaaen, and Pandalai-Nayar, 2019; Carvalho, Nirei, Saito, and Tahbaz-Salehi, 2016). My results suggest that the long-term effect of such disruptions on the suppliers depend on the flexibility with which the latter can adjust their innovation and may not be as serious as one might suppose.

The rest of the paper is organized as follows. Section 3.2 describes sample construction, variable description and empirical methodology. Section 3.3 discusses summary statistics and the results. I conclude in Section 3.4.

3.2. Data and Empirical Methodology

3.2.1 Data source and sample

My sample is based on all U.S. firms available in Compustat from 1990 to 2015. I exclude financial firms (SIC: 6000-6999), utility firms (SIC: 4900-4999), and government organizations (SIC: 9000-9999) as they are subject to different sets of regulatory requirements. Since I group firms by industry in my empirical set-up, I also drop the conglomerates (GICS: 201050). I use information from the SEC website to obtain enforcement releases in order to identify fraudulent customers and their initial public revelation dates. [36] I identify enforcement actions brought by the SEC and the Department of Justice (DOJ) based on charges of financial misrepresentation under Section 13(b) of the Securities Exchange Act of 1934. [37] Then, following Karpoff et al. (2017), I collect all fraud-related events available from enforcement releases, SEC filings, and news items (LexisNexis). These events include SEC informal/formal investigation, analyst report or whistle-blower information, restatement announcement, and press releases of a firm's internal investigation. Among these interrelated news items, I identify the news or public announcement that reveals a firm's fraud to the public for the first time. Figure 3.1 below shows the timeline of events pertaining to the fraud at Raytheon, a major U.S. defense contractor, from September 16, 1999 through March 28, 2007. In 1997 and 1998, Raytheon prematurely recognized revenue on Raytheon Aircraft Company's sale of unfinished aircraft through improper "bill and hold" transactions. Raytheon overstated approximately $190 million in net sales between 1997 and 2001. Raytheon also failed to fully and accurately disclose material trends and uncertainties. On September 16, 1999, Raytheon announced that its third-quarter earnings would be below analysts' projections, and it expected to take a pre-tax, third-quarter charge of between $350 million and $450 million. Shares in Raytheon fell by

[36] The U.S. SEC website documents enforcement releases from 1995.
[37] These fraud cases include at least one charges of violating Section 13(b)(2)(a), Section 13(b)(2)(b), and Section 13(b)(5) provisions of the 1934 Securities Exchange Act and Rule 17 CFR 240.13b2-1 and Rule 17 CFR 240.13b2-2 of the Code of Federal Regulations. For more details, please see Karpoff et al. (2017).

12% on the same day. On October 12, 1999, Raytheon announced a shortfall of its earnings projections for 1999 and 2000 -- the EPS would be between \$1.40 and \$1.50, well below Wall Street expectation of \$3.56 per share. The company's share plunged more than 40% in the afternoon. For Raytheon, I consider the first event (September 16) – subsequently emphasized in the enforcement release -- as the revelation date.

[Insert Figure 3.1 here]

I retrieve information on the customer-supplier relationships from both the FactSet Revere database and the Compustat segment customer file. The FactSet Revere database is a novel database that has comprehensive coverage for each inter-firm relationship. FactSet collects principal customers' information from firms' annual reports. In addition, FactSet analysts also collect data from various sources such as quarterly filings, press releases, investor presentations, and corporate announcements. FactSet Revere database includes comprehensive start and end dates between two inter-related firms. Suppliers can disclose their customers and customers can also disclose their suppliers, but I do not require a relationship to be disclosed by both firms. FactSet Revere Relationship database starts from April 3, 2003. The Compustat segment customer file is publicly available as the Statement of Financial Accounting Standards No. 14 (before 1997) and the Statement of Financial Accounting Standards No. 131 (after 1997) require firms to disclose the existence and sales to principal customers representing more than 10% of the firm's total revenues. However, the database reports only the name of the principal customers without identifiers, and the reported principal customer names are not consistent. Sometimes the same customer is reported in a different abbreviated form in different years and by different suppliers. I follow Banerjee, Dasgupta, and Kim (2008) to manually match customers to their Compustat identifier (i.e., GVKEY) when possible. I use both the FactSet Revere Relationship database and Compustat Customer Segment Files to identify the suppliers of fraudulent customers (affected suppliers). Affected suppliers are identified as those who supply to a fraudulent customer in the year when the customer's fraud is revealed to the public. Some supplier firms might be subject to multiple announcement events. I only include the first event in order to clearly construct the before- and after-event periods.

The patent data used in Kogan et al. (2017), which has a longer and wider coverage than the patent dataset available in the NBER, is made available to researchers by the authors. They provide this enlarged patent dataset between 1926 and 2010, which carefully matches the patents granted by USPTO with the CRSP stock identifier (PERMNO). I use this dataset as the basis of my analysis of innovation. However, any patent dataset is heavily truncated because it typically takes several years to process a patent application. The patent is not recorded by the USPTO until it is granted. Thus, the number of patents falls towards 2010 because the patents have not been granted yet. Following Hall et al. (2001), I use the historical distribution of application-grant time-lag to predict the missing number of patent applications. Dass, Nanda, and Xiao (2017) summarize the truncation bias corrections in patent data. They use updated patent data to examine the NBER-2006 sample. They find that truncation bias in the number of patent applications has worsened in recent years. I check the robustness of the results by using two historical distributions of application-grant lag. The first historical distribution of application-grant lag is from 2003-2006. The 2003-2006 historical distribution of application-grant lag is used to correct truncation bias of the number of patents from 2007 to 2010. For example, 88.82% of patents are expected to be missing in 2007 based on the distribution in the 2003-2006 period because only 11.28% of patents tend to be granted within one year (0.52% in the same year as application year (2003), 10.66% in 2004). To adjust truncation bias from historical patterns between 2003 and 2006, the number of patents that are granted in 2007 should be divided by 11.18%. I also use the distribution of application-grant lag in the 1990-2000 period. I then compute the truncation-adjusted number of patents from 2001 to 2010.[38] I get similar results using both historical patterns to adjust truncation bias in the number of patents.

3.2.2 Variables

R&D expenses have been widely used in the literature as a proxy for innovation input (Allen and Phillips, 2000; Griffith, Redding, and Reenen, 2004). Specifically, I treat R&D expenses as zero if R&D is missing, and I scale R&D expenses by the book value of total assets. Following the existing literature on corporate innovation, I measure the scale of innovation output by counting the number of patents that are filed by firms and are eventually granted for each firm-year observation. I use the patent application year rather than the grant year because the application year is closer to the time when the innovation is produced (Hall et al., 2001). I use the standard method to adjust the above innovation output measure to deal with the truncation problem associated with the patent data (Hall et al., 2001). Since I only observe the

[38] Results for the correction of patent truncation bias using historical application-grant lag between 1990 and 2000 are reported in the online appendix.

patents that are finally granted, towards the end of my sample period, those patents that are still in process are not observed.

Following existing literature, I define innovation style by classifying patents into exploitative vs. explorative patents. [39] Exploitative patents cite at least 60% of patents that are either the firm's own patents or patents that are cited by the firm in the past five years. Explorative patents cite at least 60% of patents that are neither firm's own patents nor the patents that are cited by the firm in the past five years. I also use a stricter citation requirement (80%) for classifying the style of innovation as a robustness test. Following Jaffe (1986) and Bena and Li (2014), to calculate technological proximity between supplier and customer, I calculate the closeness of their innovation activities in the technology space based on their patents' technology class distribution. The technology proximity variable takes a value between 0 and 1.

For firm characteristics, I compute all variables for firm *i* in fiscal year *t*. My variables include firm size (the natural logarithm of book value of total assets), growth opportunities (market-to-book ratio), profitability (Roa), asset tangibility (net PPE scaled by total assets), capital expenditures, leverage, and industry concentration (the Herfindahl index based on sales). Aghion et al. (2005) point out a non-linear effect of product market competition on innovation output. Hence, I include the squared Herfindahl index in my regressions. Detailed definitions of variables can be found in Appendix Table 3.1.

3.2.3 Empirical methodology

Supplier firms of fraudulent customers are classified as the treated group. I determine the control firms based on their Standard Industry Classification (SIC) codes in Compustat. Control firms operate in the same 2-digit SIC code as the treated suppliers. Following Gormley and Matsa (2011), I analyze the treated suppliers' response to their corresponding customers' announcement of fraud. Specifically, I compare changes in their behavior relative to other firms' behavior in the same 2-digit SIC industry around the time of the announcement of fraud. For every year, in each affected industry, I construct a cohort of treated suppliers and matched control firms using firm-year observations for the five years before and five years after the announcement. In the case of the revelation of Raytheon's financial misconduct, among

[39] See for example: Levinthal and March, 1993; McGrath, 2001; Benner and Tushman, 2002; Smith and Tushman, 2005; Gao, et al., 2018; Liu et al., 2017.

Raytheon's suppliers, Mercury Systems, Inc and Ducommun Incorporated operate in two-digit SIC industries 36 and 37, respectively. Then, I construct two cohorts for Mercury Systems, Inc and Ducommun Incorporated separately since the control firms come from different two-digit SIC industries. In the control group, firm-year observations are removed if they become treated by other revelations of financial misconduct. Firms are not required to be in the sample for the full ten years around the event. I then "stack" all cohorts of treated and control firms into one dataset. In total, we identify 77 fraudulent customers and 477 affected suppliers in 202 cohorts. They come from 38 different 2-digit SIC industries. Customers can have suppliers operating in different 2-digit SIC industries. Thus, the size of my control group is large for each event. Having a large control group enables me to select firms that share similar ex-ante characteristics with the treated one. For each treated firm, I select firms in the same quartile of size, leverage, sales, and trade receivables in the same 2-digit SIC industry (or same cohort) in year *t*-5.[40] In my setting, both treated and control firms in each cohort are from the same 2-digit SIC industry, so any industry trend that potentially biases my results can be absorbed (at least at 2-digit SIC level). I then estimate the average treatment effect. Specifically, I estimate the following firm-panel regression:

$$Y_{ijct} = \beta_0 + \beta_1 Exposure_{ict} + \sum_{k=2}^{N} \beta_k Controls_{it} + \gamma_{ic} + \omega_{tc} + \varepsilon_{ijct}$$

where *y* is one of several dependent variables of interest for firm *i* and year *t*, and *Exposure* is an indicator that equals one for treated suppliers in the five years after the fraud announcement in cohort *c* and industry *j*. I include a set of variables to control for observable differences among the sample firms as well as firm-cohort fixed effects, γ_{ic}, to ensure that the estimated impact of customer's fraud is controlled for any fixed differences between firms. I also include year-cohort fixed effects, ω_{tc} to control for any secular time trend.

3.3. Empirical Results

3.3.1 Summary statistics

The disclosure of fraudulent activity has a significant price impact on customer firms as shown in Figure 3.2(a). On average, fraudulent customers lose more than 10% of their market

[40] Year *t* is the event year when a customer's fraud becomes known to the public.

values. Figure 3.2(b) shows that the direct suppliers of these fraudulent firms also have a negative price impact but of a lower magnitude.[41]

[Insert Figure 3.2(a) and 3.2(b) here]

Figure 3.2(a) indicates that, for the fraudulent firms, investors respond to the information in the trigger news item quickly, and the expected loss of value is substantial (Karpoff, Lee, and Martin, 2008). Figure 3.3 plots the percentage increase in supplier's sales in year t, where t=-5,-4,…,+5, over sales six years prior to the fraud event, i.e. $(S_t - S_{-6})/S_{-6}$, . To understand the modest value loss for the suppliers, I first note from Figure 3.3 that after a drop immediately after the event year, sales to the fraudulent principal customer level off. In fact, I find that none of the fraudulent customer firms in my sample file for bankruptcy or get delisted within five years after the event. Thus, the immediate loss in revenue for the suppliers is not substantial. At the same time, Figure 3.3 shows that the percentage increase (relative to year t=-6) in sales to other customers and the percentage increase in overall sales, adjusted for the corresponding increase for median 2-digit industry sales, increase over the next five years. This latter observation is consistent with my regression results reported below showing that following the customer fraud, suppliers switch to a more diversified innovation strategy, attain higher industry-adjusted sales growth, and sell to more principal customers over a longer horizon. Since these changes involve a redistribution of profits between the present and the future, and there is uncertainty about the success of the alternative strategies as well as the survival of the fraudulent customer given the average 10% loss in market value, the market reactions for the affected suppliers are ambiguous, and unlikely to be very large in magnitude.[42]

[Insert Figure 3.3 here]

Table 3.1 presents summary statistics for my sample treated supplier firms and their 2-digit SIC industry peers. As shown in Table 3.1, an average supplier in my sample invests 9.5% of their total assets in R&D expenses and these innovation inputs translate into 9.7 granted patents per year. The average percentage of exploitative (explorative) patents is 33% (59%). The summary statistics of treated firms and matched control firms in the five years prior to the

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[41] The average cumulative abnormal buy and hold return between day -5 and day +5 is minus 1.794% with t-value of -3.469 (p-value=0.0006). The average cumulative abnormal buy and hold return on the event day is minus 0.560% with t-value of -2.844 (p-value=0.0046).

[42] Hertzel, et al. (2008) find that news of bankruptcy filings of customers have significant negative stock price impact on the direct suppliers.

revelation of frauds are reported in Table 3.2. Treated suppliers and control firms in the same industry have similar R&D expenses. On average, treated suppliers spend 8.97% of their assets on R&D, whereas control firms spend 8.80% of their assets. In addition, treated suppliers and control firms have insignificantly different characteristics. The main regression analysis is based on the matched sample of control firms, but my results remain similar using the full sample of control firms (same industry peers).

[Insert Table 3.1 and Table 3.2 here]

3.3.2 Effect of financial misconduct on suppliers' R&D and innovation strategy

I begin by analyzing how firms adjust their R&D investment in response to the disclosure of fraudulent activities of their customers. I do this by using a linear difference-in-difference analysis of R&D spending, after controlling for cohort-year and firm-cohort fixed effects. The control group includes matched firms in the same 2-digit SIC industry as the treated firms. The results are reported in Table 3.3. To address the concern that my estimates will be biased if control variables are affected by the treatment, I report results without any other firm-specific controls in the first column and add additional control variables in the second column. I find that R&D investment decreases for the treated group in the post-treatment period. Treated suppliers decrease R&D investment by 0.8% of their total assets. The fall in R&D investment accounts for 10% of the average R&D spending by the treated suppliers prior to the event.

I next examine whether the negative effect of customers' announcement of fraud on supplier R&D is also transmitted to innovation output. I calculate the natural logarithm of one plus the number of patents produced by firms. In Table 3.4, the coefficient of treated*post corresponds to a 15.76% decrease in produced patents for treated suppliers relative to matched industry peers per year in the five-year window after their customers' announcement of fraud. The results based on patenting outcomes reinforce the previous findings on treated suppliers' R&D investment.

In Appendix Table 3.2, I find similar results from the full sample in which the control firms are those that operate in the same 2-digit SIC industries as the treated suppliers.

[Insert Table 3.3 and Table 3.4 here]

Following the literature on R&D, I treat missing values of R&D as firms having no significant R&D to report.[43] However, one concern is that my estimates could be biased if these missing observations do not mean zero R&D investment. In view of this concern, I redo the analysis by dropping firms that do not report R&D expenses in any year in the sample, and the results remain very similar. Appendix Table 3.3 reports the main R&D results where firms with missing R&D are dropped. I also focus on firms with non-missing patent information to re-examine the effect of the revelation of customer fraud on suppliers' innovation output. Columns (3) and (4) in Appendix Table 3.3 show consistent results that treated suppliers produce fewer patents after the event.

In Figure 3.4, I present my tests of parallel trends. I regress R&D expenses and innovation output on the treatment dummy interacted with year dummies representing *t*-5, *t*-4, *t*-3, *t*-2, *t*-1, *t*, *t*+1, *t*+2, *t*+3, *t*+4, *t*+5. I find that there is no significant difference in either R&D investment or innovation output between the treated suppliers and control firms before the event. The decrease in treated suppliers' R&D investment coincides with the event year, whereas the decrease in their innovation output occurs two years later, possibly in response to lower R&D.

[Insert Figure 3.4 here]

Overall, these results show that the affected suppliers adjust down the scale of their innovation activity when a major customer's financial fraud is revealed. This could reflect the fact that the value of relationship-specific investment is lower, from the supplier's point of view, when the customer's financial fraud is revealed. However, since I do not observe treated suppliers' R&D investment at the relationship level, I cannot directly test whether the supplier's investment that is specific to the fraudulent customer is affected. For example, as I observed, the market reaction for the treated suppliers is negative. It is possible that treated suppliers reduce R&D investment in order to improve earnings outlook. To deal with the limitation of R&D data, I take advantage of the richness of patent data to further examine: 1) change in technological proximity between suppliers and their fraudulent customers; 2) change in treated suppliers' innovation style. I show that treated suppliers start to shift their innovation

[43] In the treated group, 77% of firms report R&D and have median (average) R&D of 0.077(0.106), 72% of firms have patent data with median (average) Log(Patents) of 1.386 (1.921). In the control group, 82% of firms report R&D and have a median (average) R&D of 0.068 (0.101), 59% of firms have patent data with a median (average) Log(Patents) of 1.099(1.680).

activities away from fraudulent customers, and engage in more explorative innovation and less exploitative innovation.

3.3.3 Do suppliers adjust their innovation activities?

First, in order to understand if suppliers move their innovation away from fraudulent corporate customers, I perform a univariate comparison of technology proximity between treated suppliers and their customers before and after the disclosure of customers' fraud. The statistics from univariate comparison are presented in panel A of Table 3.5. The results show there is a significant decrease in technological proximity between the treated suppliers and their fraudulent customers after the event and an insignificant increase in the proximity between treated suppliers and their non-fraudulent customers. The difference of changes in treated suppliers' technological proximity with the fraudulent group and with the non-fraudulent group is significant at 5% level. This suggests that treated suppliers adjust their innovation activities away from the fraudulent customers. The regression estimates are presented in panel B and show a very similar and significant pattern.

Next, I test whether treated suppliers' "style" of innovation changes. Patents are classified into "exploratory" and "exploitative" categories as defined in Section 3.2.2. I calculate exploitative (explorative) scores as the percentage of a firm's number of exploitative (explorative) patents to its total number of patents each year. In column (1) of Table 3.6, I find that treated suppliers decrease the proportion of exploitative patents by approximately 13% after fraudulent customers' disclosure of fraud. On the other hand, the disclosure of fraudulent behavior of customers drives their suppliers to explore new areas of innovation that could be potentially valuable to a broader customer base. As a result, they create 7.9% more explorative patents relative to matched firms in the same 2-digit SIC industries.[44] Treated suppliers divert their resources towards new knowledge domains.

[Insert Table 3.5 here]

[Insert Table 3.6 here]

The results based on innovation style suggest that previous findings on treated suppliers' R&D investment and subsequent innovation output represent a shift not only in the scale, but

[44] The results are similar if I use 80% threshold to define exploitative and explorative patents (see Appendix Table 3.4). The results are also similar in the full sample, reported in the online appendix.

also the scope of their innovation activity. The shift of the treated suppliers' technological focus from fraudulent customers to other customers is consistent with the hypothesis that the value from relationship-specific investment is likely to be lower after the revelation of the fraud. This, in turn, could arise from several channels, e.g., (a) the fraud firm could face greater uncertainties, and the relationship could be terminated or scaled down earlier than expected (b) the fraud firm could be less likely to honor implicit contracts with the supplier, which could expose the latter to hold-up, or (c) the threat that the customer would walk away if the supplier engages less in exploitative innovation that benefits the customer is lower, so that myopic supplier managers are more willing to engage in a more diversified innovation strategy that could broaden its customer base.

While the first two of these possibilities are consistent with optimal, shareholder-value maximizing behavior by managers, the third presumes that managers do not maximize shareholder value when principal customers have bargaining power. If managers are over-sensitive to short-term profits because of career-concern issues or because short-term earnings are over-weighted in their compensation packages, and a principal customer can use the threat of terminating the relationship if the supplier does not prioritize innovation that mainly benefits the customer, shareholder value is not maximized. As noted, none of the fraudulent customers in my sample exit within five years of the fraud revelation, and as Figure 3.3 shows, after an initial decrease, sales to the principal customer level off. Thus, it is plausible that the switch in innovation strategy is triggered not because the relationship-specific innovation has become *unprofitable,* but rather because the bargaining power of the customer is weakened (and it has become less trustworthy and less likely to honor implicit contracts). In section 3.3.4, I present results showing that the treated suppliers enjoy faster sales growth, attract more principal customers, and improve their survival likelihood relative to matched suppliers. These results also suggest that in the presence of powerful principal customers, suppliers forego innovation diversity that adversely affects overall sales growth at the expense of short-term stability. These results are more consistent with managerial agency issues.

Before leaving this section, I explore cross-sectional heterogeneity to understand the type of supplier that is more affected by customer fraud. The customer bargaining power argument suggests that the impact of customer fraud and the weakened customer bargaining power would impact the innovation strategies of the suppliers that are smaller in size relative to their customers the most. In Table 3.7, I divide the supplier firms into small and large relative size groups and report the results similar to above on the effect of customer fraud on supplier R&D investment, innovation output, and innovation style. I use dummy interactions to investigate whether suppliers with smaller relative sizes reduce their R&D investment and innovation output more aggressively. “Small” is an indicator variable taking the value of one if the average ratio of the size of the supplier to the size of the customer (over the five-year period prior to fraud) is below the median, and zero otherwise. Panel A shows that R&D expenses and patent count of the smaller suppliers fall more significantly, and Panel B shows that, while all treated firms increase their focus on exploratory innovation, the style change for smaller suppliers is more stark.

[Insert Table 3.7 here]

3.3.4 Sales growth, new customers, innovation style, and long-term survival of affected suppliers

After observing strategic shifts in R&D investments and innovation styles, I investigate the effect it has on the affected suppliers’ long-run survival. In Figure 3.5, I plot the cumulative failure rates of affected suppliers for the ten years after the financial misconduct of their customer firms become publicly known. I define firm failures as performance-related stock market delistings, liquidations, and distressed mergers (with delisting codes 400-490 and 5200584). From Figure 3.5, I observe an immediate increase of the fraction of failed firms in the treated group compared with the control group after the event, but over the long-term, the fraction of failed firms increases at a slower rate compared to the control group. This suggests that once they survive the first few years following the fraud, treated suppliers actually have better survival prospects than matched firms in the same industry. Over a 10-year period, while 12% of the control group exit, that percentage is only 8% for the treated group.[45]

[Insert Figure 3.5 here]

To examine the link between innovation style and survival, I do a series of tests. First, in Table 3.8, I report linear probability and probit regressions where I predict a firm’s likelihood to fail after the revelation of customers’ financial misconduct. Specifically, I examine failure likelihood in two sub-periods. When I confine attention to the first three years after the event (Columns (1) and (3)), I find that treated suppliers are more likely to fail in the following year

[45] The percentage of suppliers with customers accounting for at least 10 percent of the supplier’s sales is 62% for the treated group and 60% for the control group, and the difference is statistically insignificant.

than the control group. Beyond the first three years and until ten years after the event, I find that treated suppliers are less likely to fail in the following year than other firms in the same industry ((Column (2) and (4)). These results are consistent with Figure 3.5, in which I observe a flip in treated suppliers' survival rates. The estimates indicate that in the first three years, the affected suppliers have a 1.86% higher likelihood of exit the following year; however, for the next seven years, they have a 1.13% lower likelihood of exit the following year. These magnitudes are economically significant given that only about 10% of the sample firms exit over the ten-year period after the revelation of the customer fraud.

