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Abstract

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JEL Classification: G14, G30, M41

Keywords: Cost of Equity, Disclosure, transparency, Information Environment, Information Complementarity

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Information Complementarities and the Dynamics of Transparency Shock Spillovers*

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Abstract. We show that information complementarities play an important role in the spillover of transparency shocks. We 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.

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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.[1] This makes the empirical association between disclosure and the cost of capital

[1] 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.

difficult to identify—perhaps one reason why the empirical evidence has not been very convincing.[2]

In this paper, we address two major issues. First, we provide evidence of this complex interaction. We 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. We 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, we 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

[2] Leuz and Wysocki (2016) provide an extensive discussion of the difficulties of interpreting related empirical evidence.

market matters for investor participation, as highlighted in the influential work of Guiso, Sapienza, and Zingales (2008).

We establish that *information complementarity* plays an important role in the transmission of adverse disclosure shocks to other firms.[3] Specifically, we 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 we exploit is as follows. We 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). 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. We 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). We conduct our 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. We examine how the revelation of financial misconduct by a high-profile firm in a particular 4-digit (3-digit) industry affects the peer firms'

[3] As discussed in section 3, a group of firms are related via information complementarity if their fundamentals are perceived to be correlated.

implied cost of capital, relative to that of the control group.[4] We 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.[5]

We explore how the peer firms' disclosure responds to the revelation of financial misconduct by the high-profile firm. We 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. We 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.

We 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 firm, or the same institutional investor owns a peer firm, together with the high-profile

[4] Following the method of Pástor, Sinha, and Swaminathan (2008) and Chava and Purnanandam (2010), we 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.

[5] 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, we are 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). We find no such evidence.

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.[6] 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.

We 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. We also find that the subsequent increase in the frequency of management forecasts and the length of the MDA section is stronger for such firms.[7] We 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 our premise that information complementarity is likely to be weaker at the 2-digit industry level.

Interestingly, we find no evidence that having an auditor in common with the fraudulent firm exposes the peer firm more to the transparency shock spillover. Our 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

[6] See also Lee, Ma, and Wang (2016), Muslu, Rebello, and Xu (2014), and Israelsen (2016) for related findings.
[7] 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.

typically reflects on the quality of the auditor involved.[8] 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), we explicitly control for the Hoberg and Phillips (2010) product similarity score between the fraudulent firm and the close or distant peer firm. We examine whether firms with higher similarity scores have a higher cost of capital and increase disclosure after the misconduct is revealed. We find no such evidence, and the main results for close peers and firms linked via co-coverage and co-ownership remain.

Next, we examine the dynamic behavior of the cost of capital and disclosure in our difference-in-difference setting. Consistent with Clinch and Verrecchia (2015) and Leuz and Schrand (2009), we 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, we 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

[8] 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.

expense of equity financing following the revelation of misconduct by the high-profile firm. This is what we 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. We 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. Related Literature and Contribution

Our 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.

Our analysis differs from Leuz and Schrand (2009) in the following respects. First, we focus not only on one event, but on many events, and thus we can examine the effect of financial misconduct by high-profile firms on peer firms in a staggered difference-in-difference setting. We 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 our results. Moreover, disclosure practices may reflect economic fundamentals (Leuz and Wysocki, 2016), and thus it is appropriate that our control sample is chosen to reflect at least some of these fundamentals.[9]

Second, we 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 our analysis is that, by examining the effect of common analysts and common ownership, we provide evidence that information complementarity plays an important role in the transmission of voluntary disclosure shocks. Our 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, we show that peer firms' security issuance activity is affected in a manner consistent with great information uncertainty or asymmetry surrounding the misconduct events.

Our 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 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

[9] As we demonstrate, there is little evidence of any spillover to distant peers. In additional tests reported in Appendix D, we compare the effects on the 2-digit peers relative to a control sample drawn from firms with a different 1-digit SIC code. We 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.

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.

Our 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 we 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. Our 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, we 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 our setting.

Our 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 our 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 our paper are consistent with this conjecture.[10] Our 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 our results, they find that after the loss of analyst coverage, liquidity initially decreases and then improves.

Third, we 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

[10] 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."

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 our 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 our analysis. Our results point to previously unexplored channels through which disclosure events experienced by one firm are likely to affect the disclosure decisions of other firms. We find that information complementarities play a key role in the transmission of transparency shocks. We 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. Our 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.[11]

[11] Our 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

Finally, our 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). We 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). Our 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, 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.

of restatements, rival firms update their beliefs about other firms' strategic decisions and adjust their investment decision accordingly.