[Insert Table 3.8 here]

The explanation for this result may lie in the significant changes in the nature of innovation activities of the treated firms, noted earlier. In order to test the effect of innovation style on survival, in Table 3.9, I perform survival analysis for the ten-year post-event period. The results based on Cox proportional hazard model are reported in column (1), results based on the hazard function that assumes Weibull distribution are reported in columns (2), while column (3) reports results based on the linear probability model. Panel A reports results on the matched sample. Since my purpose here is to explore the association between innovation style and survival likelihood of both treated suppliers and control firms, Panel B reports results for the full sample as well, which includes all the industry peers of the affected suppliers. The dependent variable takes a value of one if failure occurs (i.e., the firm exits). Panel A reports results for the matched sample, and panel B those for the full sample, which includes all supplier firms from the same 2-digit SIC industry as the treated supplier. The variables of interest are those corresponding to innovation style, i.e., *Explore* and *Exploit*. *Exploit* (*Explore*) is the natural logarithm of 1 plus the percentage of cumulative number of exploitative (explorative) patents after the revelation of customer fraud. I find that likelihood of failure decreases if the firm engages in more explorative innovation (higher values of *Explore*) in all regressions. The treated firms are less likely to fail even after controlling for innovation style, although the results are marginal for the matched sample.

Since innovation style is endogenously chosen by firms, a causal interpretation of the results of Table 3.9 is problematic. For example, it could be the case that suppliers who are more likely to survive take more risk and engage ton more explorative innovation. In Table 3.10, I address this endogeneity concern. I first run cross-sectional linear probability and probit regressions, where the dependent variable takes a value of one if the firm exits the sample at the end of 10 years after the fraud event is revealed, and is zero otherwise. Columns (1) and (2) show that treated firms are less likely to fail over the 10-year horizon. The economic magnitude is significant: the failure rate of the treated firms is 1.24% lower than for control firms, in the context of an 11% failure rate for all sample firms. Column (3) shows that treated firms do more explorative innovation. The dependent variable, *Explore*, is the natural logarithm of one plus the total percentage of the explorative patents up to five years after the revelation of customer fraud, or the year prior to its exit, whichever is earlier. Since my purpose is to investigate whether more explorative innovation is the likely channel for lower failure rate of the treated firms, I use the regression in column (3) as the first stage of two-stage regressions in which the endogenous *Explore* is instrumented by the *Treated* dummy. In columns (4)-(5), I report the second stage of two-stage OLS (linear probability) and probit models, respectively. In these regressions, I control for the changes in the fraction of a supplier's sales to fraudulent customers for up to five years after the fraud event, or the year before its exit, whichever is earlier. This mitigates concern about my instrument meeting the exclusion restriction, since the main alternative channel through which exposure to customer fraud could affect survival likelihood is through the effect of the former on the supplier's sales.

In Tables 3.11 and 3.12, I provide additional evidence of the benefits of a more diversified innovation strategy. In Table 3.11, I examine how the number of customers that can be identified in my database changes for the affected suppliers vis-a-vis the control group. I find that, over the next five years, affected suppliers increase the number of principal customers. The result is consistent with the observation that the suppliers try to diversify their customer base when a major customer is impaired, and that the long-term survival rate of the affected suppliers increases relative to the control group. In Table 3.12, the dependent variable is the supplier's sales in period t over its sales six years before the event minus the corresponding ratio based on industry median sales for the supplier's 2-digit industry. The regression is done in the same stacked difference-in-difference setting as for tables 3.3-3.5. The results show that the treated suppliers experience more rapid industry-adjusted sales growth relative to the baseline year than the control group.

3.4. Conclusion

Suppliers often need to make relationship-specific investments to customize their products to meet their customers' specific requirements, and increasingly they are implementing innovation activities for their customers. Relationship-specificity, however, comes at a cost. In this paper, I highlight one such cost: suboptimal diversity of innovation. I investigate how the suppliers respond to the disclosure of financial misconduct by one of their corporate customers by adjusting their innovation activity. I examine how supplier firms respond to adversity – an issue that derives its importance from ideas put forward originally by Schumpeter (1939) and followed up by others. Schumpeter (1939) argues that adversity (e.g., economic recession) leads to a process of creative destruction and spurs firms to develop new technologies that make the economy stronger in the long run. I find that suppliers make significant adjustments to innovation when their customer firms are revealed to have committed financial misconduct. Suppliers diversify their innovation and tailor their R&D away from the fraudulent customers and towards their other corporate customers. Interestingly, these adjustments increase their long-term survival rate as they engage in more explorative innovation. The results indicate that the supplier firms might be trapped into doing too much exploitative innovation at the behest of their customer firms – essentially trading off better long-term survival prospects for short-term profits.

Appendix 3.1: Variable definitions

Dependent variables	Definitions
R&D	Firm's R&D expense (compustat item: xrd) scaled by lagged total asset (compustat item: at). If R&D is missing, then the ratio is replaced as zero.
Log(patents)	Natural logarithm of 1 plus a firm's total number of patents filed (and eventually granted) in a fiscal year (firm's total number of patents are corrected for truncation bias).
Exploitative	The number of exploitative patents filed (and eventually granted) divided by the number of all patents filed (eventually granted) by the firm in a fiscal year.
Explorative	The number of explorative patents filed (and eventually granted) divided by the number of all patents filed (eventually granted) by the firm in a fiscal year.
Technological proximity	Following Jaffe (1986), the technology proximity between supplier i and customer j is computed as the uncentered correlation between their respective vectors of technological subcategories: $$T_{ij} = \frac{N_i N_j'}{(N_i N_i')^{1/2} (N_j N_j')^{1/2}}$$ Where $N_i = (N_{i1}, N_{i2}, \dots N_{i37})$ is a vector indicating the share of patents applied by supplier i in each technological subcategories every year. $N_j = (N_{j1}, N_{j2}, \dots N_{j37})$ is a vector indicating the share of patents applied by customer j in each technological subcategory in the past three years. I match the technology classes assigned by USPTO to 37 subcategories following the mapping in Hall et al. (2001). Technology proximity takes a value between 0 and 1 according to their common technology interests.
Control variables	
Size	Natural logarithm of total asset (compustat item: "at").
Mtb	The ratio of market value of total assets (compustat: "at" - "ceq" + "prcc_f" * "csho") to book value of total assets.
Leverage	Long-term debt (compustat item: dltt) and short-term debt (compustat item: dlcc) scaled by market value of total asset.
Roa	Income before extraordinary items (compustat item: ib) scaled by lagged total asset
Capex	Capital expenditure (compustat item: capx) scaled by total value of property, plant and equipment (compustat item: ppent) at the beginning of the year.
Tangibility	The ratio of total value of property, plant and equipment to the lagged total asset (compustat item: "ppent").
HIndex	The sum of squared market shares in the 4-digit-SIC industry.

Appendix 3.2: R&D and innovation output

This table reports the stacked DID results of the full sample. In columns (1) and (2), the dependent variable is R&D expense scaled by total asset. In column (3) and (4), the dependent variable is the natural logarithm of one plus a firm's total number of patents in a year. Treated is a dummy variable indicating affected suppliers. Post is an indicator variable equal to one for five years post fraud revelation and zero for five years before the fraud revelation. The standard errors are clustered by SIC 2-digit industry. Firm-cohort and year-cohort fixed effects are included. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	(1)R&D	(2)R&D	(3)Log(Patents)	(4)Log(Patents)
Treated*Post	-0.0122***	-0.0066**	-0.1108***	-0.1171***
	(-3.14)	(-2.44)	(-3.09)	(-3.39)
Controls	No	Yes	No	Yes
Firm*Cohort FE	Yes	Yes	Yes	Yes
Year*Cohort FE	Yes	Yes	Yes	Yes
Observations	462,991	462,991	422,579	422,579
Adjusted R^2	0.679	0.751	0.850	0.852

Appendix 3.3: Missing R&D and Patent Information

This table reports the stacked DID results of the matched sample. In column (1) and (2), the dependent variable is R&D expense scaled by total asset. Firms are excluded if they do not report R&D expense in any year in the sample. In column (3) and (4), the dependent variable is the natural logarithm of 1 plus a firm's total number of patents in a year. Firms are excluded if they do not produce any patents in any year in the sample. *Treated* is a dummy variable indicating affected suppliers. *Post* is an indicator variable equal to one for five years post fraud announcement and zero for five years before the fraud announcement. The standard errors are clustered by SIC 2-digit industry. Firm-cohort and year-cohort fixed effects are included. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	(1)R&D	(2)R&D	(3)Log(Patents)	(4)Log(Patents)
Treated*Post	-0.0105***	-0.0097**	-0.0857**	-0.0884*
	(-3.11)	(-2.38)	(-2.18)	(-1.99)
Controls	No	Yes	No	Yes
Firm*Cohort FE	Yes	Yes	Yes	Yes
Year*Cohort FE	Yes	Yes	Yes	Yes
Observations	10,829	10,829	7,558	7,558
Adjusted R^2	0.679	0.755	0.873	0.881

Appendix 3.4: Innovation style (80% threshold)

This table reports the stacked DID results of the matched sample. Strict citation threshold (80%) is sued to classify a patent into exploitative or explorative patent (see section 3.2.2 for details). The dependent variable in column (1) and (2) is the number of exploitative patents divided by the number of patents of a firm in a fiscal year. The dependent variable in column (3) and (4) is the number of explorative patents divided by the number of patents of a firm in a fiscal year. *Treated* is a dummy variable indicating affected suppliers. *Post* is an indicator variable equal to one for five years post fraud revelation and zero for five years before the fraud revelation. Firm-cohort and year-cohort fixed effects are included. The standard errors are clustered by SIC 2-digit industry. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	(1) Exploitative	(2) Exploitative	(3) Explorative	(4) Explorative
Treated*Post	-0.0450***	-0.0449***	0.0336*	0.0375*
	(-3.85)	(-3.54)	(1.72)	(1.91)
Controls	No	Yes	No	Yes
Firm*Cohort FE	Yes	Yes	Yes	Yes
Year*Cohort FE	Yes	Yes	Yes	Yes
Observations	4,334	4,334	4,334	4,334
Adjusted R^2	0.379	0.381	0.445	0.449

Figure 3.1 Timeline of the key fraud related events of Raytheon

This figure provides the timeline of key informational events pertaining to Raytheon. The events are collected from enforcement releases, SEC filings, and LexisNexis. The fraud period is the period of financial misconduct. Enforcement releases is the period when the SEC concludes the investigation and issue enforcement proceedings.

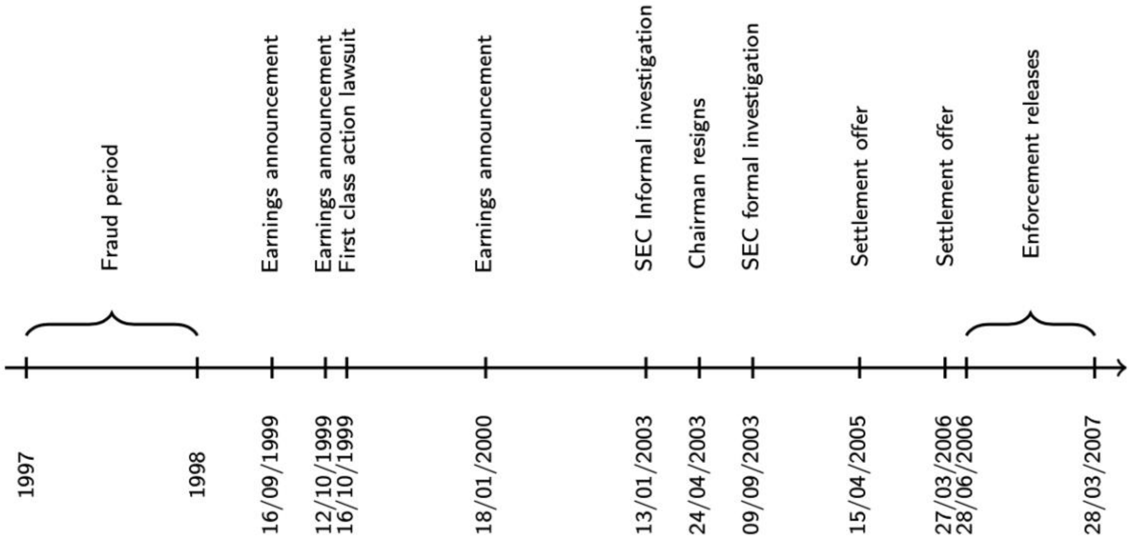


Figure 3.2 Average CAR around public revelation of fraud

Figure 3.2(a) and 3.2(b) reports the average cumulative buy and hold returns of fraudulent customers and their direct suppliers. The period starts from twenty days prior to the public revelation of customers' frauds until twenty days after the revelation. Day zero is the revelation day.

Figure 3.2(a) Average CAR of fraudulent customers from day -20 to +20

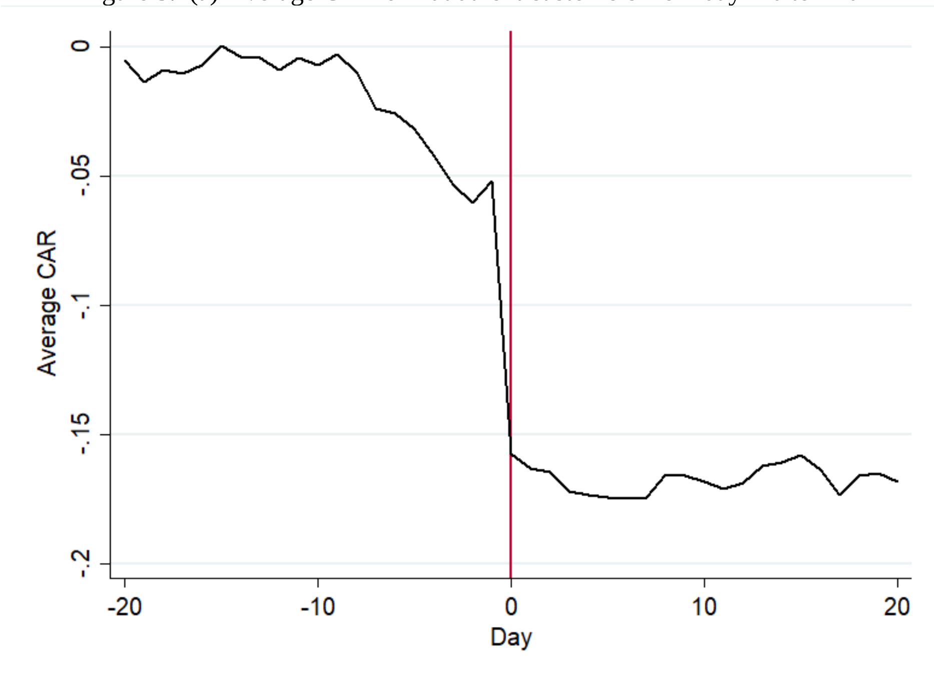


Figure 3.2(b) Average CAR of affected suppliers from day -20 to +20

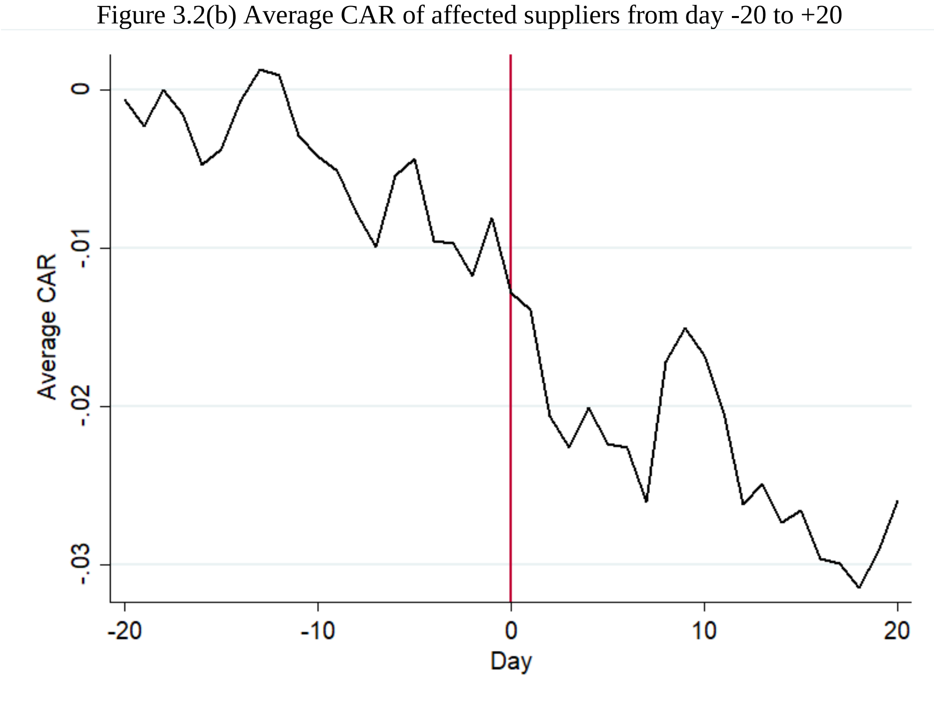


Figure 3.3 Trends in sales and sales to customers

This figure shows the average percentage change in affected suppliers' sales to fraudulent customers, sales to other customers (non-fraudulent customers), and suppliers' industry-adjusted sales revenue, respectively. The percentage change in the sales to fraud customers (non-fraud customers) in year *t* (*t* = -6, -5, -4. -3, -2, -1, 0, 1, 2, 3, 4, 5) is the difference between the sales to fraud customers (non-fraud customers) in year *t* and the sales to fraud customers (non-fraud customers) in year *t* = -6 scaled by sales to fraud customers (non-fraud customers) in year *t* = -6. The percentage change in industry adjusted sales revenue of suppliers in year *t* (*t* = -6, -5, -4. -3, -2, -1, 0, 1, 2, 3, 4, 5) is the difference between the sales adjusted by industry median sales in year *t* and the sales adjusted by industry median sales in year *t* = -6 scaled by the industry adjusted sales in year *t* = -6.

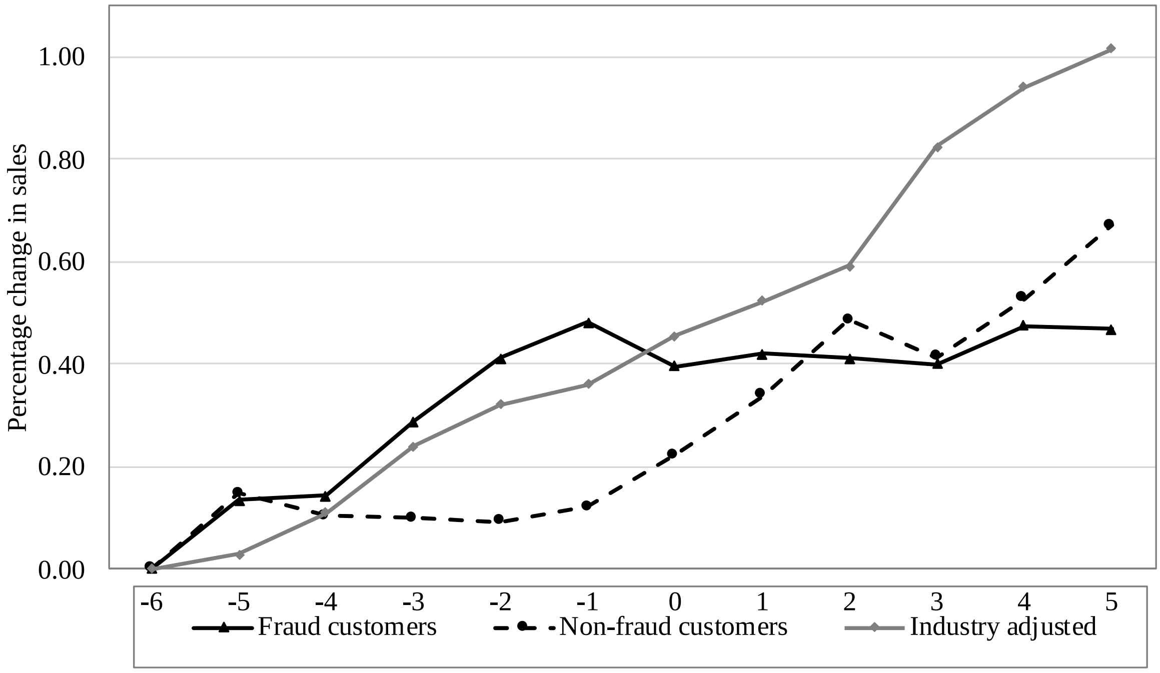
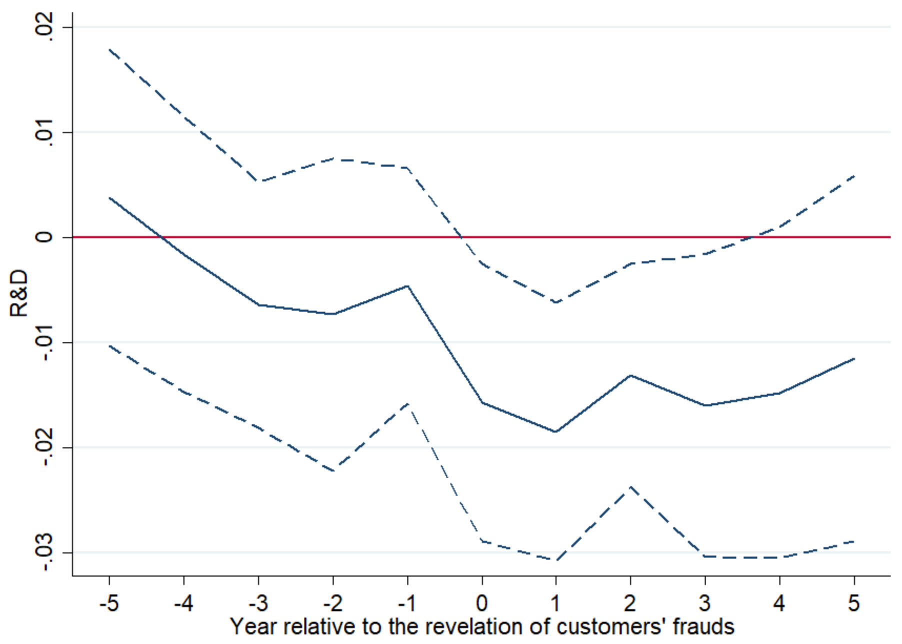


Figure 3.4 The effect of revelation of fraud on R&D and innovation output

The following figures plot the regression estimates from a firm-panel regression of R&D spending and the number of patents on the treatment dummy interacted with year dummies representing *t-5*, *t-4*, *t-3*, *t-2*, *t-1*, *t*, *t+1*, *t+2*, *t+3*, *t+4*, *t+5*. The sample includes the affected suppliers and the matched control firms. I include control variables, firm-cohort fixed effects, and year-cohort fixed effects. The effect of revelation of fraud is allowed to vary by year for each year from five years before the revelation of fraud through five years after. Ninety-five-percent confidence intervals are plotted as dotted lines. Standard errors are clustered at the industry level.