3. Hypothesis Development and Empirical Implications

Our 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), we 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.[12] We focus on high-profile financial misconduct committed by industry leaders (S&P 500 firms that were accused of 13(b) violations). We 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 our 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

[12] 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.

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

We proxy for the presence of information complementarity with the high-profile fraudulent firm in two ways. Our 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. We 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). We 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, our empirical approach is designed to test whether the spillover effects are stronger for firms with which the informational complementarities are likely to be stronger.[13] We 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.

We 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. We accordingly hypothesize that within 4-digit (alternatively, 3-digit) same industry peers, the

[13] However, as shown in Appendix D, we find that such spillovers to the chosen control firms are absent.

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, we 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*). We 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. 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, our 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, our 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, we 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.). Our 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, we 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 our 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.

We 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 our setting, we 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, we 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.

Our 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.

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. We describe each data source in detail and outline the construction of the variables used in this paper.

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. Our 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, we 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[14]:

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;

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.

We identify 670 SEC enforcement actions against violations of 13(b) rules from 1999 to 2015 and track these firms in Compustat. Our 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), we hand-collect trigger events that attract the regulator's attention and prompt informal inquiry

[14] 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).

and possibly a formal letter of an investigation by the SEC. Most of these trigger events are documented in the enforcement proceedings. We 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.

We focus on financial misconduct committed by high-profile industry leaders, i.e., S&P 500 firms that were accused of 13(b) violations.[15] These firms were in the S&P 500 when their financial misconduct was revealed to the public for the first time. We exclude financial and utility firms. In total, we 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, we only include the date when the financial misconduct of the first firm becomes known to the public as the event date for that industry.

The figure in Appendix C 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 our 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).[16]

4.2. Implied cost of capital

We 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

[15] Beatty, Liao, and Yu (2013) study the effect of Fortune 500 firms' frauds on industry peers' investment during the misconduct period.
[16] We 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, we did not find any cases of high-profile financial misconduct revelation after 2007.

cash flows.[17] We 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. We 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 B.[18] The ICC used in the analysis is adjusted using the risk-free rate.

4.3. Financial disclosure

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

Our 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,

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[17] What we estimate is the implied cost of equity, but following the literature, we use the terms cost of capital and cost of equity interchangeably.

[18] Our ICC construction closely follows the methodology described by Chava (2014).

2013). We collect both quarterly and annual forecasts on earnings per share. Our 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.

Our 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, we first download all the 10-K fillings between 1996 and 2019 from SEC EDGAR.[19] Then we 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), we remove all the HTML tags

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

(i.e., tables) from the MD&A. Finally, we construct our dependent variable, *LengthMDA*, as the natural logarithm of one plus the number of words in the MD&A section in 10-K filings.

Our 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 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. We closely follow Morck, Yeung, and Yu (2000) and Jin and Myers (2006) to calculate R^2 from the market model:

$$r_{it} = \alpha_i + \beta_i r_{m,t} + \epsilon_{it} \quad (1)$$

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. We exclude stocks that have less than 30 weekly returns in a particular year in our sample. We measure a firm's stock market synchronicity in a year by estimating the R^2 of the regression in Eqn. (1) for that year. Our disclosure variable proxy is an inverse measure of synchronicity, given by $\log(1 - R^2)$.

4.4. Equity and debt issuance

Following Leary and Roberts (2014), we use net equity issuances and net debt issuances as dependent variables to measure firms' financing activities. Our 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. Our 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. We confirm the robustness of our results by using a 2% or 1% cutoff for equity issuance and debt issuance.

4.5. Common analyst coverage and common ownership

We 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), we 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 we 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.

We construct our common large shareholder measures as follows. For each quarter in our sample period, we 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. We 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, we 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.

5. Empirical Methodology and Results

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

5.1 Methodology

We 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 our study as revelations of financial misconduct are multiple treatment events that occur at different times (see Gormley and Matsa (2011)). Specifically, we 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 we 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 we 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.[20] The control group from the same 2-digit industry is desirable to properly control for the underlying economics (at the 2-digit level).[21] We first construct a cohort of control and treated

[20] 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).

[21] 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 our empirical design. In our regressions, we 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.

firms starting three years prior (excluding revelation year) and extending to four years after the revelation of financial misconduct.[22] We 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:

$$Y_{ict} = \alpha_0 + \alpha_1 Peer_{ic} * Post_{ict} + Controls_{ict} + \theta_{tc} + \gamma_{ic} + \epsilon_{ict} \quad (2)$$

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), we 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. We cluster the standard errors at the firm level. Financial firms, utility firms, conglomerates, and government entities are excluded.

Table 1, Panel A reports descriptive statistics for the outcome and control variables used in our 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, we 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

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

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 8.47 and 8.30 for the peer firms and control group, respectively. Our 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), we 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 ratio and operating performance.

[Insert Table 1 here]

There are two important issues that need to be addressed to validate a causal interpretation of our 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 5.4, we take advantage of the fact that in most of the cases of fraud in our sample, the actual period during which fraud is committed precedes the year the fraud is revealed. We 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 5.5, we 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, we 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, we 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:

$$Y_{ict} = \eta_0 + \eta_1 Peer_{ic} * Post_{ict} + \eta_2 * Peer_{ic} * Post_{ict} * Common_{ic} + \eta_3 * Post_{ict} * Common_{ic} + Controls_{ict} + \theta_{tc} + \gamma_{ic} + \epsilon_{ict} \quad (3)$$

In specification (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 our 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, we 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. (3). Similarly, we also identify common auditors and in robustness tests, include interactions with the common auditor dummy.[23]

5.2 Cost of capital and transparency shock spillover

In this section, we 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 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, we report four sets of results (four columns). The first two columns report results for specifications that drop all firm-level controls, to ensure 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), we 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

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

Compustat database and are lagged by at least six months.[24] The variable definitions are given in Appendix A. Standard errors are heteroscedasticity-adjusted in columns (1) and (3), and clustered by firm in columns (2) and (4).

[Insert Table 2 here]

Consistent with Hypothesis 1(i), we 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 we shall see below in section 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, we 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, we 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 3 and 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

[24] 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.

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 and the associated information complementarity drives the results in Table 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. We 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.[25]

Finally, we 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 our 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, we 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 3 and Table 4 here]

To verify the robustness of our findings, in Online Appendix Table OA1, we estimate the regression specification (2) with an alternative close peer group which comprises firms in the same 3-digit industry as the fraudulent firm. Our 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 and OA3, we interact *Peer*Post* with the

[25] 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.

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.

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. We next examine how firms' disclosure decisions respond to the increase in the cost of capital after the negative transparency shocks (Hypothesis 2). We test Hypothesis 2(i) in Table 5.

Panels A, B, and C of Table 5 provide the estimation results of Eqn. (2) in which we adopt various measures of corporate disclosure. In Panel A of Table 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, our 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 5 here]

We 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 6. For the frequency of management forecasts and the number of words in the MD&A section, we 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. Our 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, we find no such effect for *log* $(1-R^2)$, 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 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 6, we 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. We 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 Online Appendix Tables OA4, we repeat the tests based on the 3-digit classification of close peers. One noticeable difference is that once we 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.

Our 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 D, we show that the spillover effects already fade away and are no longer discernible when we 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. Our difference-in-difference regressions, similar to those in Tables 2 and 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, we 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 we show in the next section, the relationship between disclosure and cost of capital in our setting is, in fact, more nuanced than what the results discussed so far might suggest. While we cannot establish a direct causal link, we find evidence that a commitment to more disclosure does lower the cost of capital, as the literature has typically assumed.

Second, our 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, we find that information complementarity is weak when firms are not close product market peers.[26] 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).

[26] 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.

5.4 Could common (industry) shocks explain our results?

For a causal interpretation of our 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, we 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, we should find that the outcome variables for the close peers begin to diverge 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), we 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, we augment the regression specification in Eqn. (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, we 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 E, we report the regression results with the cost of equity and the three disclosure measures as our dependent variables. The coefficient of *Peer*Before* is insignificant in all regressions, but that of *Peer*Post* remains positive and significant.

5.5 The dynamics of cost of capital and disclosure

In this section, we conduct further tests to examine how the impact of the industry leaders' financial misconduct on treated firms varies over time (Hypothesis 3). We construct a dynamic difference-in-difference model by running the same OLS regression as Eqn. (2), adding an indicator variable for the year before the transparency shock, and splitting the dummy variable $Peer_{ic} * Post_{ict}$ by year:

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

In specification (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 our 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 7, we examine the dynamic behavior of each of our disclosure measures, and in Table 8, we examine the dynamic behavior of the cost of capital. Consistent with Hypothesis 3(i), our 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 our 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, we 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 our results.

[Insert Table 7 here]

In the face of this sustained increase in disclosure activity, the cost of capital shows interesting dynamics. As shown in Table 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 we 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 8 here]

Online Appendix Tables OA5 and OA6 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.