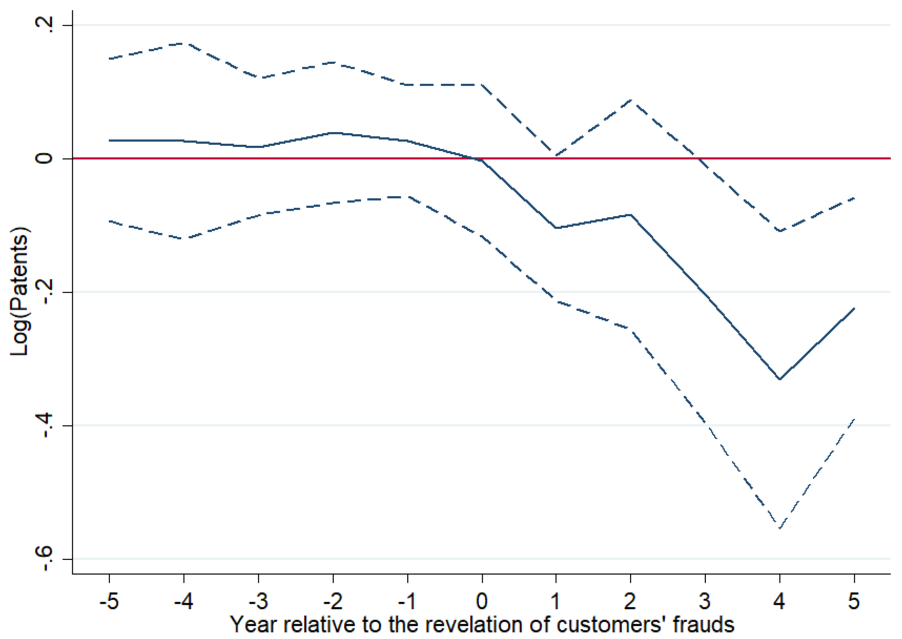


Figure 3.5 Cumulative failure rates of suppliers

This figure plots cumulative failure rates for the direct suppliers of fraud customers and the matched industry peers of treated suppliers over event year *t* to *t+10*. I define failures as performance-related stock market delistings, liquidations, and distressed mergers (delisting codes 400-490 and 520-584).

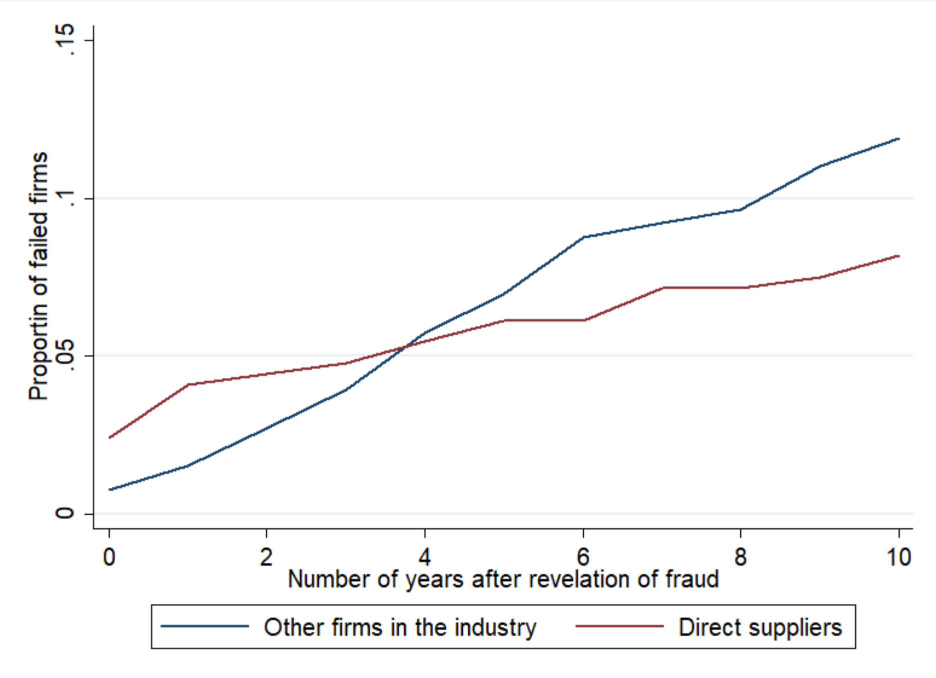


Table 3.1 Firm characteristics before the revelation of customers' frauds

This table reports summary statistics for variables used in the full sample. Variable definitions are in Appendix 3.1.

	N	Mean	Std. Dev.	Q1	Median	Q3
R&D	463,794	0.0955	0.1381	0.0000	0.0458	0.1331
Log(Patents)	423,984	0.5896	1.1551	0.0000	0.0000	0.6931
Exploitative	77,519	0.3310	0.3337	0.0000	0.2500	0.5064
Explorative	77,519	0.5987	0.3515	0.3333	0.6316	1.0000
Size	463,794	5.0239	2.2076	3.4012	4.8656	6.4837
Mtb	463,794	2.3678	2.1600	1.1696	1.6651	2.6685
Leverage	463,794	0.1214	0.1575	0.0007	0.0554	0.1872
Roa	463,794	-0.0748	0.3279	-0.1331	0.0186	0.0837
Capex	463,794	0.4449	0.5850	0.1367	0.2599	0.5055
Tangibility	463,794	0.2049	0.2084	0.0617	0.1345	0.2703
HIndex	463,794	0.2226	0.1696	0.1004	0.1822	0.2901

Table 3.2 Summary statistics for the matched sample

This table reports summary statistics for firm characteristics in the five years before the revelation of fraud. The means are reported separately for the two samples of firms. I restrict the control group to firms that are ex-ante similar to treated suppliers by matching each firm in the treatment group with firms belonging to the same quartile of size, leverage, sales, and receivables to sales at year *t-5* in the same 2-digit SIC industry. The *p*-value of the difference between treated suppliers and control firms is reported in the last column. The standard errors are clustered by SIC 2-digit industry.

	Suppliers		Control Firms		
	N	Mean	N	Mean	Difference（p-value）
R&D	1,441	0.0897	5,986	0.0880	0.0017 (0.911)
Log(Patents)	1,365	1.0916	5,646	1.0011	0.0905 (0.248)
Exploitative	656	0.3269	2,062	0.3068	0.0201 (0.193)
Explorative	656	0.5939	2,062	0.5655	0.0284 (0.120)
Size	1,441	5.9806	5,986	6.0049	-0.0243 (0.923)
Mtb	1,441	2.3794	5,986	2.1778	0.2016 (0.433)
Leverage	1,441	0.1324	5,986	0.1222	0.0102 (0.585)
Roa	1,441	-0.0155	5,986	-0.0299	0.0144 (0.182)
Capex	1,441	0.4132	5,986	0.3685	0.0447 (0.296)
Tangibility	1,441	0.2425	5,986	0.2458	-0.0033 (0.870)
HIndex	1,441	0.2120	5,986	0.2119	0.0001 (0.998)

Table 3.3 Corporate fraud and supplier firms' R&D

This table reports the stacked DID results of the matched sample. The dependent variable is R&D expense scaled by total asset. *Treated* is a dummy variable indicating affected suppliers. *Post* is an indicator variable equal to one for five years post fraud revelation and zero for five years before the fraud revelation. Firm-cohort and year-cohort fixed effects are included. The standard errors are clustered by SIC 2-digit industry. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	Dependent variable: R&D	
	(1)	(2)
Treated*Post	-0.0120**	-0.0080**
	(-2.14)	(-2.43)
Size		-0.0111**
		(-2.28)
Mtb		0.0099***
		(6.34)
Leverage		-0.0319***
		(-2.63)
Roa		-0.1039***
		(-5.43)
Capex		0.0130***
		(4.74)
Tangibility		0.1741***
		(4.05)
Hindex		-0.0084
		(-0.23)
Hindex squared		0.0197
		(0.67)
Firm*Cohort FE	Yes	Yes
Year*Cohort FE	Yes	Yes
Observations	13,467	13,467
Adjusted R^2	0.719	0.777

Table 3.4 Corporate fraud and supplier firms' innovation output

This table reports the stacked DID results of the matched sample. The dependent variable is the natural logarithm of 1 plus a firm's total number of patents filed and eventually granted. *Treated* is a dummy variable indicating affected suppliers. *Post* is an indicator variable equal to one for five years post fraud revelation and zero for five years before the fraud revelation. Firm-cohort and year-cohort fixed effects are included. The standard errors are clustered by SIC 2-digit industry. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	(1) Log(Patents)	(2) Log(Patents)
Treated*Post	-0.1502**	-0.1576***
	(-2.71)	(-2.86)
Size		0.1242***
		(4.68)
Mtb		0.0209***
		(3.97)
Leverage		-0.1330*
		(-1.74)
R&D		0.2159
		(0.94)
Roa		-0.0046
		(-0.12)
Capex		0.0759*
		(1.86)
Tangibility		0.0553
		(0.27)
Hindex		-0.3038
		(-0.56)
Hindex squared		0.2049
		(0.40)
Firm*Cohort FE	Yes	Yes
Year*Cohort FE	Yes	Yes
Observations	12,635	12,635
Adjusted R^2	0.896	0.898

Table 3.5 Innovation style

This table reports the stacked DID results of the matched sample. The dependent variable in column (1) and (2) is the number of exploitative patents divided by the number of patents of a firm in a fiscal year. The dependent variable in column (3) and (4) is the number of explorative patents divided by the number of patents of a firm in a fiscal year. Treated is a dummy variable indicating affected suppliers. Post is an indicator variable equal to one for five years post fraud revelation and zero for five years before the fraud revelation. Firm-cohort and year-cohort fixed effects are included. The standard errors are clustered by SIC 2-digit industry. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	(1) Exploitative	(2) Exploitative	(3) Explorative	(4) Explorative
Treated*Post	-0.0441**	-0.0438**	0.0443**	0.0473**
	(-2.53)	(-2.51)	(2.02)	(2.08)
Size		0.0190		-0.0123
		(1.06)		(-0.71)
Mtb		-0.0011		0.0058
		(-0.20)		(0.78)
Leverage		0.1027**		-0.1062*
		(2.52)		(-1.94)
R&D		0.0854		0.0062
		(0.63)		(0.04)
Roa		0.0136		-0.0501
		(0.42)		(-1.52)
Capex		0.0723**		-0.0511*
		(2.79)		(-1.91)
Tangibility		-0.0650		0.0888
		(-0.40)		(0.49)
Hindex		0.2328		-0.1963
		(0.72)		(-0.55)
Hindex squared		-0.2279		0.2691
		(-0.64)		(0.75)
Firm*Cohort FE	Yes	Yes	Yes	Yes
Year*Cohort FE	Yes	Yes	Yes	Yes
Observations	4,334	4,334	4,334	4,334
Adjusted R^2	0.409	0.417	0.421	0.429

Table 3.6 Technology proximity with fraudulent customers and non-fraudulent customers

This table reports the affected suppliers' technology proximity with fraudulent customers and non-fraudulent customers in the prior and post fraud revelation period. For each affected supplier, its customers are identified in the year of the fraud revelation and sorted into fraudulent customer group and non-fraudulent customer group. In Panel A, the univariate results are reported. In Panel B, regression results are reported, *t*-statistics are in paratheses. *Fraudulent Customer* is one if the customer firm's fraud is revealed and zero otherwise. *Post* is one for up to five years post the fraud revelation, and zero for up to five years prior to the fraud revelation. Detailed variable definition is in Appendix A1. The standard errors are clustered by SIC 2-digit industry.

Panel A: Technology proximity univariate analysis

Technology Proximity	Before Fraud Announcement	After Fraud Announcement	Difference
Fraudulent Customers	0.4347	0.3523	-0.0824** (-2.29)
Non-fraudulent Customers	0.3951	0.4047	0.0096 (0.41)
Difference	0.0396 (1.41)	-0.0524* (-1.69)	-0.0920** (-2.11)

Panel B: Technology proximity regression analysis

	(1)	(2)
Fraudulent Customer	0.0396	0.0401
	(1.38)	(1.32)
Post	0.0096	0.0062
	(0.40)	(0.26)
Fraudulent Customer * Post	-0.0920**	-0.0900**
	(-2.11)	(-2.07)
Controls	No	Yes
Observations	1,750	1,750
Adjusted R^2	0.004	0.066

Table 3.7 Small suppliers

This table reports the stacked DID results of small suppliers on R&D, innovation output, and innovation style. "Small" is an indicator variable taking the value of one if the average size of the supplier to the size of the customer is below the median during the five years prior to the revelation of customers' fraud. In column (1) and (2) of Panel A, the dependent variable is R&D spending scaled by total assets. In column (3) and (4) of Panel A, the dependent variable is the natural logarithm of one plus the number of patents in each year. In column (1) and (2) of Panel B, the dependent variable is the number of exploitative patents divided by the number of patents of a firm in a fiscal year. In column (3) and (4) of Panel B, the dependent variable is the number of explorative patents divided by the number of patents of a firm in a fiscal year. *Treated* is a dummy equal to one for affected suppliers. *Post* is a dummy equal to one for five years post fraud revelation and zero for five years before the fraud revelation. Firm-cohort and year-cohort fixed effects are included. The standard errors are clustered by SIC 2-digit industry. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Panel A: R&D and innovation output

	(1) *R&D*	(2) *R&D*	(3) *Log(Patents)*	(4) *Log(Patents)*
Treated*Post	-0.0043	-0.0002	-0.0801*	-0.0953**
	(-1.18)	(-0.06)	(-1.87)	(-2.09)
Treated*Post*Small	-0.0164***	-0.0167***	-0.1400**	-0.1260*
	(-3.05)	(-3.32)	(-2.32)	(-1.78)
Firm*Cohort FE	Yes	Yes	Yes	Yes
Year*Cohort FE	Yes	Yes	Yes	Yes
Control Variables	No	Yes	No	Yes
Observations	13,467	13,467	12,635	12,635
Adjusted R^2	0.719	0.774	0.896	0.898

Panel B: Innovation style

	(1) Exploitative	(2) *Exploitative*	(3) *Explorative*	(4) *Explorative*
Treated*Post	-0.0385**	-0.0343*	0.0416**	0.0429*
	(-2.01)	(-1.84)	(2.04)	(1.96)
Treated*Post*Small	-0.0464*	-0.0417*	0.0552**	0.0539*
	(-1.92)	(-2.17)	(2.19)	(1.78)
Firm*Cohort FE	Yes	Yes	Yes	Yes
Year*Cohort FE	Yes	Yes	Yes	Yes
Control Variables	No	Yes	No	Yes
Observations	4,334	4,334	4,334	4,334
Adjusted R^2	0.409	0.417	0.421	0.429

Table 3.8 Linear probability and probit models of survival

This table presents the results from linear probability and probit models of survival of treated suppliers and their matched industry peers after the revelation of customer fraud. The unit of observation is firm-year. In column (1) and (2), the results of the linear probability model are reported. In column (3) and (4), the results of the probit model are reported. The dependent variable is a dummy variable which equals one if a firm fails in the next year and zero otherwise. Other variable definitions are in Appendix A. In column (1) and (3), the analysis examines survival likelihood in the first three years after the fraud revelation. In column (2) and (4), the analysis covers the sub-period starting from year four after the revelation of customer's fraud. *P*-values are reported in parentheses. Standard errors are clustered by SIC 2-digit industry. Industry fixed effects and year fixed effects are included. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	(1) LPM	(2) LPM	(3) Probit	(4) Probit
	<=3 years	>3years	<=3 years	>3years
Treated	0.0186**	-0.0113***	0.6852***	-0.9922**
	(0.042)	(0.008)	(0.001)	(0.012)
Size	-0.0072***	-0.0051***	-0.3857***	-0.2307***
	(0.000)	(0.000)	(0.000)	(0.000)
Mtb	0.0000	0.0021	-0.1084	-0.0400
	(0.988)	(0.127)	(0.160)	(0.514)
Leverage	0.1229***	0.1479***	3.1626***	3.2761***
	(0.000)	(0.000)	(0.000)	(0.000)
Roa	-0.0153**	-0.0218*	-0.3071***	-0.4030***
	(0.041)	(0.059)	(0.006)	(0.008)
Capex	-0.0015	-0.0036	0.0167	-0.6924**
	(0.609)	(0.105)	(0.912)	(0.014)
Tangibility	-0.0122	-0.0178**	-0.7968	-0.8218
	(0.252)	(0.026)	(0.209)	(0.121)
Hindex	0.0077	-0.0014	0.1230	-0.0385
	(0.641)	(0.836)	(0.835)	(-0.09)
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	2,688	3,958	2,688	3,958

Table 3.9 Firm survival and explorative vs. exploitative innovation

This table presents the results from regressions of survival analysis on treated suppliers and their industry peers after the revelation of customers' fraud. In panel A, I report the results of the matched sample. In panel B, I report the results of the full sample. *Exploit* (*explore*) is the natural logarithm of one plus the percentage of cumulative number of exploitative (explorative) patents after the revelation of customers' fraud. *P*-values are reported in parentheses. Standard errors are clustered by SIC 2-digit industry. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	Survival Analysis		Failure (1/0)
	(1) Cox	(2) Weibull	(3) LPM
Panel A			
Exploit	1.3160	1.2358	0.0055
	(0.194)	(0.140)	(0.134)
Explore	0.4512**	0.4463**	-0.0132***
	(0.021)	(0.020)	(0.007)
Treated	0.5813*	0.5761*	-0.0037
	(0.058)	(0.058)	(0.160)
Size	0.6725***	0.6700***	-0.0046***
	(0.000)	(0.001)	(0.000)
Mtb	0.8206***	0.8198***	-0.0011**
	(0.003)	(0.003)	(0.013)
Leverage	2.4277***	2.4382***	0.0303***
	(0.000)	(0.000)	(0.000)
Roa	0.6224***	0.6226***	-0.0187***
	(0.000)	(0.000)	(0.004)
Capex	0.6761	0.6719***	-0.0050**
	(0.190)	(0.182)	(0.035)
Tangibility	1.4721	1.5063	-0.0104
	(0.669)	(0.654)	(0.132)
Hindex	1.9028	1.9181	0.0042
	(0.337)	(0.339)	(0.609)
Year FE	No	No	Yes
Industry FE	Yes	Yes	Yes
Observations	7,595	7,595	7,595
Panel B			
Exploit	1.1637	1.1262	0.0091
	(0.288)	(0.269)	(0.119)
Explore	0.5284***	0.5161***	-0.0263***
	(0.000)	(0.000)	(0.000)
Treated	0.5425***	0.5339***	-0.0095***
	(0.000)	(0.000)	(0.000)
Year FE	No	No	Yes
Industry FE	Yes	Yes	Yes
Observations	31,639	31,639	31,639

Table 3.10 Cross-section regression: Survival over a ten-year horizon

This table reports the results of cross-sectional linear probability, probit regression, and a two stage approach. In Models (1), (2), (4), and (5), the dependent variable is one if a firm has failed in the 10 years after the revelation of customer fraud, otherwise, it is zero. In Models (1) and (2), I report the results of the linear probability model and probit model respectively. Model (3) present the first stage regression of the determinant of explorative innovation. Models (4) and (5) present the second stage regression of the failure on the explorative innovation instruments obtained from the first stage. Explore is the natural logarithm of one plus the total percentage of the explorative patents up to five years after the revelation of customer fraud. Principle customer sales ratio is the total sales to the principle customers to supplier's total sales before the revelation of fraud. The change in sales to fraud customer is the difference between the annualized cumulative sales to fraud customers up to five years after the fraud event (before its exit) and the sales to fraud customers in the year before the revelation of fraud, scaled by the sales of the suppliers before the revelation of fraud. All explanatory variables are measured in the year before the revelation of customer fraud. *P*-values are reported in parentheses. Standard errors are clustered by SIC 2-digit industry. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Dependent variable =	Failure Model (1) LPM	Failure Model (2) Probit	Explore Model (3) 1st stage	Failure Model (4) 2nd stage IV LPM	Failure Model (5) 2nd stage IV Probit
Treated	-0.0124*	-0.1075*	0.0457***		
	(0.066)	(0.072)	(0.009)		
Explore				-0.4995**	-0.5551***
				(0.016)	(0.000)
Change in sales to fraud customer			0.0483**	-0.2532**	-0.2791*
			(0.020)	(0.015)	(0.062)
Principle customer sales ratio			-0.0641***	0.1739**	0.5566***
			(0.005)	(0.040)	(0.000)
Size	-0.0281***	-0.1892***	-0.0087***	-0.0222*	-0.0698*
	(0.000)	(0.000)	(0.008)	(0.056)	(0.051)
Mtb	-0.0054***	-0.1417***	-0.0006	-0.0018	-0.0075
	(0.005)	(0.005)	(0.378)	(0.404)	(0.373)
Leverage	0.0488	0.4227***	0.0209*	0.0109	0.0117
	(0.156)	(0.004)	(0.062)	(0.750)	(0.904)
Roa	-0.0382	-0.0850	0.0372**	-0.1210***	-0.2822***
	(0.504)	(0.726)	(0.012)	(0.009)	(0.001)
Capex	0.0620	0.5347*	-0.0934***	0.1255	0.4537***
	(0.289)	(0.064)	(0.000)	(0.139)	(0.003)
Tangibility	0.0035	-0.0672	0.0646*	-0.0693	-0.2242
	(0.969)	(0.890)	(0.080)	(0.513)	(0.394)
Hindex	0.0761	0.4478	0.0246	0.0470	0.0658
	(0.286)	(0.263)	(0.472)	(0.558)	(0.790)
R&D			0.0119		
			(0.414)		
Industry FE	Yes	Yes	Yes	Yes	Yes
Observations	923	876	923	923	876

Table 3.11 Fraud revelation and diversification of suppliers' customer base

This table reports DID estimation results in the full sample (column (1) and (2)) and the matched sample (column (3) and (4)). In column (1) and (3), the dependent variable is the number of important customers for each supplier in a year. In column (2) and (4), the dependent variable is the natural logarithm of one plus the number of important customers for each supplier in a year. Firm-corhot and year-cohort fixed effects are included. Standard errors are clustered by SIC 2-digit industry. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)
Treated*Post	0.1257*	0.0302*	0.3173**	0.0711**
	(1.74)	(1.81)	(2.35)	(2.40)
Size	0.0087	-0.0069***	0.0042	-0.0073
	(1.19)	(-2.80)	(0.06)	(-0.32)
Mtb	0.0145***	0.0041***	0.0216	0.0056
	(6.52)	(5.49)	(1.08)	(0.75)
Leverage	-0.0945***	-0.0323***	-0.2317	-0.0500
	(-4.40)	(-4.95)	(-1.20)	(-1.04)
Roa	0.0265*	0.0086*	0.2994*	0.0645
	(1.70)	(1.66)	(1.89)	(1.42)
Capex	0.0113**	0.0035*	0.0202	0.0096
	(2.06)	(1.76)	(0.41)	(0.60)
Tangibility	0.3023***	0.0818***	-0.1261	-0.0642
	(7.10)	(6.02)	(-0.25)	(-0.58)
Hindex	-0.6052***	-0.0765	1.1673	0.8428*
	(-2.81)	(-1.12)	(0.62)	(1.67)
Hindex squared	0.4191**	0.0610	-2.1920	-1.0196*
	(1.99)	(0.80)	(-1.15)	(-1.90)
Firm*Cohort FE	Yes	Yes	Yes	Yes
Year*Cohort FE	Yes	Yes	Yes	Yes
Observations	130,623	130,623	4,116	4,116
Adjusted R^2	0.479	0.453	0.594	0.504

Table 3.12 Supplier sales growth

This table reports the stacked DID results of the matched sample. The dependent variable is the growth in sales adjusted for industry median sales in the same SIC 2-digit industry. Growth in industry adjusted sales is computed each year relative to year (t = -6). Year t = 0 is the revelation year. *Treated* is a dummy variable indicating affected suppliers. *Post* is an indicator variable equal to one for five years post the revelation of customer fraud and zero for five years before the revelation. Firm-cohort and year-cohort fixed effects are included. The standard errors in column (1) are adjusted for heteroskedasticity (White, 1980). The standard errors in columns (2) are clustered by SIC 2-digit industry. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

	(1) Adjusted sales growth	(2) Adjusted sales growth
Treated*Post	0.2005 ***	0.1771**
	(3.44)	(2.20)
Size		0.8970***
		(7.05)
Mtb		0.0547**
		(2.08)
Leverage		-0.0138
		(-0.03)
Roa		0.1340
		(0.72)
Capex		-0.2065
		(-0.48)
Tangibility		-0.4999**
		(-2.51)
Hindex		-2.6069*
		(-1.91)
Hindex squared		2.6408**
		(2.48)
Firm*Cohort FE	Yes	Yes
Year*Cohort FE	Yes	Yes
Observations	13,275	13,275
Adjusted R^2	0.295	0.347

Chapter 4

Predatory Advertising, Financial Fraud, and Leverage

4.1. Introduction

In the context of product markets, *predatory behavior* usually refers to aggressive strategic behavior by an incumbent or a dominant firm to deter entry or drive out a weaker firm from business. Theoretical foundations for various aspects of the idea have been provided by McGee (1958) and Telser (1966), who explore the so-called "deep pockets" argument; by Selten (1978), Milgrom and Roberts (1982), Kreps and Wilson (1982), and Benoit (1984) who motivate such behavior from the perspective of incumbent reputation and entry deterrence; and by Fudenberg and Tirole (1985), Poitevin (1989), Benoit (1984) and Bolton and Scharfstein (1990), who show that a rival firm's financial condition and/or financial market imperfections can encourage predatory behavior. In a recent paper, Wiseman (2017) proves an "anti-folk" theorem: in a model of repeated competition among oligopolists, if the firms are sufficiently patient, predatory price wars arising from differences in financial conditions of firms occur very early. Wiseman also provides several historically important anecdotes of predatory behavior. Chen et al. (2019) model a dynamic duopoly with the possibility of default. They show that the possibility of default can soften future punishments, and thus weaken the conditions for collusion. When a financially weak firm's condition further worsens, the rival firm lowers its price, which in turn pushes the weak firm further towards bankruptcy.

Despite these theoretical foundations, empirical evidence on predatory behavior is limited. One reason for this is that predatory behavior is extremely difficult to identify. Predatory pricing could occur for a small range of products produced by the firm aimed at a particular segment of the market it operates in, and the relevant costs are difficult to observe. Moreover, a dominant firm could be persistently charging lower prices than its rivals, but it is notoriously hard to gauge whether this is because the intent is to drive out rivals or because firm fundamentals are different.[46] Given this difficulty, the most promising empirical strategy is to examine the impact of exogenous events that could trigger predation. However, these exogenous events need to be such that they directly affect only a subset of firms in an industry,

[46] See the "dumping" literature in international trade (Ethier, 1982; Brander and Krugman, 1983).

so that the responses of the remaining firms could be studied to detect evidence of predation. Generally, such events are difficult to observe (Chevalier, 1995a and 1995b are exceptions).

A second reason why it has been difficult to identify predation is that most of the theoretical literature has focused on product prices as the key strategic variable, and firm-level product price data is generally not available, except for a limited set of industries (e.g., the supermarket industry (Chevalier, 1995a). Absent price data, some studies have focused on plant closings/openings and investment decisions (Kovenock and Phillips, 1997; Bharath, Dittmar, and Sivadasan, 2014). Other studies have mostly relied on indirect evidence – for example, Billett, Garfinkel, and Yu (2017) examine whether firms lose market share relative to rivals when they lose analyst coverage for exogenous reasons (consistent with the view that greater information asymmetry encourages predation), and Kini, Shenoy, and Subramaniam (2018) find that firms that recall products have more adverse stock price reactions around announcement when they are more levered and operate in more concentrated markets (suggesting that competitors prey on financially weak rivals).

In this paper, I examine a strategic variable for which data is readily available for about 40% of Compustat firms – namely, firms' advertising expenditures. The economics of advertisement spending has a long history dating back to the late 19th century in the works of Marshall (1890 and 1919) and further developed by Chamberlin (1933). By the close of the 20th century, substantial literature had developed to provide both a positive and normative analysis of advertising. Bagwell (2007) provides a comprehensive survey of this literature. Although the nature of advertising (e.g., whether it is *persuasive*, *combative*, or *informative*) and consequently its impact on price and profit margins is still not a settled issue, it is regarded as a key strategic variable that firms choose, along with price, to affect market share. To the best of my knowledge, however, the role of advertising expenditure as a predatory mechanism has not been investigated in the literature.

The triggering events around which I examine changes in advertising behavior of rival firms are financial frauds committed by major firms (S&P 500 constituents). To identify the first date of public revelation of a fraud, I follow methods similar to Karpoff, Koester, Lee, and Martin (2017), described in detail in Section 4.2. These frauds are associated with major declines (to the order of 20%) in the market values of the fraudulent firms, and are thus major shocks to their leverage ratios as well. Moreover, these stock price declines at least partially represent trust or reputational problems (Karpoff, Lee, and Martin, 2008) and thus are likely to exacerbate financial constraints (i.e., problems in raising external finance) faced by these firms. My setting is therefore somewhat different from the typical context in which predation has been discussed, which is that of a financially strong firm with a deep purse driving a financially weak rival with a small purse out of business. However, recent theoretical analysis of predation makes no assumptions about the relative firm size of financially strong and weak firms (Wiseman, 2017; Chen et al., 2019), and many historically documented cases of predation involve major industry players engaged in price wars (Wiseman, 2017).

In this setting, I study whether peer firms with very similar products (specifically, in the same TNIC3 product similarity group constructed by Hoberg and Phillips (2010, 2016)) – i.e., close competitors of the fraud firm -- step up their advertising spending subsequent to the revelation of financial fraud. Since I do not have access to detailed product price data, to examine pricing strategy, I consider the profit margin (Finkel and Tuttle, 1971; Ferreira and Matos, 2008). My estimation is a multi-event difference-in-difference setting, in which the control firms are drawn from the same TNIC2 group. The TNIC2 group classification is a coarser classification of firms based on product similarity than the TNIC3 group (i.e., firms are classified based on lower product similarity). These control firms are matched to the TNIC3 firms on the basis of several firm characteristics. In my test design, both peer and control firms are thus chosen from the same TNIC2 group to absorb industry trends that could affect advertisement spending. It is worth noting that to the extent that TNIC2 firms also respond to some degree to competitive opportunities similar to TNIC3 firms, this design would bias *against* finding my results.

To understand how one should expect prices and advertisement to respond to a financially weakened rival, I examine a two-period switching cost model in a duopoly market, as in Klemperer (1995) and Chevalier and Scharfstein (1996). In this model, if product differentiation is sufficiently high, a rival firm lowers its price and steps up advertisement spending when its competitor is financially weakened as its fraud is revealed. I do not model the financial shock directly, but assume that the fraud firm becomes more myopic as it must increase current period profits to meet interest payments and survive. Consequently, in equilibrium, it raises its current period price and cuts down on advertisement spending. Even though in the typical switching cost model prices are strategic complements, the rival firm in my model lowers its price and steps up advertisement spending. A crucial driver of this result is that the rival firm expects to gain at the expense of the financially weakened firm in the future period – possibly via aggressive tactics such as accelerating the launch of new products, poaching critical employees, or switching suppliers. In other words, the aggressive pricing and advertising in the initial period is a reflection of future aggressive intent. I show that the net effect could be to increase the losses incurred by the rival firm in the current period at the expense of higher profits in the future. The fact that for both firms, price-cost margins and advertisement spending move in opposite directions is also unusual[47] (but consistent with my findings, as discussed below). The model generates additional implications which are broadly consistent with my findings.

I now turn to my main findings. I find that relative to the control group, rival (peer) firms that are in the same TNIC3 group as the industry-leading fraud firm increase advertising in the three years following the first public revelation of fraud (compared to three years before). I also find that profit margins fall (or do not increase as much relative to control firms). This initial evidence is consistent with my model, but I note that prices going down in response to more advertising could also reflect the fact that advertising is informative, which makes demand curves more elastic and could lower prices (Butters, 1977; Stahl, 1989).

I find that the change in advertising is larger when: 1) the fraud firm's leverage is relatively high compared to the leverage of the unaffected peers, 2) the average industry leverage of the peer firms is lower, and 3) the TNIC3 industry, excluding the fraud firm, is more concentrated. The first two of these results are consistent with the idea of "deep pockets" theories that the incentive to predate is higher when the prey is financially weaker (weakened) and the predators are not as financially constrained. Both Wiseman (2017) and Chen et al. (2019) also show this theoretically, although they only consider price as a strategic variable. The third result possibly reflects the fact that in more concentrated industries, rival firms will have higher market share gains if the prey goes out of business, and this might incentivize predation.

I also find that profit margins drop (or do not increase as much) when the industry is more concentrated, which further suggests that the incentive to predate leads to changes in advertisement and profit margins in opposite directions. However, higher fraud firm leverage mitigates the drop in profit margins. Since rival firms' advertisement increase more when the fraud firm's leverage is higher, it is possible that this creates opportunities for reaping scale

[47] Hall (2014) argues that price-cost margins or markups and advertisement should move in the same direction over the business cycle since higher profit margins increase the marginal return from advertisement.

economies in advertisement. If advertisement spending gets an added boost via such scale economies, rival firms' profit margins can increase.

Next, I examine whether the nature of the industry matters for the predatory response of the peer firms. The advertising literature recognizes that the nature of advertising (i.e., whether persuasive or informative) could depend on the type of product or industry.[48] In the context of predation, in the spirit of my model, I argue that a different perspective could be useful, especially when predation need not lead to the financially weakened firm exiting the industry.[49] The incentive to attract the rival firm's customers exists in this situation only if the customers have switching costs (Klemperer, 1987 and 1995). Switching costs imply that customers would stay with a firm whose products they become familiar with, so once they switch to another firm, they continue to remain loyal to this firm. Therefore, to switch the fraud firm's customers in an industry with high switching costs, rival firms may have to increase advertisement and/or lower prices. Moreover, if switching costs are low, it may not be profitable to spend money to attract new customers, as these customers would not readily develop loyalty and could switch back later. Consistent with this hypothesis, I find that the rival peer firms increase advertisement spending, and their profit margins fall, only in industries with high switching costs.

I next examine whether the lower profit margins associated with higher advertisement spending of the peer firms imply that price is also used as strategic tools for switching customers away from the financially impaired firm (recall that lower prices could also result from more informative advertising). To do so, I separately examine markets with high and low recent sales growth in high switching cost industries. I appeal to my model to argue that when a significant number of *new* customers enter the market, since these customers have not developed loyalties to particular firms/products yet, rival peer firms are likely to rely on advertising to attract these customers, knowing that the financially impaired major industry leader would not be able to correspondingly step up advertisement and compete for these customers. In contrast, when new customer growth is low, in a high switching cost industry, it may be difficult to switch loyal customers via advertisement. Therefore, customers of the industry leader may have to be provided significant pricing discounts to switch (predatory

[48] Rauch (2013) is a recent paper showing that the association between advertising and price is positive in industries where advertisement is likely to be persuasive, and negative when it is likely to be informative.
[49] Indeed, none of the fraud firms in my sample exit the industry in the next five years, which is not very surprising, given that these are industry leaders.

pricing). Consistent with these arguments and my model, I find that rival firms' advertisement spending increases only in high switching cost industries when recent sales growth is high, and the effect is stronger if the fraud firm's leverage is higher. Margins also fall, but this effect is mitigated if the fraud firm is more highly levered. In contrast, in high switching cost industries with low recent sales growth, margins fall, and margins are even lower when the fraud firm's leverage is higher. These results suggest that advertisement and price both serve the purpose of attracting customers away from the financially impaired firm by its rivals. Stepping up advertisement is the more effective strategy when new customer growth is significant, whereas lowering price is more effective in attracting existing customers in markets without significant new customer growth.[50]

It is important to note that there could be alternative explanations of why rival firms step up advertisement spending when a major firm in the same industry is revealed to have committed fraud. For example, firms might want to project a positive image to customers and market participants and step up advertising when such an event occurs. However, there is no clear reason why this incentive should be greater in industries with high switching costs – if anything, the incentive should be less if a firm already has loyal customers. There is also no reason why this "image building" incentive should be greater for peer firms if the fraud firm has higher leverage (or higher leverage relative to the industry average).

My paper makes the following contributions to the literature. To the best of my knowledge, there is no large-sample study directly examining predatory pricing or predatory advertising behavior that encompasses multiple industries.[51] I am also not aware of any work that examines the predatory role of advertising expenses, nor one that finds differences in predation incentives in high versus low switching cost industries. I also document that advertising and pricing, as alternative tools for predation, play somewhat different roles in industries experiencing new customer growth versus those that have a more stable customer base. What is particularly interesting in my setting is that the predatory advertising is done by firms that

[50] The fact that profit margins drop significantly in the absence of advertisement going up when new customer growth is low suggests that the price decline is not due to an increase in informative advertising. It is also unclear why advertising should be more informative for low customer growth industries.

[51] As discussed above, Chevalier's seminal work (Chevalier, 1995a and 1995b) utilizes detailed data on product prices and directly examines how leveraged buy-out (LBO) decisions by supermarket chains affects the pricing behavior of firms (supermarket stores in local markets). Chevalier finds evidence that when major rivals of the LBO firm are relatively under-levered, industry prices decline. This is consistent with financially unencumbered firms predating on firms that are unable to match price cuts (which would involve sacrificing current profits to protect future market share) in a switching cost-type market.

are *smaller* than the prey, since the fraud firm is an S&P 500 constituent. This is different from what most of the early theoretical literature has assumed, where the predators are dominant firms. However, I believe that mine is a highly plausible setting in that the pool of customers available for predation has to be sufficiently large for me to detect predation by rivals.

4.2. Related Literature

In this section, I focus on the recent financial economics literature that explores how information asymmetry and financial constraints can subject firms to predation. Since McGee's (1958) influential work, it has been recognized that the "long purse" or "deep pockets" argument that a large firm with plentiful financial resources could drive out a small firm with more limited resources by incurring losses requires some assumption about financial imperfection – otherwise the small firm could always persuade a bank, for example, to commit to providing finance for an indefinite period and predation would no longer occur in equilibrium. Models showing that financial imperfections (e.g., information asymmetry between external providers of finance and the firm) can invite predation have been proposed by Benoit (1984) and Bolton and Scharfstein (1990).

The empirical literature in financial economics has tended to focus on the notion that adverse shocks to leverage can make a firm vulnerable to predation by rivals.[52] Chevalier (1995a) was the first paper to utilize product price data and examine how firms change their pricing strategies when a firm in the industry becomes highly leveraged due to a leveraged buyout or a leveraged recapitalization. She examines the supermarket industry, and exploits local variation in market characteristics under the assumption that the firm-level change in leverage is not related to local market heterogeneity. Chevalier (1995a) finds that product prices rise in a local area following an LBO when there are other LBO firms operating in that area. However, when a major rival in the local market has low leverage, prices in that market drop. Chevalier (1995a) interprets this finding as evidence of predation – conservatively financed rivals lower prices to capture market share from LBO firms, knowing that the latter would be unable to match those price cuts as they need to keep current profits high (the implicit

[52] The literature does not clearly link the effects of such large leverage changes with financial market imperfections. However, higher leverage commits firms to larger fixed payments, which limits their ability to sacrifice short-term profits. Financial imperfections could be one reason why firms cannot quickly rebalance their leverage ratios, or raise new financing.

assumption is that the market is one in which consumers have "switching cost", and firms sacrifice current profits to build future market share).

Several papers find indirect evidence in support of predation. Opler and Titman (1994) find that when there are adverse shocks to industry sales and stock prices, firms that are more highly levered and produce more customized products lose market share to their rivals. These effects are stronger in concentrated industries, where rival firms have the strongest incentives to prey. Kovenock and Phillips (1997) examine ten manufacturing industries in which at least one of the top four firms recapitalize via an LBO or a financial recapitalization. They find that the competitive conditions in the market have an important impact on the subsequent investment (plant addition/closing) decisions of the firm undergoing the large leverage increase as well as those of the rival firms. Specifically, the highly levered firm is more likely to close plants and less likely to open new plants when the market is more concentrated, whereas the effect on the rivals is exactly the opposite when the firm's market share is larger. Zingales (1998) considers the effect of deregulation in the late 1970s on the trucking industry. He finds that firms that had higher leverage prior to the deregulation charged lower prices per ton-mile; however, this effect was entirely concentrated in the less competitive segment of the market. This evidence is consistent with highly leveraged firms facing predatory pricing by less leveraged rivals in the more concentrated and less standardized segment of the market, where the benefits of predation can be recouped.

More recently, Haushalter, Klasa, and Maxwell (2007) examine whether firms hold more cash and hedge more when they are subject to more "predation risk". The latter is captured in several ways – industry concentration, and the extent of the interdependence of a firm's investment opportunities with its rivals – measured in terms of the absolute deviation of the firm's capital-labor ratio from the industry median, and the correlation of the firm's return with the industry return. They find that the higher the predation risk, the larger the size of cash holdings and the currency swap usage. They find that when the industry-wide investment decreases, firms are more likely to increase their capital expenditure and R&D expenditure if they are cash abundant and face a higher level of predation risks. Bernard (2016) examines whether predation risk (proxied by leverage) influences the disclosure decisions of firms and argues that financially constrained firms avoid disclosing financial information to lessen predation risk. Utilizing a regulatory change in Germany that requires all private firms to publish certain annual financial statements, he finds that financially constrained firms (which are more vulnerable to predation risk) are more likely to avoid disclosure before the regulatory change. He finds that after the regulatory change, the most constrained firms disclosing their financial information experience the largest decrease in their market shares, fixed assets, and cash holdings.

Examining Korean business groups, Kim (2016) finds that high business group leverage has a negative impact on the product market performance (sales growth) of group-affiliated firms, and thus group-affiliated firms lose market shares to their rivals. Moreover, this negative impact is more pronounced for affiliated firms which are financially weak (less profitable, smaller size, less cash, and less tangible assets), and in fast-growing industries where rivals could potentially benefit from taking advantage of growth opportunities. Cookson (2017) examines entry and incumbent investments in the U.S. Casino industry. He finds a negative relation between incumbent investments and the likelihood of entry, specifically, high financial leverage hinders the strategic response of the incumbent casino firms to nearby entry threats. By contrast, low-leverage incumbent firms expand physical capacity to pre-empt entry. The value of the incumbent firms increases by 5% after an effective pre-emption. Cookson (2017) concludes that the relation between leverage and competition is stronger than previous literature has recognized, as leverage matters for incumbent firms' investment decisions even before competitors enter the market. Using a difference-in-difference test around the brokerage house closure/merger events, Billett, Garfinkel, and Yu (2017) find that firms that experience drop in analyst coverage lose market shares, compared with unaffected firms. They argue that brokerage house closure/merger results in greater information asymmetry between investors and affected firms, thus leading to stronger predation from their competitors. Moreover, their findings are stronger for affected firms with greater agency problems (low institutional monitoring), firms that are financially constrained, and firms with greater asymmetric information (opaque financial statements and less followed by financial analysts). Kini, Shenoy and Subramaniam (2018) examine whether a firm's leverage relative to its rivals can explain the announcement period returns of product recalls for the recalling firms, their industry rivals, and their key suppliers. They find that when a recalling firm is highly leveraged, its rivals can benefit from the recall and experience higher abnormal returns (by comparison, recalling firms' suppliers experience negative abnormal returns). Importantly, the positive abnormal returns for rivals and the negative abnormal returns of the recalling firm come from concentrated industries where rivals' predation-related benefits are high. Finally, EI Ghoul, Guedhami, Kwok, and Zheng (2018) hypothesize that the strong creditor rights increase the costs of high leverage through increasing the adverse responses of customers and competitors, and find consistent evidence for a sample of global firms.

4.3. Predatory Pricing and Advertising in an Industry with Switching Costs

4.3.1 Conceptualizing predation

In this section, I outline a model to derive implications for product pricing and advertising, which I later take to the data. In the process, I clarify the sense in which strategies of a rival can be considered to be predatory, in a context where the objective is not necessarily to drive the financially impaired competitor out of business (which is unlikely when the competitor is a major industry leader), but rather, to gain at the expense of the competitor when it is financially weaker.

The traditional notion of predation (McGee, 1958; Telser, 1966) has been that of a large firm with ample financial resources (deep pockets) charging below-cost prices that compel a smaller competitor with shallow pockets to sustain losses to stay in the market. Such a predatory strategy is aimed at forcing the shallow pocket firm to exit. Subsequent theoretical research (e.g., Bolton and Scharfstein (1990)) has formalized the notion that predation is more likely if the prey is financially constrained.

In Bolton and Scharfstein (1990), predatory pricing is not explicitly modeled; however, it is assumed that by spending resources, the predator can make it more difficult for the financially constrained prey to obtain financing and remain in the market in future periods. Chevalier (1995a) interprets her evidence from LBOs in the supermarket industry more broadly: she finds that when in a local market a major rival has low leverage, prices fall subsequent to an LBO by a competing firm. This is considered “predatory” pricing in the following sense. In a switching cost industry, the LBO firm’s price is expected to rise following the LBO (Chevalier and Scharfstein, 1996; Dasgupta and Titman, 1998). Since prices are strategic complements, a rival firm’s price is also supposed to increase following the LBO by an industry competitor. However, if the rival’s price *falls*, then the rival is sacrificing current profits to gain at the expense of the LBO firm – e.g., by drawing customers away since the LBO firm is unable to sacrifice current profits by matching the price cut (as it has to make higher interest payments).[53] It is in this sense that the pricing strategy of the rival firm predatory – it is giving up (more) profits today in return for higher profits in the future, which invariably would come at the expense of the financially constrained LBO firm.

Chevalier (1995a), however, does not provide a theoretical model that shows under what conditions a shift to a predation equilibrium can occur, as opposed to the equilibrium in which both firms raise prices (Chevalier and Scharfstein, 1996; Dasgupta and Titman, 1998). A mere willingness or ability to sacrifice current profits is insufficient, since in the typical equilibrium, both firms still raise prices when the rival has not undergone an LBO. Next, I outline a two-period duopoly model in which, following the financial impairment of one of the firms (the fraud firm in my context), the rival firm lowers its product price and increases its advertisement spending, whereas the impaired firms do exactly the opposite. I show that, in the process, the rival sacrifices more in terms of losses in the first period and gains in the future period as the financial impairment of the fraud firm becomes more severe.

4.3.2 A model of predation

In this section, I present a model in the framework of Klemperer's (1995) two-period switching cost model,[54] in which two firms compete in a differentiated goods duopoly. Both firms compete in the first as well as the second period, i.e., there is no exit. This is consistent with my empirical setting in which the financially impaired fraud firm is a large industry leader that is very unlikely to exit. In the first period, firms simultaneously choose prices and advertisement expenses. In the second period, switching costs set in for consumers who have already consumed in the first period. This allows firms to set prices in the second period that are at (or, close to) the reservation utilities of consumers and extract the consumers' surplus.

I model the financial impairment of the fraud firm by assuming that following financial impairment, the firm is forced to lower the weight on its second-period profit since it must become more myopic and survive in the short-term. Crucially for my results, I also assume that the financial impairment of the fraud firm improves the profit margin (or price minus cost in my model) of the rival in the second period, while that of the fraud firm decreases. There are

[53] The incentive for the well-capitalized rival to set lower prices comes from the fact that in a switching cost model, attracting customers today pays off in terms of a higher customer base and thus higher prices in the future (Klemperer, 1987).
[54] The model is adapted from Example 1 in Klemperer (1995). Chevalier and Scharfstein (1996) also analyse the effect of financial constraints on price-cost markups based on the Klemperer (1995) model.

various channels through which these effects could occur. For example, the fraud firm may be forced to delay product improvements or launch new products, conferring an advantage to the rival. This is likely to be associated with higher (lower) reservation utilities (and hence higher (lower) second-period prices) for the rival's (fraud firm's) customers who have already consumed in period one. Similarly, there could be favorable (unfavorable) factor market consequences for the rival (fraud) firm: the fraud firm could lose valuable employees or innovators to its rival, and could be in a weaker bargaining position vis-à-vis input suppliers while the rival benefits from weakened competition in the factor market. In Appendix 4.6, I present an alternative rationale as to why the rival firm's second-period price could increase (and that of the fraud firm decrease), driven solely by the lower utility that consumers expect from its products in the second period. This rationale builds on the idea that if the fraud firm has an initial advantage in second-period advertising (either in terms of cost or impact), then the rival firm has to prices below the reservation utility of its consumers to prevent them from switching. However, as the fraud firm becomes financially impaired, its second-period price falls, while the rival is now able to charge a higher second-period price.

4.3.2.1 Period one competition

As in Klemperer (1995), I assume that firms A and B are located at the two extremities of the unit line. Consumers are uniformly distributed on the line and have a mass of unity. Consumers experience transportation costs to visit the firms. Transportation costs ate *t* per unit distance. As is well recognized, *t* captures the degree of product differentiation.

Consumers buy one unit of the good and derive utility u. Each firm can choose advertisement spending y^A and y^B. I assume that advertisement spending directly enters the utility functions of consumers. Thus, a consumer located at a distance s from form A buys from firm A if and only if

$$u - P^A - ts + y^A \geq u - P^B - t(1-s) + y^B$$

Where P^A and P^B denote the period one prices charged by the two firms. The marginal consumer who is indifferent between buying from either firm is located at s^*, given by

$s^* = \frac{1}{2} + \frac{P^B - P^A}{2t} + \frac{y^A - y^B}{2t}$. s^* is regarded as the market share of firm A (since the latter is located at "0") and denoted by σ_A. The market share of firm B is $\sigma_B = 1 - \sigma_A$.

I assume throughout that marginal cost of production in period one is constant and given by c. I abstract from fixed costs without any loss of generality. Period one profit of Firm A, net of advertisement costs, is then given by

$$\Pi_1^A = (P^A - c)\left(\frac{1}{2} + \frac{P^B - P^A}{2t} + \frac{y^A - y^B}{2t}\right) - \frac{1}{2}\alpha(y^A)^2$$

where $\frac{1}{2}\alpha(y^A)^2$ is the cost of advertising, and $\alpha>0$. Similarly, I can write the period one profit function of firm B.

4.3.2.2 Period-two prices and switching costs

Following Klemperer (1995), I assume that in the second period, consumer switching costs *s* are sufficiently high that the firms can charge consumers their reservation utilities r^A and r^B, respectively. Assume $r^A>c$ and $r^B>c$. For high enough *s*, for each firm, the deviation price that would switch the customers of the rival would have to be so low that it is better off charging its period one customers their reservation utility in the second period.[55] Period-two profit of firm A is σ_A $(r^A\text{-}c)$, and the sum of the first-and-second period profit is thus

$$\Pi^A = \big((P^A - c) + (r^A - c)\big)\left(\frac{1}{2} + \frac{P^B - P^A}{2t} + \frac{y^A - y^B}{2t}\right) - \frac{1}{2}\alpha(y^A)^2$$

and similarly for firm B. The firms simultaneously choose prices and advertising spending in the first period to maximize two-period profits. As is standard, the equilibrium is most conveniently analyzed in terms of each firm's first-order conditions with respect to price and advertising, assuming those for the other firm as given. Denoting $x^A = \frac{P^A - c}{2t}$ and $x^B = \frac{P^B - c}{2t}$, the first-order conditions for firm A with respect to price is

$$\frac{1}{2} + x^B + \frac{y^A - y^B}{2t} = R^A + 2x^A \qquad (4.1)$$

where $R^A = \frac{r^A - c}{2t}$.

The first-order condition with respect to advertising is

[55] See Appendix 4.6 for details.

$$R^A + x^A = \alpha y^A, \tag{4.2}$$

and we have analogous conditions for firm B:

$$\frac{1}{2} + x^A + \frac{y^B - y^A}{2t} = R^B + 2x^B \tag{4.3}$$

and

$$R^B + x^B = \alpha y^B. \tag{4.4}$$

These four equations can be solved to obtain the Nash Equilibrium values of the four choice variables.

Several features are noteworthy. First, from Eqn. (4.1), if $R^A > 0$, x^A is smaller than the value that maximizes period one profit. Moreover, for given x^B, the higher is R^A, the lower is x^A, and the lower is period one profit. This is a consequence of prices being set lower in period one to increase market share and gain in period two, since the market share carries over to the second period due to consumer switching costs. Moreover, from Eqn. (4.2), it is clear that period one prices-cost margin and advertisement move in the same direction. This observation has been made by Hall (2014) in the context of the cyclical behavior of markups (or profit margins), who argues that the claim that markups are countercyclical is at odds with the pro-cyclical behavior of advertising.[56]

Finally, notice that in the absence of advertising ($\alpha = \infty$), period one prices of the two firms will move in the same direction in response to parameter changes. In other words, prices are "strategic complements". However, as we shall see below, in response to one of the firms experiencing an adverse financial shock, it is possible that prices move in opposite directions. Moreover, it is possible that price-cost margins and advertisement to also move in opposite directions.

[56] For given y^B, from Eqns. (4.1)-(4.3), one can solve for y^A, which gives firm A's reaction function in the advertisement space. In the same way, one can solve for firm B's reaction function. It can be shown that ensuring the usual "stability conditions" for the symmetric case of $R^A=R^B$ requires both reaction functions to be downward sloping, and $t > \frac{1}{3\alpha}$. In what follows, we shall keep this assumption.

4.3.2.3 Effect of fraud and financial impairment

Suppose firm B is the fraud firm and experiences an adverse financial shock when it commits fraud. One consequence of this is that the firm may struggle to meet its existing debt payments in the first period as access to finance dries up. To increase the likelihood of survival, I assume the firm assigns relatively more weight to period one profit than to period two profit. One way to represent this is to assume that the objective function of firm B changes to

$$\Pi^B = ((P^B - c) + \mu(r^B - c))\left(\frac{1}{2} + \frac{P^A - P^B}{2t} + \frac{y^B - y^A}{2t}\right) - \frac{1}{2}\alpha(y^B)^2$$

where μ<1. Lower values of μ correspond to more severe financial impairment.

This formulation, however, ignores possible predatory and competitive effects that could affect second-period pricing as well. As argued above, firm B may experience lower profit margins, while firm A experiences higher profit margins, as a consequence of firm B's inability to improve existing products or to introduce new products, and also due to its weakened position in the factor market. Accordingly, I assume that firm B's profit margin has the following functional relationship with the financial impairment parameter: $r^B - c = \frac{r-c}{2} + (1+\mu)\frac{r-c}{2}$. Here, we can think of (r-c) as the reservation utility of consumers less marginal cost when $\mu = 1$. However, the financial impairment lowers firm B's profit margin. Similarly, firm A's profit margin is assumed to be $r^A - c = \frac{r-c}{2} + (3-\mu)\frac{r-c}{2}$, and it increases as μ decreases. Both firms have the same profit margin when $\mu = 1$.

Thus, firm A's objective function is to choose P^A and y^A to maximize

$$\Pi^A = \left((P^A - c) + \left(\frac{r-c}{2} + (3-\mu)\frac{r-c}{2}\right)\right)\left(\frac{1}{2} + \frac{P^B - P^A}{2t} + \frac{y^A - y^B}{2t}\right) - \frac{1}{2}\alpha(y^A)^2 \quad (4.5)$$

while that of firm B is to choose P^B and y^B to maximize

$$\Pi^B = \left((P^B - c) + \left(\frac{r-c}{2} + (1+\mu)\frac{r-c}{2}\right)\right)\left(\frac{1}{2} + \frac{P^A - P^B}{2t} + \frac{y^B - y^A}{2t}\right) - \frac{1}{2}\alpha(y^B)^2. \quad (4.6)$$

Denoting $R = \frac{r-c}{2t}$, the first-order conditions are:

$$\frac{1}{2} + x^B + \frac{y^A - y^B}{2t} = \frac{3-\mu}{2}R + 2x^A \quad (4.7)$$

and

$$\frac{3-\mu}{2}R + x^A = \alpha y^A \tag{4.8}$$

For firm A,

while for firm B, these are:

$$\frac{1}{2} + x^A + \frac{y^B - y^A}{2t} = \frac{1+\mu}{2}R + 2x^B \tag{4.9}$$

and

$$\frac{1+\mu}{2}R + x^B = \alpha y^B. \tag{4.10}$$

Solving, we get

$$x^B = \frac{1}{2} - \frac{(\mu(1-2k)+5-2k)}{2(3-2k)}\ R \tag{4.11}$$

and

$$x^A = 1 - 2R - x^B, \tag{4.12}$$

where $= \frac{1}{2\alpha t}$.

It is easily verified that for $t > \frac{1}{\alpha}$, we have:

$$\frac{dx^B}{d\mu} < 0, \text{ and } \quad \frac{dx^A}{d\mu} > 0\ . \tag{4.13}$$

Further, $$\frac{dy^B}{d\mu} = \frac{R}{\alpha(3-2k)} > 0, \text{ and } \quad \frac{dy^A}{d\mu} = -\frac{R}{\alpha(3-2k)} < 0\ . \tag{4.14}$$

4.3.2.4 Discussion and interpretations

1. Equations (4.13) and (4.14) imply that when product differentiation is sufficiently high (high t), or advertisement expenditure insufficiently effective (high α), the more severe the financial impairment of firm B (lower μ), the lower is the rival firm's (firm A) price, and the higher is firm B's price. At the same time, the higher is firm A's advertisement spending, and the lower is firm B's advertisement spending. All these responses work towards a lower market

share for firm B. The first two panels in Figure 4.1 show how the price-cost markups change for both firms as a function of μ. For the particular parameter values assumed for the figure, firm A's price is below its cost, and the price increases monotonically with μ. The opposite is the case for the price charged by firm B. The next two panels show that Firm A's period-one profit is negative and monotonically increases in μ, implying that it incurs larger losses in the current period when firm B is more financially impaired, consistent with predation. Its overall two-period profit, however, is decreasing in μ, as expected.[57]

2. In contrast to the typical implications from a switching cost model following leverage increase by one of the firms, in this model, if product differentiation is sufficiently high, as μ changes, (i) for both firms, price or profit margin in period one and advertisement spending move in opposite directions, (ii) period-one prices of the two firms move in opposite directions. With high enough product differentiation, the fraud firm does not lose too much market share as it raises the period-one price, and is able to increase period-one profit. The rival firm, on the other hand, lowers the price since the incentive to do so – driven by higher margins in the second period due to the fraud firm's impairment – is higher. Advertisement spending by the rival firm, counterintuitively, increases even though its period one margin is lower. Again, this is because of the higher potential margins in the second period, which generate higher profits if period-one market share is higher.

3. While the actions (pricing and advertising behavior) by the rival in period one can be considered predatory because they lower the fraud firm's market share and involve the sacrifice of period-one profits, one of the main drivers of this behavior is the potential for higher margins in the second period. As discussed above, the higher second-period margins could be the outcome of both the current and (unmodelled) future predatory actions. For example, if the fraud firm is forced to delay the launch of new products, or is preempted by the rival, this could affect the future margin of the rival (fraud firm) favorably (adversely). The margins could also be affected in this manner if the rival firm exploits the weakened financial position of the fraud firm to gain advantages in the factor market, i.e., poach inventors, skilled workers, or switch suppliers. In other words, current period predatory actions are encouraged by the possibility of future predatory actions. Note that a reverse feedback effect is also highly plausible, although

[57] The last two Panels in Figure 4.1 show that firm A's sales increase even though it lowers its price, while that of firm B decrease, as μ becomes smaller. This result is possible here because advertisement spending steps up for firm A and decreases for firm B.

I do not model this here. This could occur if the current period predatory actions that further lower firm A's period one profits limit its ability to compete in the second period in the ways mentioned above.

4. It is easy to check from Eqns. (4.11) - (4.14) that while the effect of a change in μ on the prices P^A and P^B increase in t and α (that is, the price changes are larger in magnitude for higher values of t and α), the effect on advertising is decreasing in α and t. In one of my empirical tests, I distinguish between markets that experience growth of new customers versus stagnant customer bases. I argue that in a switching cost industry with old customers, advertisement is less likely to be important in changing customer tastes, and product differentiation (in the minds of old customers) is likely to be more important. Thus, these industries correspond to high values of t and α, and I expect to see the effects of a change in μ manifest mostly in prices, and not advertising. Exactly the opposite is the case when there is new customer growth.

5. One may wonder whether my empirical results previewed in the Introduction could be due to competition between rival firms to attract a newly dislodged customer pool from the fraud firm, rather than predation. In other words, if there are concerns about product quality, customers may simply leave the fraud firm, and then competition sets in amongst industry rivals to attract these customers. This situation is equivalent to the new arrival of a mass *A* of new customers in the first period of a switching cost model. Assume that rival firms 1 and 2 (distinct from the unmodelled fraud firm) are located at the two extremes of the unit line. The new mass *A* is also uniformly distributed on the unit line. Then the objective function of firm 1 is

$$\Pi^1 = (1 + A)((P^1 - c) + r - c)\left(\frac{1}{2} + \frac{P^2 - P^1}{2t} + \frac{y^1 - y^2}{2t}\right) - \frac{1}{2}\alpha(y^1)^2$$

and similarly for firm 2. The symmetric equilibrium solutions are

$x^1 = x^2 = \frac{1}{2} - \frac{r-c}{2t}$ and $y^1 = y^2 = \frac{1+A}{2\alpha}$. Thus, higher A increases advertisement spending, but does not lower prices. Alternatively, one could assume that the dislodged customers show up in the second period, so they do not directly affect period-one market share. However, when a mass of new customers show up in a later period, prices have to be lower in that period to attract these customers and build market share. Lower second (or later) period price, in turn, makes it less important to cut prices in the current period and build market share. Thus, even this situation would not predict lower prices in the current period.

6. One of my empirical results that is harder to explain in terms of the model is that in industries with significant new customer inflow, the effect of fraud on the rival firms' profit margins is mitigated at higher levels of fraud firm's leverage. A possible explanation is economies of scale in advertising. The rival firm may be prevented from moving its advertisement to platforms where the marginal cost is lower, on account of fixed costs. As we have seen, for lower values of μ, advertisement spending by the rival firm increases. This may enable it to incur the fixed costs if the scale economies associated with lower marginal costs of alternative platforms is sufficiently high. Suppose the parameter α for firm B is normalized to 1. It can be shown that if for firm A, the condition $2t < 1 + \frac{1}{\alpha} < 6t$ holds, then $\frac{dx^A}{d\mu} < 0$. Notice that under my assumptions, when α=1 for both firms, the first of these inequalities cannot hold since I require tα>1. However, the price it charges could increase if the fall in μ prompts a sufficiently large increase in the scale of advertising, and α falls as a result.

4.4. Data and Sample Overview

4.4.1 High-profile fraudulent firms

To identify fraudulent firms and initial public revelation dates, I turn to the SEC website to obtain enforcement releases.[58] I follow Karpoff et al. (2017) to select fraud cases with 13 (b) charges.[59] I collect all fraud-related events available from enforcement releases, SEC filings, and LexisNexis. These events include SEC informal/formal investigation, restatement announcement, and press releases of the firm's internal investigation. Among the interrelated events, I identify the public announcement that reveals a firm's misconduct for the first time.

I focus on financial frauds committed by high-profile firms. I define high-profile firms as the S&P 500 constituents. In total, I identify 47 high-profile fraudulent firms that are incorporated in the U.S. Figure 4.2 summarizes the key events pertaining to the fraud at Office Depot, Inc., the world's second-largest office supplies chain. Office Depot overstated its net earnings for the third quarter of 2006 through the second quarter of 2007. Office Depot

[58] The U.S. SEC website documents enforcement releases from 1995.
[59] These fraud cases include at least one charges of violating Section 13(b)(2)(a), Section 13(b)(2)(b), and Section 13(b)(5) provisions of the 1934 Securities Exchange Act and Rule 17 CFR 240.13b2-1 and Rule 17 CFR 240.13b2-2 of the Code of Federal Regulations. For more details, please see Karpoff et al. (2017).

prematurely recognized approximately $30 million in funds received from vendors in exchange for the company's merchandising and marketing efforts, instead of recognizing the funds over the relevant reporting periods in a manner consistent with Generally Accepted Accounting Principles. Office Depot also violated Regulation FD in 2007 by selectively communicating to analysts that it would not meet analysts' quarterly earnings. Six days after the calls to analysts began, Office Depot filed a Form 8-K announcing that its earnings would be negatively impacted due to continued soft economic conditions, and the company's stock dropped 7.7% in six days. On 29th October 2007, Office Depot announced that it is delaying its third-quarter earnings results due to an independent review of vendor program funds by its audit committee. On the same day, the stock price fell by 16%. I consider the 29th October 2007 as the initial revelation date.

[Insert Figure 4.2 here]

4.4.2 Identification of industry competitors and empirical strategy

I identify peer firms using text-based network industry classifications (i.e., TNIC2 and TNIC3) by Hoberg and Phillips (2010, 2016). The industry classifications are constructed based on product descriptions in firms' 10K filings. The TNIC industry classifications are not transitive and list a distinct set of competitors for each firm that all produce similar products and services, and are updated annually. This allows me to define industry boundaries more accurately compared to standard and transitive industry classifications, such as SIC codes. The TNIC3 classification is as coarse as three-digit SIC codes, while a TNIC2 classification is as coarse as two-digit SIC codes. Since TNIC3 is a subset of TNIC2, this relationship enables me to identify close competitors of the high-profile fraudulent firm (peer firms) and control firms.

Specifically, I identify treated firms as close peers in the same TNIC3 group as the fraud firm during the three years before the revelation of fraud. Control firms are from the TNIC2 group, excluding close peers found from the TNIC3 group. I exclude financial firms and conglomerate industries from my peer and control sample. All the firms are incorporated in the U.S. Firms with total assets or sales less than $1 million are excluded from the sample. If fraudulent firms share the same peer firm, I only assign the peer firm to the fraud firm, which is exposed first. Fraud firms are excluded from the peer and control groups.

My empirical strategy is similar to the "stacked difference-in-difference" approach for multiple events (e.g., Gormley and Matsa, 2011). I employ the propensity score matching approach to control for potentially different observable firm characteristics between treated peer and control firms. The matching technique that I adopt is the one-to-one nearest neighbor matching with replacement (Heckman, Ichimura, and Todd, 1997).[60] I start the matching with a logit regression to predict the probability of becoming a peer firm. The matching is based on firm characteristics at year t-4 (four years before a fraud is first revealed to the public). For each peer firm, I identify a matching firm as the one with the closest propensity score based on a set of firm characteristics: firm size, book-to-market ratio, sales, sales scaled by total asset, past stock returns, and an advertising dummy equal to one for firms with non-zero advertising spending (and zero otherwise). Thus, for each event (revelation of fraud by an S&P 500 constituent firm), I have a set of same-TNIC3 peer firms (treated firms) and a matched set of control firms from the same TNIC2 industry. I estimate:

$$Y_{ict} = \beta * Peer_{ic} * Post_{ict} + \gamma_{ic} + \omega_{ct} + \varepsilon_{ict}$$

where the dependent variable is either the logarithm of one plus advertising expenditure, or the adjusted profit margin. $Peer_{ic}$ takes a value of one if firm i is a TNIC3 peer firm of the fraud firm, and zero if it is a control firm, in cohort c. $Post_{ict}$ is an indicator variable which equal to one for three years post announcement (excluding announcement year) i.e. year t+1, t+2, and t+3, and zero for three years prior to the announcement, i.e. year t-3, t-2, and t-1; γ_{ic} captures firm-cohort fixed effects, ω_{ct} represents cohort-year fixed effects. Standard errors are clustered at the firm level.

4.4.3 Variables

I measure advertising spending in three different ways: the natural logarithm of (1+advertising spending), advertisement scaled by sales (advertising intensity) or book value of assets (scaled advertising). Adjusted profit margin is the sum of earnings before interest and advertising spending scaled by sales.

Since only about 37% of firms in the combined peer and control groups report advertisement expenditure, a key issue for most studies on advertisement is the treatment of missing values. Some studies (e.g., Grullon, Kanatas and Weston (2004), Chemmanur and Yan (2009), Vitorino (2013), Fich Starks and Tran (2018)) replace the missing advertising expenditures with zero. However, if missing observations do not actually represent zero

[60] My results are robust to alternative matching procedures and are available in the online appendix.

advertising, my difference-in-difference estimates could be biased if the fraction of firms with missing observations is different for the peer and control subsamples. My matching procedure includes an indicator variable for missing advertisement spending information and produces a similar, but not identical, proportion of observations with missing advertisement in the treated and control subsamples. However, for some of my subsample tests, the proportions tend to be statistically different (although generally in a direction that would bias against my hypothesis).[61] In view of this concern, my main tests are only for firms with non-missing advertisement expenditures. In Appendix Table 4.2, I compare firm characteristics of firms with available and missing advertising expenditure. There are no significant differences in firm characteristics. This is also true for the peer-firm sample and the control sample separately, as reported in the online appendix. This suggests that there is no potential selection bias from dropping firms with missing advertising. However, all my conclusions remain if I treat missing values as zero. [62] These results are available in the online appendix.

Finally, following Lou (2014), I construct several control variables as of date *t-4*. For regressions in which various measures of advertising are dependent variables, I include *Assets*, *Market-to-book Ratio*, *Sales*, *Age*, the *KZ index*, as well as stock market controls such as past one-year and two-to-five-year stock cumulative returns. *Asset*s is calculated as the natural logarithm of total asset. *Market-to-book Ratio* is defined as the market value of assets divided by the book value of assets. *Sales* is the natural logarithm of total sales. *Age* is estimated as the natural logarithm of the number of years since a firm's establishment. *KZ Index* is constructed following Kaplan and Zingales (1997). For regressions in which the adjusted profit margin is the dependent variable, following Ferreira and Matos (2008), I include *Assets*, *Market-to-book*, *Cash* and *Leverage* as control variables. *Cash* is cash and short-term investments scaled by total assets. *Leverage* is the sum of short-term debt and long-term debt divided by total assets. Appendix 4.1 provides detailed definitions of all variables in my regressions.

[61] For example, if both peer and control firms increase advertisement after the event, and the control sample has a higher proportion of missing observations, assigning the value of zero both before and after the event to missing advertising when in fact it is not zero will bias the estimated difference-in-difference coefficient upwards.

[62] Below, I examine how firms' sales (sales scaled by that of the fraud firm) change after the event. I find that the impact of the event on scaled sales is identical for firms that report advertising and those that do not. Since firms that hold advertisement spending at zero after the fraud firm is impaired would be unlikely to experience similar gains in market share as the one that do increase advertisement, this suggests that the ones with missing advertisement spending information are simply not reporting it separately.

4.5. Results

4.5.1 The effect of fraud revelation on fraudulent firms

Figures 4.3(a) and 4.3(b) present, respectively, the impact of the fraud revelation on the stock prices and market value leverage of the sample fraud firms. The average cumulative abnormal returns on announcement is substantial and averages around 20%. Correspondingly, the market-value leverage of the fraud firms also increases sharply and remains significantly higher than the average levels of the three years prior to the event for at least another three years. Such an increase in leverage sets the stage for predatory activities by the rival firms.

[Insert Figures 4.3(a) and 4.3(b) here]

Figures 4.3(c)-4.3(d) show one consequence of the adverse leverage shock that I explore in detail in the rest of the paper. Figure 4.3(c) shows that the ratio of the advertisement expenditure of the fraud firm to that of the rival firms in the same TNIC3 industry falls dramatically after the shock. Figure 4.3(d) shows that the mean dollar spending of the fraud firms also falls after the fraud, while the mean dollar spending of the peer firms moves in the opposite direction. Unable to match the stepped-up advertisement spending (and, as we shall see below, price cuts) by rival firms, the financially and reputationally impaired fraud firm loses sales and market share. As shown in Figure 4.3(e), the fraud firms experience a sharp decline in market share relative to rival firms in the same TNIC3 industry immediately after fraud revelation. Figure 4.3(f) shows that the mean dollar sales of the fraud firms remain flat after the events after falling sharply from the level of the year before the fraud, while the mean dollar sales of the peer firms continue to increase.

[Insert Figures 4.3(c) – 4.3(f) here]

4.5.2 Univariate comparisons

Panel A of Table 1 presents descriptive statistics. Close peer firms of the fraud firm and matched control firms have insignificantly different characteristics in the three years before the public revelation of fraud (*Announcement*), with the exception of profit margin (peer firms have higher profit margins, possibly because they are producing products that are more similar to the major industry leader) and past one-year stock return (higher for peer firms at 10% level of significance). In particular, they have similar advertisement spending. However, in the three years after the fraud revelation, while both peer and control firms increase advertisement spending, peer firms spend significantly more. Peer firms also experience more rapid asset growth, which is consistent with (greater) market share increase at the expense of the fraud firm.

Panels B, C, D, and E show the univariate difference-in-difference comparisons. Panel B, C, and D show that peer firms increase advertisement spending, advertising intensity, and scaled advertising significantly more than control firms, whereas in panel E, I find that profit margins drop more for peer firms than for control firms. These results are consistent with predation.

[Insert Table 4.1 here]

The different trajectories of profit margins of the peer firms and control firms require further discussion. I discuss this further in Section 4.5.3.3 below.

4.5.3 Difference-in-difference regressions

4.5.3.1 Changes in market share

In Table 4.2, I first examine whether the peer firms which are closer competitors of the affected fraud firm gain in terms of sales compared to the control group. The dependent variable is a firm's sales in a particular year scaled by the fraud firm's sales. The coefficient of Peer*Post is significant and positive, consistent with the idea that the predatory strategies of the peer firms increase their sales at the expense of the fraud firm, compared to the control firms. Of particular interest is the coefficient of Peer*Post*Missing, where "Missing" is an indicator variable for missing value of advertisement throughout in the sample. This coefficient is insignificant. This result implies that the firms with missing advertisement spending experience similar market share gains as those that report advertisement. This would be unlikely if the former firms in fact did not spend resources on advertisement, and thus not assigning zero values for missing advertisement appears appropriate.

4.5.3.2 Predation: Advertisement expenditure and product pricing

In Table 4.3, I present results from my difference-in-difference regressions. In Panel A, I report results with the full set of controls,[63] while in panel B, I report results without any firm-specific controls, to ensure that control variables that are also affected by the treatment do not bias my estimates. The coefficient of Peer*Post is positive when the advertisement spending, advertising intensity, and scaled advertising are the dependent variable, while it is negative when the profit margin is the dependent variable. This is consistent with my hypothesis that predatory advertising by close competitors goes up when the major industry peer is impaired. Predation also seems to take the form of lower prices charged by competitors to attract the fraud firm's customers, since profit margins are lower for the peer firms. These results are consistent with the model presented in Section 4.3. However, these lower prices could be an outcome of more informative advertising as well. I will examine this issue further below.

[Insert Table 4.3 here]

In Figures 4.4(a)-(d), I present my tests of parallel trends following Dasgupta, Li, and Wang (2017) and Brav, Jiang, Ma, and Tian (2018). The *peer* dummy in my regression specification is interacted with year dummies representing *t*-3, *t*-2, *t*-1, *t*, *t*+1, *t*+2, and *t*+3 years relative to the event year t. Year *t*-4 is included and treated as the base year. The figures show that treated (*peer*) and control firms have similar difference in advertisement expenditure and profit margin prior to the event year as year *t*-4. However, the difference starts to diverge after the event year.

[Insert Figures 4.4(a)-(d) here]

Table 4.4 examines how the TNIC3 industry concentration (excluding the fraud firm), the fraud firm's leverage, and the mean or median leverage in the industry affect the incentive to predate. I note here that the model in Section 4.3 does not directly address these "triple-difference" results, and my arguments, though drawn from the literature, are somewhat informal. I posit that greater market concentration increases the incentive to predate, since the gains from predation are shared by a smaller number of rival firms. Also, if predation involves

[63] The set of control variables used in the advertisement and profit margin regressions reported are not identical since I follow existing literature in choosing the determinants for advertisement spending and profit margin (Ferreira and Matos, 2008; Lou, 2014; respectively). However, my results are robust to the inclusion of the union of the two sets as control variables.

coordinated action on the part of the rivals, that is easier with a smaller number of players. Higher fraud firm leverage makes the shock from fraud revelation more severe, and is analogous to a larger drop in the parameter μ in the model. Finally, lower industry leverage (excluding the fraud firm) suggests that it is easier for the rival(s) to sustain first period losses, which is implied by the model (see also Figure 4.1).

In Column (1) of Panels A-C, I find that if the fraud firm's industry is more concentrated, advertisement expenditure increases more for the peer firms. In Columns (2) - (4) of Panel A-C, I examine how the fraud firm's leverage and the mean and the median leverage of the industry affect the incentive to predate. Consistent with the idea that higher fraud firm leverage makes it more financially constrained after the revelation of fraud and the adverse shock to equity price, I find that advertisement spending by rival firms increases (suggesting more aggressive predation) when 1) the fraud firm's leverage is higher, and 2) when the industry leverage is lower (suggesting that more financial slack facilitates predation). I get consistent results when I examine the ratio of the fraud firm's leverage to the industry leverage.

Turning to Panel D of Table 4.4, in Column (1), I find that profit margins of peer firms drop more if the fraud firm's industry is more concentrated. This result is also consistent with the idea that larger firms have a greater incentive to set lower prices and bring down industry profit margins to attract customers of the fraud firm when they can capture more of the gains from predation. However, when I examine the interactions with fraud firm leverage or industry mean/median leverage, I find that these interactions are positive. I later show that the positive interaction effect of fraud firm's leverage on the profit margin is exclusive to industries with high switching costs and new customer growth, and postpone a discussion of this issue until later.

[Insert Table 4.4 here]

4.5.3.3 Placebo tests

Although the peer firms and the matched control firms belong to the same TNIC2 group and produce similar products, it is possible that the two groups of firms differ in terms of the elasticity of advertisement spending with respect to economic fundamentals that my regression specifications do not pick up. [64] This could be the case, for example, if the peer firms, which

[64] For example, Hall (2014) shows that advertising responds positively to exogenous changes in the profit margin, since the marginal profit from additional output sales is higher. However, as seen from Table 4.1, Panel A, since

are the same TNIC3 counterparts as the S&P500 fraud firm, produce products that are more high-end than the control firms in TNIC2 industries that do not necessarily have close product overlaps with industry leaders. If this is the case, and the frauds cluster at certain phases of the economic cycle, it is possible that the different-in-difference estimate is picking up the different response of advertisement spending to the economic activity of the two groups.

The parallel trends in Figures 4.4(a)-4.4(d) discussed earlier suggest against such business cycle type of effects. In an unreported table, I regress an indicator variable denoting fraud on industry profitability, industry sales growth, and firm characteristics. When I do not include year-fixed effects, industry profitability is negatively associated, and industry sales growth is positively associated, with the likelihood of fraud. When year-fixed effects are included, none of the other regressors have any explanatory power. These results suggest that while frauds cluster in certain years, it does not seem that this clustering is related to underlying economic activity.

For clearer evidence, I conduct a placebo analysis. The sample for this placebo test is constructed as follows. For each high-profile fraud firm, I pick another S&P500 constituent firm (or a firm that is close in terms of market capitalization to the fraud firm) from the same TNIC2 group. I identify the TNIC3 peers of this new focal firm, and ensure that no firm in that TNIC3 group has committed fraud in the previous or next three years. I then consider all the peer firms of this new TNIC3 group as the treated firms, and exclude those firms that also belong to the original TNIC3 industry of all fraud firms. I then create a matched control sample of firms that belong to the original TNIC2 industry based on year *t-4* firm characteristics in the same way as my regression sample. Appendix Table 4.3 reports the summary statistics. Notably, this procedure generates a much smaller sample (mainly because the TNIC classifications are not transitive, and many of the TNIC3 peers of the new focal firm have to be excluded because they also belong to TNIC3 industries of fraud firms). When I compare the pseudo-treated group and the matched control group, I see no significant differences in advertisement or margins at *t-4*. In Panels B-D, I notice that even though both groups increase advertisement spending in the post-event period, the difference-in-difference coefficients are not significant. The differences for profit margin are also not significant in Panel E. Finally, in Appendix Table 4.4,

the profit margin for the control group increases more sharply, the trends in profit margins actually bias against finding that peer firms increase advertising more.

I run the difference-in-difference regression on this placebo sample. The coefficient of post*peer is insignificant in all specifications.

These results mitigate the concern that the difference in the trajectory of profit margins of peer firms and control firms in Panel A of Table 4.1 reflect different sensitivities of the products of these two groups to the economic cycle. While peer firms have higher profit margins prior to the fraud revelation, control firms experience a much sharper increase from a lower base relative to the peers of the fraud firm. I find that, to some extent, the pre-event difference between the peer firms and the control firms is driven by some extremely low values of the profit margin for the control group. When (instead of winsorization at 1 percent) I winsorize the profit margin at 2 percent, the difference is smaller: peer firms have an average profit margin of 0.1109, while for control firms this is 0.0831. The post-event values are, respectively, 0.1243 and 0.1180, and the univariate difference-in-difference is significant and negative at the 1 percent level. The coefficient of the *Peer*Post* variable corresponding to Column (4) in Table 4.3 when I winsorize at 2 percent is -0.0364 (instead of -0.0495 in Table 3) and is significant at the 1 percent level (t-value of -4.92).[65] If I drop these observations that are beyond the 2 percent threshold altogether, the coefficient of Peer*Post is -0.0325 (t-value of -4.10).

4.5.3.4 Switching cost and leverage

In Table 4.5, I examine whether switching costs play a role in the predatory strategies of the same industry peers of the fraud firm. As discussed earlier, switching costs or "customer capital"[66] have important implications for firms' competitive strategies. The model presented in Section 4.3, which delivers results consistent with most of my findings is based on switching costs.

I classify industries as "high" or "low" switching cost industries based on the fraud firm's industry median R&D over book value of assets prior to the revelation of fraud. Industries that spend more on R&D produce more unique or specialized products, and customer switching costs are likely to be higher (Opler and Titman, 1994; Bhattacharyya and Nanda, 2000).

[65] The difference in pre-event profit margin between peer and control firms does not exist for firms in high switching cost industries, where I argue the incentives of predation are highest. However, post-event, control firms enjoy much higher increase in margin than peer firms. The subsample statistics for high and low switching cost industries as in Table 4.1 are presented in online appendix.

[66] Customer markets are those in which the customer base is sticky and thus an important determinant of firms' pricing strategy. See Gilchrist et al. (2017) for a model of customer markets and empirical results very similar to Chevalier and Scharfstein (1995).

Appendix 4.5 provides a list of products that are produced by my classification based on product descriptions of my sample fraud firms from item 1 or 1(a) in 10-K reports filed to the U.S. Securities and Exchange Commission. High switching cost industries feature products such as aircrafts, automotive parts, commercial electronics, defense electronics, electrical equipment, electric power generation systems and engines, heavy-duty diesel trucks, medical equipment, personal computers, pharmaceutical products – which have unique product features and typically use patented knowhow, and are highly customized. In addition, the group includes many product categories where network effects are likely to be important and creates high switching costs, such as computing software, computer networking, etc. In contrast, the low switching cost group includes retailers, distributors and service providers, and more products that are unlikely to have strong consumer loyalties, such as agricultural products, apparel, discount retailer, healthcare products distribution, jewelry, marketing services, media and entertainment, office products and services retailer, packaged food, personal services (car rental, holiday, hotel and etc.), pharmaceutical automation, and information services, pharmaceutical distribution, pharmaceutical technologies and services.

Panel A of Table 4.5 presents results where the profit margin and advertising spending are the dependent variables, while Panel C presents results for advertising intensity and scaled advertising. Profit margin falls for both high and low switching cost industries; however, the DID coefficient is two times larger in magnitude for the high switching cost subsample. I find that advertisement spending, advertising intensity, and scaled advertising increase after fraud revelation only in the industries with high switching costs. This is consistent with the idea that 1) in industries with low switching costs, rivals do not need to step up advertisement significantly or lower prices very much in order to induce the fraud firm's customers to switch, and 2) it is only profitable to spend resources to switch customers if they develop loyalties and are not easily switched back or lured away by other competitors.[67]

[Insert Table 4.5 here]

These results for high versus low switching cost industries are difficult to explain if the main reason for the increase in advertising spending and lower prices were to improve the firm's image in a "tainted" industry. If the latter were the reason, there is no reason why firms in high switching cost industries would have a greater incentive to do so. If anything, with loyal

[67] See Zingales (1998, page 910) for related arguments.

customers who would incur switching costs if they moved away from this industry's products, the incentive to promote the products to customers to repair the industry's image would be lower. Moreover, if the purpose were to project a positive image to the media or to suppliers of capital, it is difficult to argue why the incentive would be higher in industries with high switching costs.[68]

In Panels B and D of Table 4.5, I examine interaction effects with the fraud firm's leverage.[69] I find that in the high switching cost industries, fraud firm leverage encourages rivals to increase advertising spending, advertising intensity, and scaled advertising, consistent with the idea that a highly levered fraud firm is constrained from matching the advertising spending of less levered rivals. In contrast, there is no such effect for the low switching cost industries. The effect of high fraud firm leverage on profit margins is positive for high switching cost industries, but there is no such effect for low switching cost industries. However, as seen from Panel F of Table 4.1, the effect of fraud in the post-fraud period on the profit margin of the industry peers remains negative even for the 75th percentile value of fraud firm leverage. Nonetheless, the fact that fraud firm leverage moderates the effect on profit margins appears counter-intuitive, since higher fraud firm leverage is likely to be associated with a more adverse financial shock. One possibility that is discussed in point 6 of Section 4.3.2.4 is that there might be economies of scale from increasing advertisement spending for the peer firms. Higher fraud firm leverage leads to more advertisement spending, but if this increase is sufficiently large (e.g., when fraud firm leverage is higher), the peer firms might be able to absorb the fixed costs of moving some of their advertising to lower (marginal) cost platforms. These lower marginal costs could be associated with even more advertising, boosting demand. To take advantage of the higher demand, prices, and margins could rise.

4.5.3.5 Switching cost, old and new customers, and predatory strategies

Finally, in Table 4.6, I partition the sample based on industries with high versus low recent sales growth and re-examine the results reported in Table 4.5. Industries with high recent sales

[68] It is also hard to argue why the incentive to promote advertising or lower prices should be greater when the fraud firm's leverage is higher (as seen from our results in Panels A-C of Table 4.4).

[69] It is worth pointing out that the mean and the median industry leverage ratios of the high switching cost industries are, respectively, 0.138 and 0.182, whereas those for the low switching cost industries are 0.340 and 0.333. Thus, to the extent that lower industry leverage plays a role in facilitating predation, the lower leverage ratio in high switching cost industries is another reason why predation is more likely in such industries. Because differences in industry leverage within each type of industry are not as important as across these two types of industries, in my triple difference setting, I do not consider the role of industry leverage.

growth are likely to have new customers who have not yet formed strong loyalties towards the products of a particular firm. The fraud is likely to impair the ability of the fraud firm to compete effectively for these new customers, and advertisement spending is likely to be a very effective predatory tactic to attract customers who do not have strong loyalties to the industry leader's products. In contrast, if the industry has slow sales growth, the customers that can be diverted to the rival firms are the existing customers of the fraud firm, and advertisement is less likely to be effective as their tastes/loyalties are already formed. The only way to divert these customers, therefore, is to offer higher consumers' surplus in the form of large price discounts. The model presented in Section 4.3 generates precisely these implications, discussed further in point 4 of section 4.3.2.4.

In Panel A of Table 4.6, I examine how past sales growth affects the profit margin and pricing strategy in high and low switching cost industries. I report results when industry sales growth is measured as of year *t-4*. However, my results are very similar when classify based on the average sales growth for years *t-6* to *t-4*. Two sets of results are reported. The results at the top of Panel A report the difference-in-difference results without leverage interactions, while those at the bottom include leverage interactions. I focus on the top of the panel first.

The results show that there is some tendency for profit margins to drop even when sales growth is high in high switching cost industries. This is consistent with the idea the lowering prices to attract customers today is an investment in future market share when switching costs are high. However, consistent with the model, I find that margins drop more when sales growth is low. Predatory pricing calls for larger price cuts when customer growth is low, since advertising is less effective in switching old customers of the fraud firm. These customers have to be offered consumers' surplus to leave the fraud firm.

In Panels B-D, I examine how advertisement responds to the fraud revelation event when sales growth is high or low, in high or low switching cost industries. Again, focusing on the top of each panel, I see that advertisement increases only in high switching cost industries, and only when sales growth is high. This is consistent with the idea that when the industry demand is growing due to the influx of new customers who have not yet formed strong product preferences, increasing advertisement spending is an effective way of attracting these new customers.

In the set of results reported at the bottom of each panel, I examine the interactions with leverage. In panels B-D, where I examine advertisement spending, I notice that the fraud firm's leverage increases predatory advertising, but only in industries characterized by high switching cost and high influx of new customers, who have not yet developed switching costs and are easier to divert away from the fraud firm's products. In contrast, turning to the lower set of results in Panel A, while we again see no effect on the profit margin in low switching cost industries, we see opposite effects of leverage for the high switching cost industries depending on sales growth. For the latter industries, when sales growth is low, higher fraud firm leverage is associated with lower margins for the peer firms, which is consistent with higher fraud firm leverage corresponding to a more severe financial impact of fraud. However, this effect is reversed when there is more new customer growth, and fraud firm leverage mitigates the decline in profit margin. As discussed above, this is consistent with scale economies in advertising, which are likely to appear when fraud firm leverage is high and in industries with new customer growth. Advertisement by peer firms increase even more under these conditions, and prices rise to capitalize on the boost to demand that this creates.

[Insert Table 4.6 here]

4.6. Conclusions

While it is well recognized that financial weakness or the inability to raise financing from external sources makes a firm vulnerable to predatory tactics by rival firms, direct large-scale empirical evidence is uncommon. Since product pricing data is not widely available, in this paper, I focus on advertising spending and profit margins to study predatory behavior by rival firms when a major industry leader's financial fraud is publicly revealed for the first time. A unique feature of this setting is that unlike most of the theoretical literature on predation that examines the incentives of a financially well-capitalized *large* firm to prey on a financially weaker *small* rival firm, I focus on the predatory activities of smaller rival firms when a major industry leader is financially impaired. I choose this setting, in part, because predation is easier to identify when the gains from predation are potentially large, as is the case when an industry leader becomes vulnerable.

I show that when a major industry leader's financial fraud is revealed, competitors increase advertising. Indirect evidence from profit margins suggests they also lower prices. The fraudulent firm's leverage (leverage relative to rivals) and industry concentration exacerbates predation. Predation is stronger in industries where customers have higher switching costs. While stepping up advertising is the more potent strategy to attract customers away from the impaired fraud firm when there is significant new customer growth in the market, lowering prices appears to be the favored strategy when the customer base is stagnant.

Appendix 4.1 Variable definition

Variable	Definition
Adjusted Profit Margin	The sum of earnings before interest and advertising, scaled by sales.
Advertising spending	The natural logarithm of (1+advertising spending).
Advertising intensity	Advertising spending scaled by a firm's total sales.
Age	The natural logarithm of the number of years since a firm's establishment.
Assets	The natural logarithm of a firm's total asset.
Cash	Cash and short-term investments divided by total assets.
Fraudulent Firm Leverage	The leverage of a fraud firm at year *t*-4.
High-profile firm	S&P500 constituent firms when their frauds are revealed.
Industry Average Leverage	Fraudulent firm's TNIC3 industry average leverage at year *t-4*.
Industry Median Leverage	Fraudulent firm's TNIC3 industry median leverage at year *t-4*.
KZ index	-1.002*cash flow over lagged assets-39.368*cash dividends over lagged assets-1.315*cash balances over lagged assets+3.139*leverage ratio+0.283*market to book ratio.
Leverage	The sum of short-term debt and long-term debt scaled by total assets.
Market-to-book Ratio	The market value of assets scaled by the book value of assets
R&D	A firm's research and development spending scaled by its total assets at year *t-4*.
Relative Leverage Ratio	The fraudulent firm's leverage divided by the industry median leverage at year *t-4*.
$\mathrm{Ret}_{(t-1)}$	A firm's past one year cumulative returns.
$\mathrm{Ret}_{(t-2,\ t-5)}$	A firm's past two to five year cumulative returns.
Sales	A firm's sales scaled by total assets.
Sales Growth	The growth rate in sales at year *t*-4 or average growth rate in sales between year *t*-6 and year *t*-4.
Scaled advertising	Advertising spending scaled by a firm's total sales.
Top 4 Market	The percentage of sales within a TNIC3 industry attributable to the four largest firms within the high-profile fraudulent firms' industry
$\mathrm{Volatility}_{(t-1)}$	Volatility of a firm's past one year stock returns.
$\mathrm{Volatility}_{(t-1,\ t-2)}$	Volatility of a firm's past two to five year stock returns.

Appendix 4.2 Descriptive statistics comparisons: firms that report and do not report advertising expenditure

This table summarizes firm characteristics between firms that report advertising spending and firms that do not report advertising spending in my nearest one matched sample. The comparisons are shown for the three years before the revelation of high-profile fraud. ***, **, and * implies significance at the 1%, 5% and 10% levels, respectively.

	Reporting Firms	Non-reporting Firms	Difference (*t*-statistics)
Adjusted Profit Margin	0.0899	0.1003	-0.0104 (-1.349)
Assets	6.0503	6.0007	0.0496 (0.929)
Mtb	2.0800	2.0258	0.0542 (1.283)
Sales	5.9190	5.8908	0.0282 (0.522)
Age	2.7196	2.7407	-0.0211 (-0.996)
$\mathrm{Ret}_{(t-1)}$	0.1924	0.1821	0.0103 (0.540)
KZ Index	-2.3484	-2.0470	-0.3014 (-1.478)
Obs	2,161	4,096	

Appendix 4.3 Placebo tests

In this table, I create the placebo peer group by selecting a new TNIC3 industry in which there is another non-fraudulent S&P500 firm or a firm with similar market capitalization with the high-profile fraudulent firm found in fraudulent firm's TNIC2 group (excluding TNIC3 peers). There is no major firm in the new TNIC3 industry committed a fraud. The placebo sample includes peer firms of the new TNIC3 industry and matched firms from the TNIC2 industry of high-profile fraudulent customers. Nearest one propensity score matching at year t-4 (i.e. 4 years prior to announcement). For each peer firm, a matching firm (with replacement) is identified as the one with the closest propensity score based on a set of firm characteristics: firm size, book-to-market ratio, sales, sales scaled by total assets, advertising dummy and past stock returns. Panel A reports descriptive statistics for peer and control groups. In Panel B, C, D and E, data are collapsed into single data points (based on averages) both before and after announcement. This results in two data points per firm. Advertising spending is the natural logarithm of (1 + advertising spending). Advertising intensity is advertising spending scaled by total sales. Scaled advertising is advertising spending scaled by total assets. Adjusted profit margin is (earnings before interest + advertising)/sales. Standard errors are clustered by the number of firms and reported in parentheses. ***, **, and * implies significance at the 1%, 5% and 10% levels, respectively.

Panel A: Descriptive Statistics

Variables	Before Announcement			After Announcement		
	Peer	Control	Difference (*t*-statistics)	Peer	Control	Difference (*t*-statistics)
Advertising Spending	0.5306	0.5489	-0.0183 (-0.258)	0.7948	0.8422	-0.0474 (-0.486)
Advertising Intensity	0.0083	0.0086	-0.0003 (-0.238)	0.0100	0.0107	-0.0007 (-0.446)
Scaled Advertising	0.0099	0.0105	-0.0006 (-0.362)	0.0132	0.0140	-0.0008 (-0.327)
Adjusted Profit Margin	0.1301	0.1178	0.0123 (1.354)	0.1276	0.1270	0.0006 (0.059)
Assets	5.4670	5.4889	-0.0219 (-0.022)	5.8341	5.9743	-0.1402 (1.159)
Market-to-Book	1.6264	1.6481	-0.0217 (-0.389)	1.6576	1.4416	0.2160*** (3.192)
Sales	5.5687	5.5512	0.0175 (0.177)	5.8979	6.0461	-0.1482 (-1.241)
Age	2.8687	2.8743	-0.0056 (-0.134)	3.1339	3.1579	-0.0240 (-0.634)
$Ret_{(t-1)}$	0.1511	0.1527	-0.0016 (-0.058)	0.1328	0.1056	0.0272 (0.757)
KZ Index	-1.6001	-1.6261	0.0260 (0.086)	-1.1729	-1.3930	0.2201 (0.728)
Number of observations	617	650		401	468	

Panel B: Advertising Spending				
	Before	After	Difference	No. of Observations
Peer	0.5186 (0.081)	0.7802 (0.091)	0.2616*** (0.071)	389
Control	0.5367 (0.083)	0.8207 (0.124)	0.2840*** (0.076)	379
Difference			-0.0224 (0.105)	

Panel C: Advertising Intensity				
	Before	After	Difference	No. of Observations
Peer	0.0083 (0.0013)	0.0101 (0.0013)	0.0018* (0.0010)	389
Control	0.0087 (0.0015)	0.0107 (0.0020)	0.0020* (0.0011)	379
Difference			-0.0002 (0.0014)	

Panel D: Scaled Advertising				
	Before	After	Difference	No. of Observations
Peer	0.0097 (0.0022)	0.0130 (0.0022)	0.0033** (0.0013)	389
Control	0.0104 (0.0021)	0.0136 (0.0032)	0.0032** (0.0014)	379
Difference			0.0001 (0.0019)	

Panel E: Adjusted Profit Margin				
	Before	After	Difference	No. of Observations
Peer	0.1247 (0.010)	0.1235 (0.012)	-0.0012 (0.010)	389
Control	0.1138 (0.010)	0.1247 (0.008)	0.0109 (0.010)	379
Difference			-0.0121 (0.015)	

Appendix 4.4 Placebo tests continued

This table reports difference-in-difference estimation results in the matched placebo sample. I report results for advertising spending (natural logarithm of 1 + advertising spending), advertising intensity (advertising spending scaled by total sales), scaled advertising (advertising spending scaled by assets) and adjusted profit margin ((earnings before interest + advertising)/sales). In column (1) to (3), firms are removed if no advertising spending are reported. Assets is the natural logarithm of total assets. Market to book is the ratio of the market value of assets to the book value of assets. Cash is cash and short-term investments divided by total assets. Leverage is the sum of short-term debt and long-term debt divided by total assets. I also include sales (natural logarithm of net sales), age, KZ index (Kaplan and Zingales (1997)), past stock returns and return volatility. Standard errors are clustered by the number of firms. ***, **, and * implies significance at the 1%, 5% and 10% levels, respectively.

Variables	Advertising Spending (1)	Advertising Intensity (2)	Scaled Advertising (3)	Adjusted Profit Margin (4)
Peer*Post	-0.0313	-0.0009	-0.0005	-0.0101
	(-0.10)	(-0.17)	(-0.07)	(-0.68)
Assets	0.3827	0.0067	-0.0307***	0.0498***
	(1.43)	(1.16)	(-2.94)	(2.86)
Mtb	0.0425	0.0089***	0.0099***	0.0427***
	(0.46)	(2.72)	(3.54)	(3.38)
Cash				-0.0805
				(-1.22)
Leverage				-0.0640***
				(-2.20)
Sales	1.0883***		0.0445***	
	(3.54)		(4.33)	
KZ Index	-0.0239	0.0003	-0.0008	
	(-1.21)	(0.83)	(-1.63)	
Age	-0.9327	-0.0359*	-0.0487*	
	(-1.26)	(-1.70)	(-1.81)	
$\text{Ret}_{(t-1)}$	-0.1179	-0.0020	-0.0029	
	(-1.36)	(-0.95)	(-1.04)	
$\text{Ret}_{(t-2,\ t-5)}$	-0.0305	0.0009	0.0011	
	(-0.68)	(0.94)	(0.82)	
$\text{Volatility}_{(t-1)}$	-0.0364	-0.0157	-0.0234	
	(-0.04)	(-0.67)	(-0.82)	
$\text{Volatility}_{(t-2,\ t-5)}$	0.4146	-0.0653	-0.0186	
	(0.33)	(-1.63)	(-0.39)	
Firm*Cohort FE	Yes	Yes	Yes	Yes
Year*Cohort FE	Yes	Yes	Yes	Yes
Adj. R^2	0.808	0.770	0.837	0.661
No obs.	632	632	632	2,115

Appendix 4.5 Product description in low switching cost vs. high switching cost industries

This table describes (in alphabetical order) the products of high-profile fraudulent firms from low switching cost and high switching cost industries respectively. Product description are obtained from item 1 or 1(a) in 10-K reports filed to the U.S. Securities and Exchange Commission.

Low Switching Cost Industry
agricultural products, apparel, broadband internet communications, business process solutions, cars and trucks, cosmetics, discount retailer, electric generation, fashion accessories, fragrance, healthcare products distribution, healthcare services, homebuilder, information technology solutions, jewellery, marketing services, media and entertainment, medical-surgical products (surgical drapes, gowns and apparel and etc.), noncarbonated beverage concentrates, office products and services retailer, packaged food, personal services (car rental, holiday, hotel and etc.), pharmaceutical automation and information services (bedside clinical and patient entertainment systems), pharmaceutical distribution, pharmaceutical technologies and services (aseptic blow-fil-seal technology), refrigerated foods, retail drug store, soft drink, syrups, toiletries.
High Switching Cost Industry
aircraft, automated office equipment distribution, automotive parts, commercial electronics, communication and information processing software, computing software, computer networking, defence electronics, document solutions, electrical equipment, electric power generation systems and engines, graphics and media communication processors, hardware and software product interfaces, heavy duty diesel trucks, imaging services, information technology services, media distribution, media and entertainment, medical equipment, network consulting and design, networking solutions, personal computers, pharmaceutical products, search engine, semiconductor, software solutions, storage software, wireless networking and information system.

Appendix 4.6

In this Appendix, I discuss the sufficient conditions for an equilibrium in which each firm charges a price equal to the second period reservation utility of its (first-period) consumers, and discuss how these prices could change if μ decreases. Note that because the consumers do not get any consumers' surplus from either firm in the second period, second period pricing does not affect their first period choice of which firm to consume from.

Second-period game: Let u_2^A and u_2^B denote the utility per unit of product for consumers of firm A and B, respectively, in the second period. The prices charged are denoted by r^A and r^B, respectively. Let *s* denote consumer switching costs.

If switching cost s is sufficiently high, firms will charge $r^A = u_2^A$ and $r^B = u_2^B$. Then, r^A increases and r^B decreases as μ decreases if consumers' perception of firm B's product worsens, and that of firm A's product improves, resulting in lower u_2^B and higher u_2^A. These changes in u_2^B and u_2^A could also result from (unmodelled) competitive effects in the second period – for example, firm A might accelerate the introduction of product improvements at a time when consumers of firm B have concerns about the quality of the latter firm's products.

The conditions needed for firms to charge $r^A = u_2^A$ and $r^B = u_2^B$ are as follows:

The Indifference Condition for firm A's consumers is given by:

$$u_2^A - r^A = u_2^B - P_2^B - s \ ,$$

where P_2^B denotes a deviation price charged by firm B,

The condition required to prevent firm B from cutting price is:

$$\sigma_B(u_2^B - c) \geq P_2^B - c = u_2^B - u_2^A + r^A - s - c \ ,$$

Similarly, the required condition preventing firm A from cutting its price is:

$$\sigma_A(u_2^A - c) \geq P_2^A - c = u_2^A - u_2^B + r^B - s - c \ .$$

For $r^A = u_2^A$ and $r^B = u_2^B$ to be equilibrium second-period prices, we need

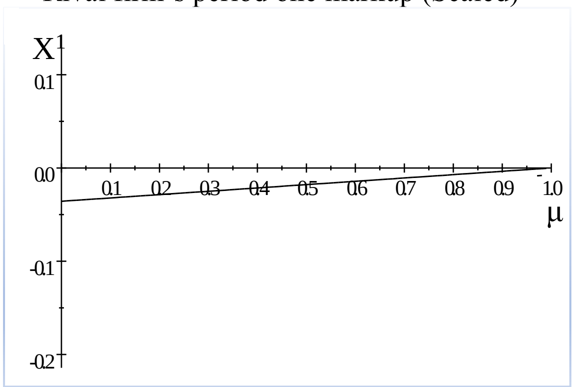
$$\sigma_B(u_2^B - c) \geq u_2^B - s - c \Leftrightarrow s > \sigma_A(u_2^B - c)$$

and

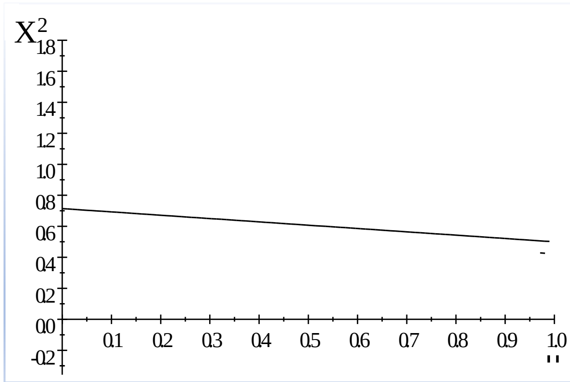
$$\sigma_A(u_2^A - c) \geq u_2^A - s - c \Leftrightarrow s > \sigma_B(u_2^A - c) \ .$$

Figure 4.1 Model implied prices and profits as functions of μ
Model Parameters: t=1.5; α=1, c=1, R=0.5

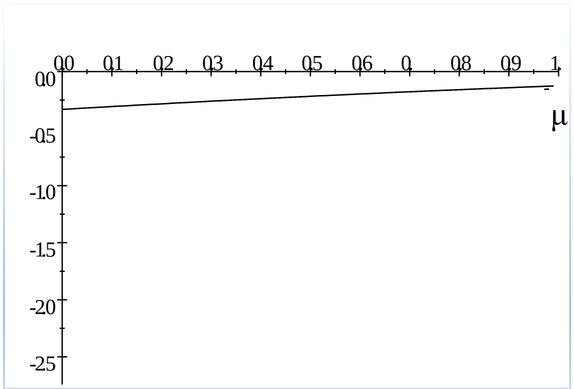
Rival firm's period one markup (Scaled)



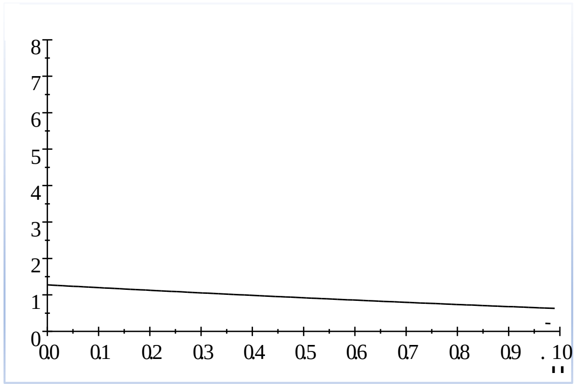
Fraud firm's period one markup (Scaled)



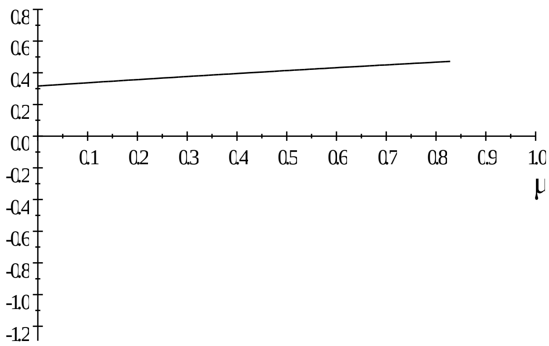
Period one profit of rival firm



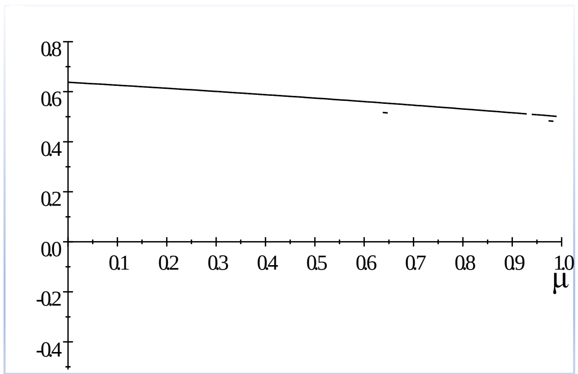
Combined two-period profit of rival firm



Period one sales of fraud firm



Period one sales of rival firm



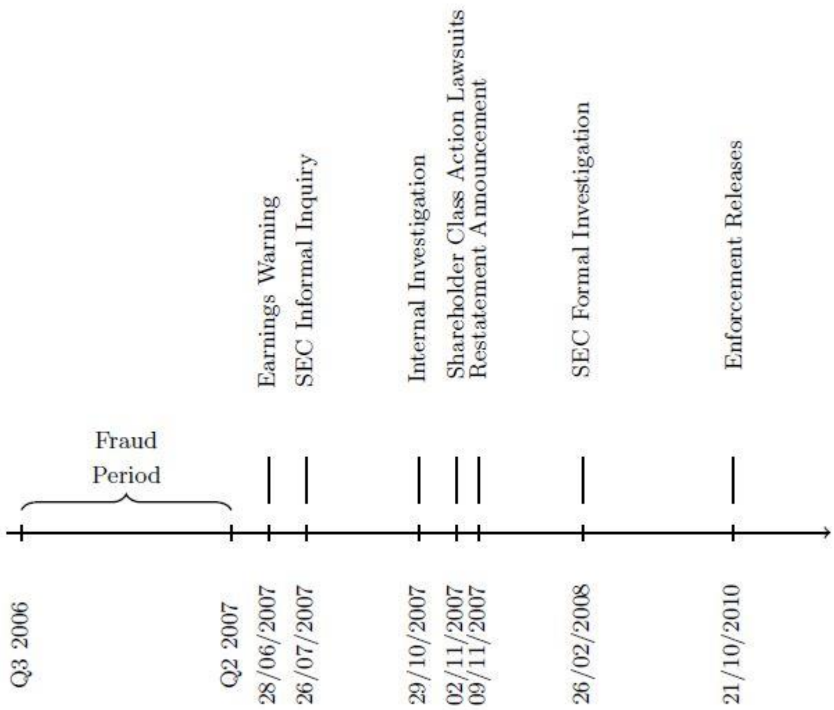


Figure 4.2 Timeline of the key fraud related events: Office Depot, Inc.

Figure 4.3 CAR and trends in sales, advertising spending, and market share

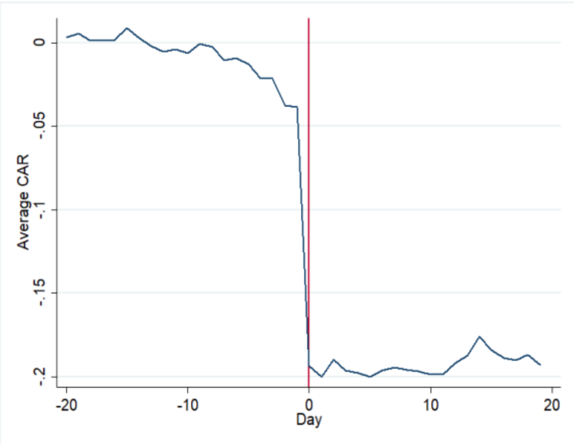


Figure 4.3(a): Average CAR of fraudulent firms 20 days around announcement

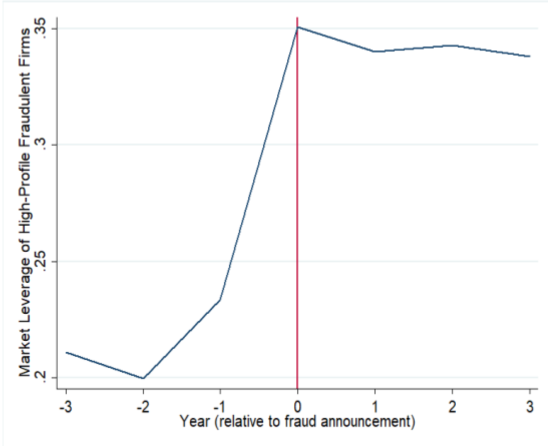


Figure 4.3(b) Leverage of fraudulent firms

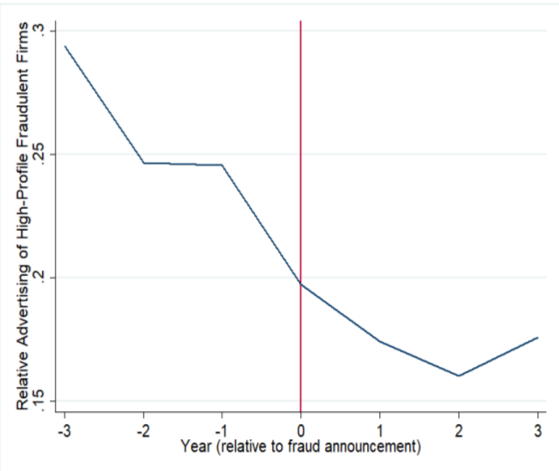


Figure 4.3(c): Relative advertising spending of high-profile fraudulent firms

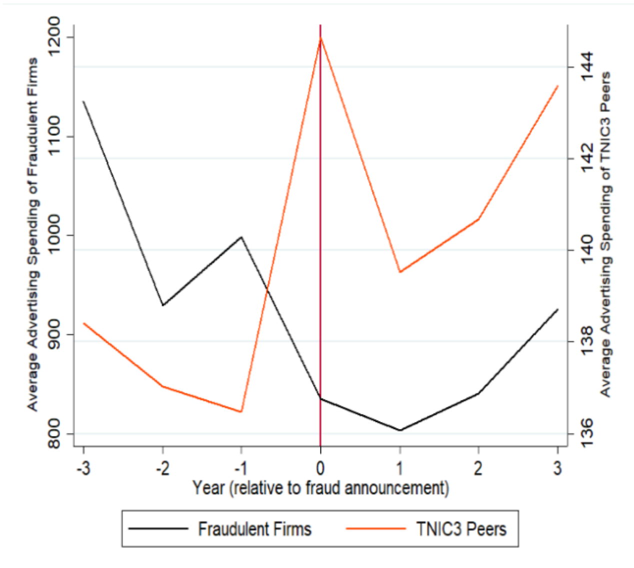


Figure 4.3(d): Average advertising spending of fraudulent firms and TNIC3 peers

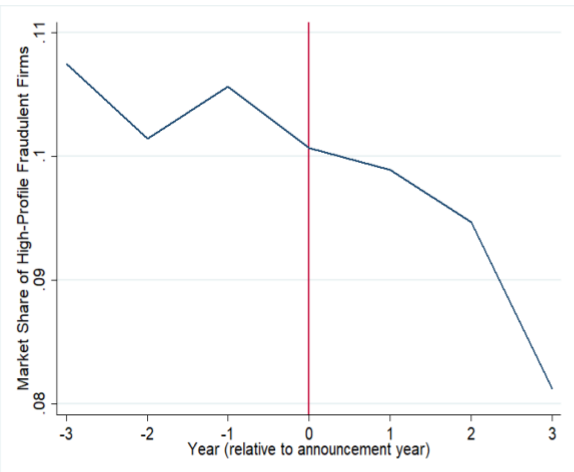


Figure 4.3(e): Market share of high-profile fraudulent firms

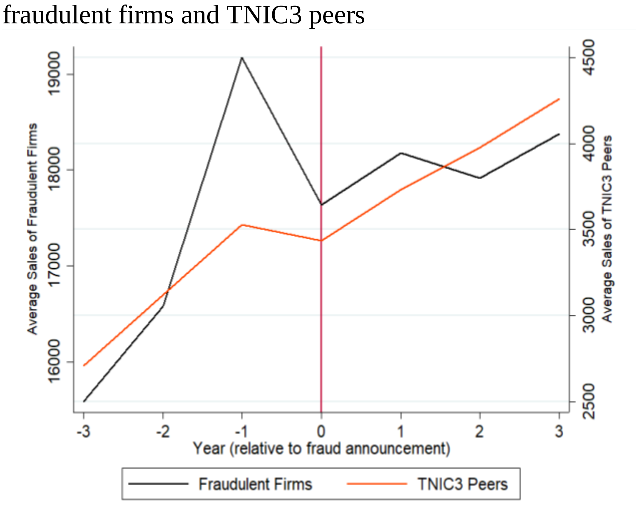


Figure 4.3(f): Average sales of fraudulent firms and TNIC3 peers

Figure 4.4 Dynamic effects

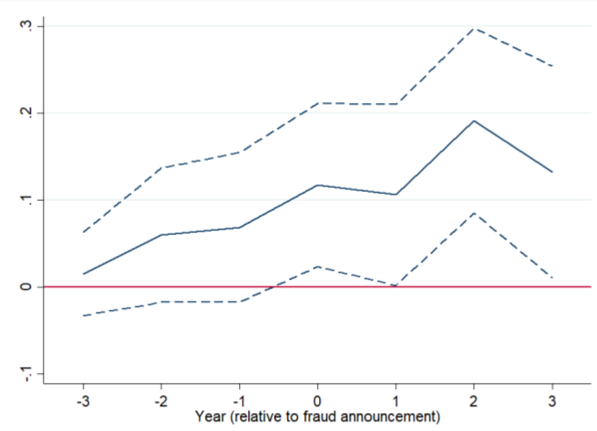


Figure 4.4(a): Advertising spending

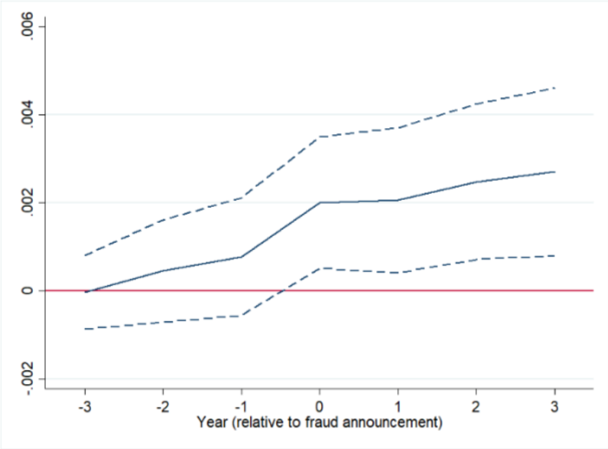


Figure 4.4(b): Advertising intensity

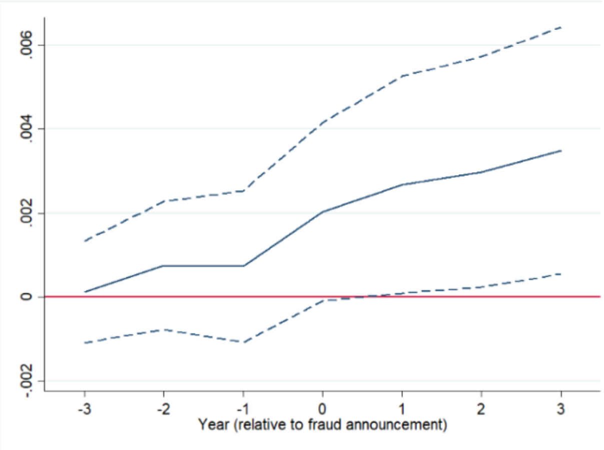


Figure 4.4(c): Scaled advertising

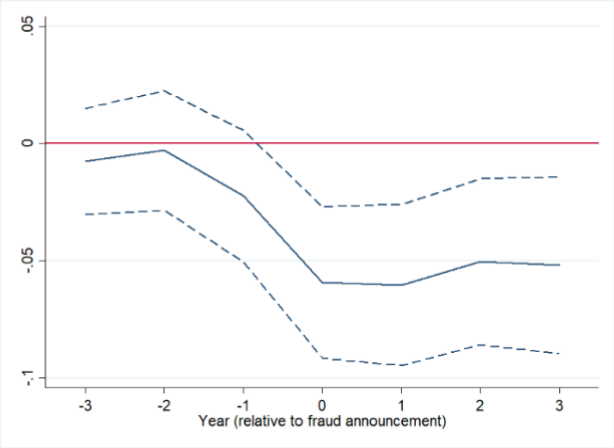


Figure 4.4(d): Adjusted profit margin

Table 4.1 Descriptive statistics

Announcement is industry leader's (S&P500) public revelation of its frauds. Industries are classified using Hoberg-Phillips' product similarities. Peer firms are close peer firms (TNIC3) in the 3 years prior to announcement. Control firms are distant peers (TNIC2) excluding close peers in the 3 years prior to announcement. Nearest one propensity score matching at year *t-4* (i.e. 4 years prior to announcement). For each peer firm, a matching firm (with replacement) is identified as the one with the closest propensity score based on a set of firm characteristics: firm size, book-to-market ratio, sales, sales scaled by total asset, advertising dummy and past stock returns. Panel A reports descriptive statistics for peer and control groups. In Panel B, C, D and E, data are collapsed into single data points (based on averages) both before and after announcement. This results in two data points per firm. Advertising spending is the natural logarithm of (1 + advertising spending). Advertising intensity is advertising spending scaled by total sales. Scaled advertising is advertising spending scaled by total assets. Adjusted profit margin is (earnings before interest + advertising)/sales. Standard errors are clustered by the number of firms and reported in parentheses. ***, **, and * implies significance at the 1%, 5% and 10% levels, respectively.

Panel A: Descriptive Statistics

Variables	Before Announcement			After Announcement		
	Peer	Control	Difference (*t*-statistics)	Peer	Control	Difference (*t*-statistics)
Advertising Spending	0.6643	0.6902	-0.0259 (-0.649)	0.9215	0.8051	0.1164** (2.418)
Advertising Intensity	0.0074	0.0083	-0.0009 (-1.502)	0.0083	0.0073	0.0010* (1.889)
Scaled Advertising	0.0089	0.0094	-0.0005 (-0.787)	0.0099	0.0083	0.0016** (2.413)
Adjusted Profit Margin	0.1109	0.0831	0.0278*** (3.824)	0.1243	0.1181	0.0062 (0.931)
Assets	6.0574	5.9798	0.0776 (1.526)	6.5354	6.3857	0.1497*** (2.642)
Market-to-Book	2.0636	2.0262	0.0374 (0.925)	1.7542	2.0382	-0.2840*** (-7.032)
Sales	5.9413	5.8614	0.0799 (1.545)	6.3841	6.2947	0.0894 (1.594)
Age	2.7205	2.7459	-0.0254 (-1.263)	3.0485	3.0518	-0.0033 (-0.188)
$\mathrm{Ret}_{(t-1)}$	0.2035	0.1685	0.0350* (1.941)	0.1276	0.2059	-0.0783*** (-4.299)
KZ Index	-2.1111	-2.1896	0.0785 (0.404)	-3.0757	-2.0854	-0.9903*** (-4.845)
Number of observations	3,068	3,189		2,443	2,692	

Panel B: Advertising Spending				
	Before	After	Difference	No. of Observations
Peer	0.6402 (0.044)	0.8930 (0.055)	0.2528*** (0.031)	1,987
Control	0.6933 (0.050)	0.8224 (0.056)	0.1291*** (0.027)	2,083
Difference			0.1237*** (0.041)	

Panel C: Advertising Intensity				
	Before	After	Difference	No. of Observations
Peer	0.0072 (0.0006)	0.0083 (0.0006)	0.0011** (0.0005)	1,987
Control	0.0084 (0.0008)	0.0074 (0.0006)	-0.0010** (0.0005)	2,083
Difference			0.0021*** (0.0007)	

Panel D: Scaled Advertising				
	Before	After	Difference	No. of Observations
Peer	0.0086 (0.0008)	0.0098 (0.0008)	0.0012** (0.0006)	1,987
Control	0.0095 (0.0008)	0.0082 (0.0007)	-0.0013*** (0.0005)	2,083
Difference			0.0025*** (0.0008)	

Panel E: Adjusted Profit Margin				
	Before	After	Difference	No. of Observations
Peer	0.1044 (0.008)	0.1114 (0.008)	0.0070 (0.008)	1,987
Control	0.0763 (0.009)	0.1132 (0.007)	0.0369*** (0.006)	2,083
Difference			-0.0299*** (0.010)	

Panel F: Fraud Firm Leverage				
	Mean	Median	25^{th} percentile	75^{th} percentile
Full Sample	0.3331	0.2990	0.1870	0.4466
Low switching Cost	0.4016	0.3621	0.2892	0.6002
High switching Cost	0.2795	0.2079	0.1772	0.4387

Table 4.2 Relative sales

The dependent variable is peer firms' sales relative to the sales of their high-profile fraudulent firms. Missing is one if firms never report advertising spending in the sample. In column (1), controls are excluded. Advertising spending is natural logarithm of (1 + advertising spending). Assets is the natural logarithm of total assets. Market to book is the ratio of the market value of assets to the book value of assets. I also include age, KZ index (Kaplan and Zingales (1997)), past stock returns and return volatility. Standard errors are clustered by the number of firms. ***, **, and * implies significance at the 1%, 5% and 10% levels, respectively.

Variables	Relative Sales (1)	Relative Sales (2)
Peer*Post	0.1284***	0.1308***
	(4.42)	(4.51)
Peer*Post*Missing	0.0072	0.0168
	(0.43)	(0.90)
Assets		0.0615***
		(3.48)
Mtb		0.0039
		(0.72)
Advertising Spending		-0.0022
		(-0.29)
KZ Index		-0.0002
		(-0.32)
Age		0.3152***
		(3.54)
$\mathrm{Ret}_{(t-1)}$		0.0108
		(1.49)
$\mathrm{Ret}_{(t-2,\ t-5)}$		-0.0006
		(-0.23)
$\mathrm{Volatility}_{(t-1)}$		-0.3638***
		(-5.11)
$\mathrm{Volatility}_{(t-2,\ t-5)}$		-0.4273**
		(-2.57)
Firm*Cohort FE	Yes	Yes
Year*Cohort FE	Yes	Yes
Adj. R^2	0.782	0.786
No obs.	9,814	9,814

Table 4.3 Advertising and profit margin

This table reports difference-in-difference estimation results in the matched sample. I report results for advertising spending (natural logarithm of 1 + advertising spending), advertising intensity (advertising spending scaled by total sales), scaled advertising (advertising spending scaled by assets) and adjusted profit margin ((earnings before interest + advertising)/sales). In column (1) to (3), firms are removed if no advertising spending are reported. Assets is the natural logarithm of total assets. In column (4), adjusted profit margin is winsorized at 1%. In column (5), adjusted profit margin is winsorized at 2%. Market to book is the ratio of the market value of assets to the book value of assets. Cash is cash and short-term investments divided by total assets. Leverage is the sum of short-term debt and long-term debt divided by total assets. I also include sales (natural logarithm of net sales), age, KZ index (Kaplan and Zingales (1997)), past stock returns and return volatility. Standard errors are clustered by the number of firms. ***, **, and * implies significance at the 1%, 5% and 10% levels, respectively.

Variables	Advertising Spending (1)	Advertising Intensity (2)	Scaled Advertising (3)	Adjusted Profit Margin (4)	Adjusted Profit Margin (5)
Peer*Post	0.1112***	0.0032**	0.0043***	-0.0495***	-0.0364***
	(2.76)	(2.32)	(3.04)	(-4.49)	(-4.92)
Assets	0.2781***	0.0040**	-0.0061**	0.0627***	0.0598***
	(4.30)	(2.19)	(-2.60)	(4.66)	(6.78)
Mtb	0.0121	0.0001	0.0006	0.0145***	0.0152***
	(0.98)	(0.17)	(0.86)	(2.91)	(4.70)
Cash				-0.1663***	-0.1226***
				(-3.82)	(-3.80)
Leverage				-0.0973***	-0.0841***
				(-2.68)	(-3.39)
Sales	0.3609***		0.0070*		
	(6.49)		(2.07)		
KZ Index	0.0020	0.0002*	0.0003*		
	(0.85)	(1.86)	(1.76)		
Age	-0.2543*	-0.0045	0.0021		
	(-1.81)	(-0.85)	(0.34)		
$\text{Ret}_{(t-1)}$	0.0011	0.0000	0.0003		
	(0.07)	(0.00)	(0.32)		
$\text{Ret}_{(t-2,\ t-5)}$	0.0223***	0.0002	0.0002		
	(2.65)	(0.62)	(0.76)		
$\text{Volatility}_{(t-1)}$	0.2860*	0.0220**	0.0145		
	(1.78)	(2.15)	(1.58)		
$\text{Volatility}_{(t-2,\ t-5)}$	-0.5381	-0.0077	-0.0007		
	(-1.64)	(-0.74)	(-0.05)		
Firm*Cohort FE	Yes	Yes	Yes	Yes	Yes
Year*Cohort FE	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.857	0.750	0.784	0.772	0.792
No obs.	3,996	3,996	3,996	11,354	11,354

Table 4.4 DIDID tests

This table reports DIDID estimation results in the matched sample. The dependent variables include advertising spending (natural logarithm of 1 + advertising spending), advertising intensity (advertising spending scaled by total sales), scaled advertising (advertising spending scaled by assets) and adjusted profit margin ((earnings before interest + advertising)/sales). The main independent variables include peer, post, peer*post, Z, peer* Z, post* Z, peer*post*Z. Z are proxies for the characteristics of interest: top 4 concentration, fraudulent firm's leverage, industry average leverage, industry median leverage, and relative leverage ratio (the ratio of fraudulent firm's leverage to the industry median leverage). They are measured at year *t*-4. Top 4 market concentration is the proportion of sales within a TNIC3 industry attributable to the four largest firms within the high-profile fraudulent firms' industry. Industry average (median) leverage is fraudulent firm's TNIC3 industry's average (median) leverage. Fraudulent firm's leverage is the leverage of a fraud firm. Relative leverage ratio is the fraudulent firm's leverage divided by the industry median leverage. In Panel A to C, firms with missing advertising spending are dropped. Standard errors are two-way clustered at firm and year, respectively. ***, **, and * implies significance at the 1% level, 5% level, and 10% level, respectively

Panel A: Advertising Spending

Variables	(1)	(2)	(3)	(4)	(5)
Peer*Post	-0.2211	-0.2494	0.4463**	0.2790**	0.1850**
	(-1.39)	(-1.21)	(2.65)	(2.70)	(2.12)
Peer*Post*Top 4 Market	0.5987**				
	(2.19)				
Peer*Post*Fraudulent Firm Leverage		1.4770***			
		(3.00)			
Peer*Post*Industry Average Leverage			-1.2390*		
			(-2.04)		
Peer*Post*Industry Median Leverage				-0.8564*	
				(-1.96)	
Peer*Post*Relative Leverage Ratio					0.0069**
					(2.34)
Controls	Yes	Yes	Yes	Yes	Yes
Firm*Cohort FE	Yes	Yes	Yes	Yes	Yes
Year*Cohort FE	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.857	0.857	0.857	0.857	0.857
No obs.	3,996	3,996	3,996	3,996	3,996

Panel B: Advertising Intensity

Variables	(1)	(2)	(3)	(4)	(5)
Peer*Post	-0.0050	0.0063***	0.0139***	0.0097***	0.0034*
	(-1.22)	(3.07)	(3.44)	(3.42)	(1.88)
Peer*Post*Top 4 Market	0.0248**				
	(2.64)				
Peer*Post*Fraudulent Firm Leverage		0.0103**			
		(2.09)			
Peer*Post*Industry Average Leverage			-0.0356**		
			(-2.33)		
Peer*Post*Industry Median Leverage				-0.0285**	
				(-2.36)	
Peer*Post*Relative Leverage Ratio					0.0001**
					(2.22)
Controls	Yes	Yes	Yes	Yes	Yes
Firm*Cohort FE	Yes	Yes	Yes	Yes	Yes
Year*Cohort FE	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.750	0.750	0.750	0.750	0.750
No obs.	3,996	3,996	3,996	3,996	3,996

Panel C: Scaled Advertising					
Variables	(1)	(2)	(3)	(4)	(5)
Peer*Post	-0.0087	0.0023	0.0104**	0.0057***	0.0036**
	(-1.69)	(0.96)	(2.52)	(3.18)	(2.33)
Peer*Post*Top 4 Market	0.0256**				
	(2.58)				
Peer*Post*Fraudulent Firm Leverage		0.0137**			
		(2.49)			
Peer*Post*Industry Average Leverage			-0.0407*		
			(-2.05)		
Peer*Post*Industry Median Leverage				-0.0189*	
				(-1.98)	
Peer*Post*Relative Leverage Ratio					0.0001**
					(2.17)
Controls	Yes	Yes	Yes	Yes	Yes
Firm*Cohort FE	Yes	Yes	Yes	Yes	Yes
Year*Cohort FE	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.784	0.784	0.784	0.784	0.784
No obs.	3,996	3,996	3,996	3,996	3,996

Panel D: Adjusted Profit Margin					
Variables	(1)	(2)	(3)	(4)	(5)
Peer*Post	0.0028	-0.1300***	-0.0916***	-0.1055***	-0.0607***
	(0.09)	(-4.51)	(-3.59)	(-2.94)	(-4.63)
Peer*Post*Top 4 Market	-0.1220*				
	(-1.92)				
Peer*Post*Fraudulent Firm Leverage		0.2098***			
		(3.15)			
Peer*Post*Industry Average Leverage			0.1819**		
			(2.17)		
Peer*Post*Industry Median Leverage				0.1736*	
				(1.77)	
Peer*Post*Relative Leverage Ratio					0.0007
					(0.84)
Controls	Yes	Yes	Yes	Yes	Yes
Firm*Cohort FE	Yes	Yes	Yes	Yes	Yes
Year*Cohort FE	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.772	0.772	0.772	0.772	0.772
No obs.	11,354	11,354	11,354	11,354	11,354

Table 4.5 Switching costs

This table reports the results for the sub samples divided by R&D. The dependent variables include advertising spending (natural logarithm of 1 + advertising spending), advertising intensity (advertising spending scaled by total sales), scaled advertising (advertising spending scaled by assets) and adjusted profit margin ((earnings before interest + advertising)/sales). The sample is sorted into two groups according to the fraudulent industry's median R&D (excluding fraudulent firms) at year *t*-4. R&D is firm's research and development spending scaled by its total assets. Fraudulent firm's leverage is the leverage of a fraud firm at year *t*-4. In column (5) to (16), firms with missing advertising spending are excluded. Standard errors are clustered at firm level. ***, **, and * implies significance at the 1% level, 5% level, and 10% level, respectively.

Panel A: DID (High switching cost industries vs low switching cost industries)

	Adjusted Profit Margin		Advertising Spending	
Variables	(1)	(2)	(5)	(6)
	Low R&D	High R&D	Low R&D	High R&D
Peer*Post	-0.0379***	-0.0630***	0.0365	0.1813**
	(-3.29)	(-3.27)	(0.49)	(2.37)
Controls	Yes	Yes	Yes	Yes
Firm*Cohort FE	Yes	Yes	Yes	Yes
Year*Cohort FE	Yes	Yes	Yes	Yes
No obs.	6,151	5,203	2,112	1,884
Adj. R^2	0.801	0.746	0.857	0.858

Panel B: DIDID (High switching cost industries vs low switching cost industries)

	Adjusted Profit Margin		Advertising Spending	
Variables	(3)	(4)	(7)	(8)
	Low R&D	High R&D	Low R&D	High R&D
Peer*Post	-0.0739**	-0.1562***	0.0129	-0.3440
	(-1.99)	(-3.25)	(0.07)	(-1.36)
Peer*Post*Fraudulent Firm Leverage	0.0894	0.3323***	0.0740	1.7537**
	(1.10)	(2.61)	(0.15)	(2.37)
Controls	Yes	Yes	Yes	Yes
Firm*Cohort FE	Yes	Yes	Yes	Yes
Year*Cohort FE	Yes	Yes	Yes	Yes
No obs.	6,151	5,203	2,112	1,884
Adj. R^2	0.801	0.746	0.857	0.858

Panel C: DID (High switching cost industries vs low switching cost industries)

	Advertising Intensity		Scaled Advertising	
Variables	(9)	(10)	(13)	(14)
	Low R&D	High R&D	Low R&D	High R&D
Peer*Post	0.0020	0.0112***	0.0041	0.0059**
	(0.49)	(3.03)	(0.74)	(2.48)
Controls	Yes	Yes	Yes	Yes
Firm*Cohort FE	Yes	Yes	Yes	Yes
Year*Cohort FE	Yes	Yes	Yes	Yes
No obs.	2,112	1,884	2,112	1,884
Adj. R^2	0.788	0.725	0.789	0.766

Panel D: DIDID (High switching cost industries vs low switching cost industries)				
	Advertising Intensity		Scaled Advertising	
Variables	(11)	(12)	(15)	(16)
	Low R&D	High R&D	Low R&D	High R&D
Peer*Post	0.0041	0.0033	0.0062	-0.0011
	(0.35)	(0.73)	(0.46)	(-0.29)
Peer*Post*Fraudulent Firm Leverage	-0.0054	0.0243*	-0.0057	0.0227**
	(-0.17)	(1.86)	(-0.30)	(2.15)
Controls	Yes	Yes	Yes	Yes
Firm*Cohort FE	Yes	Yes	Yes	Yes
Year*Cohort FE	Yes	Yes	Yes	Yes
No obs.	2,112	1,884	2,112	1,884
Adj. R^2	0.788	0.725	0.789	0.766

Table 4.6 Switching cost and recent sales growth

This table reports the results for the sub samples divided by R&D and sales growth. The dependent variables include adjusted profit margin ((earnings before interest + advertising)/sales) and advertising spending (natural logarithm of 1 + advertising). The sample is sorted independently into four groups according to the fraudulent industry's median R&D and sales growth (excluding fraudulent firms) measured at year *t*-4. R&D is firm's research and development spending scaled by its total assets. Fraudulent firm's leverage is the leverage of a fraud firm at year *t*-4. In Panel B to D, firms with missing advertising spending are excluded. Standard errors are clustered by the number of firms. LL, LH, HL and HH indicates low sales growth and low R&D, low sales growth and high R&D, high sales growth and low R&D and high sales growth and high R&D, respectively. ***, **, and * implies significance at the 1%, 5% and 10% levels, respectively.

Panel A: Adjusted Profit Margin				
	LL	LH	HL	HH
Peer*Post	-0.0401**	-0.1412***	-0.0247	-0.0623**
	(-2.58)	(-3.64)	(-1.54)	(-2.55)
No obs.	2,763	1,512	3,388	3,691
Adj. R^2	0.778	0.743	0.818	0.748
Peer*Post	-0.0734*	-0.1149*	-0.0526	-0.1674***
	(-1.70)	(-1.68)	(-0.80)	(-3.04)
Peer*Post*Fraudulent	0.0896	-0.2548*	0.0659	0.3155**
Firm Leverage	(0.91)	(-1.72)	(0.48)	(2.18)
No obs.	2,763	1,512	3,388	3,691
Adj. R^2	0.778	0.743	0.818	0.748
Panel B: Advertising Spending				
	LL	LH	HL	HH
Peer*Post	0.0694	0.0985	0.0403	0.2121**
	(0.28)	(0.38)	(0.35)	(2.20)
No obs.	1,337	533	775	1,351
Adj. R^2	0.856	0.883	0.859	0.852
Peer*Post	0.0320	-0.1182	0.0720	-0.5006*
	(0.07)	(-0.27)	(0.16)	(-1.73)
Peer*Post*Fraudulent	0.1027	0.8028	-0.0759	2.3711**
Firm Leverage	(0.10)	(0.56)	(-0.05)	(2.05)
No obs.	1,337	533	775	1,351
Adj. R^2	0.856	0.883	0.859	0.852
Panel C: Advertising Intensity				
Peer*Post	0.0044	0.0015	-0.0022	0.0105***
	(1.07)	(0.44)	(-0.26)	(3.46)
No obs.	1,337	533	775	1,351
Adj. R^2	0.842	0.745	0.677	0.725
Peer*Post	0.0045	0.0008	0.0101	0.0026
	(0.50)	(0.12)	(0.14)	(0.16)
Peer*Post*Fraudulent	0.0019	0.0030	-0.0183	0.0292*
Firm Leverage	(0.08)	(0.17)	(-0.44)	(1.86)
No obs.	1,337	533	775	1,351
Adj. R^2	0.842	0.745	0.677	0.725

Panel D: Scaled Advertising				
Peer*Post	0.0056	0.0025	-0.0008	0.0080***
	(0.90)	(0.41)	(-0.11)	(3.21)
No obs.	1,337	533	775	1,351
Adj. R^2	0.836	0.694	0.653	0.792
Peer*Post	0.0078	-0.0001	0.0026	0.0002
	(0.59)	(-0.01)	(0.15)	(0.05)
Peer*Post*Fraudulent Firm Leverage	-0.0064	0.0094	-0.0081	0.0261*
	(-0.16)	(0.24)	(-0.19)	(1.98)
No obs.	1,337	533	775	1,351
Adj. R^2	0.836	0.694	0.653	0.792

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