5.6 Transparency shocks and financing

So far, our 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, we 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 we do not attempt to resolve the small firm financing puzzle,[27] our 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, we 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, we should expect debt issuance to increase at the expense of equity issuance following the shock. In Panel

[27] 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).

A of Table 9, we define debt (equity) issuance to occur if net debt (equity) issuance exceeds 3 percent of the book value of assets.[28] We report results for a linear probability model, and the specification is similar to that in Eqn. (2). We 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, we examine the dynamics of issuance activity, in a specification similar to Eqn. (4). Consistent with our earlier results that the adverse effect on the cost of capital is mitigated after the first two years (possibly in response to consistently higher disclosure), we find that there is no longer any significant difference in the financing behavior between close and distant peers after the second year.

[Insert Table 9 here]

Online Appendix Table OA7 reports 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.

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, we attempt to identify the

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

consequences of such adverse transparency shocks for close industry peer firms. We 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.

Our results also address the relatively underexplored issue of channels of disclosure spillover. We 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, we contribute to a contentious literature that asks whether a firm's information environment is a first-order determinant of its financing choices. Our 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.

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Appendix A
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 A—*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 B
The methodology for constructing the implied cost of capital

We 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 we 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 $t+k$. We 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. We 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.[29] We 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%. We 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. We 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:

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

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

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

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

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

Next, we 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, we set it to the median payout ratio of the industry (2-digit SIC code). We 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,* we set the plowback ratio to the ratio calculated from the above procedure. Then, we 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, we assume the growth rate of earnings (*g*) equals the return on new investment times the plowback ratio. We 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:

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

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

We compute the terminal value as the perpetuity:

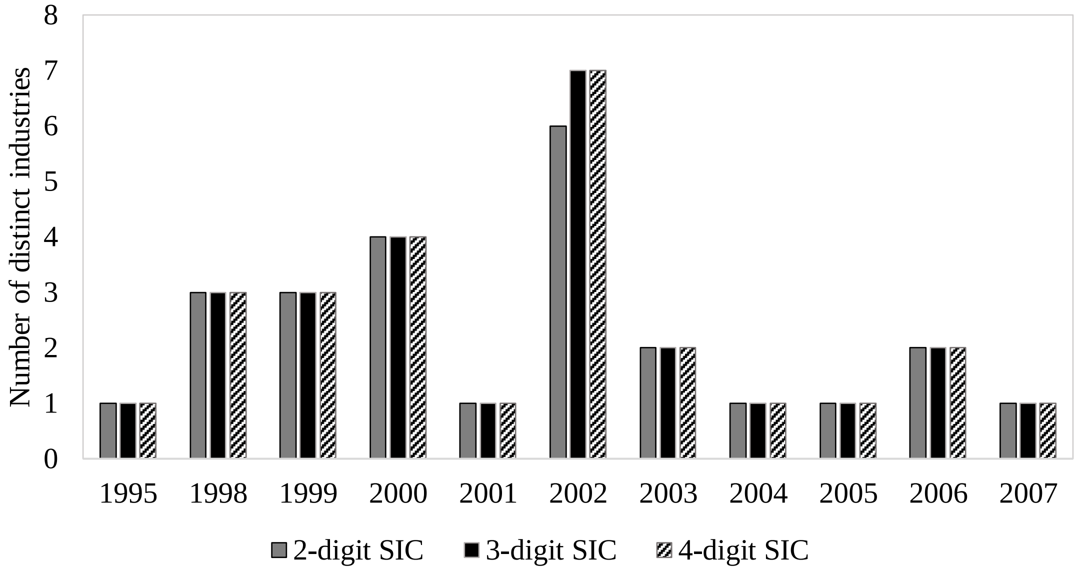
$$TV_{i,t+T} = \frac{EPS_{i,t+T+1}}{r_{i,e}} \tag{10}$$

Then, we 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 C

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 our regression sample.

Appendix D
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, we 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 A. 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 D—*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 E
"Fraud years", cost of equity, and disclosure

We 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 A. 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 1

Descriptive statistics

Panel A reports summary statistics for the outcome and control variables used in our empirical analysis. Detailed variable definitions are in Appendix A. 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

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 A. 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 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 A. 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 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 A. 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 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 A. 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 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 A. 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 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 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 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 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 A. 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 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 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 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 A. 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 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 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 (-0.04)	-0.0017 (-0.08)	0.0218 (0.84)	0.0203 (0.79)
$Peer * Year_{t+1} = 1$	-0.0302 (-1.18)	-0.0249 (-0.98)	0.0569** (2.20)	0.0490* (1.93)
$Peer * Year_{t+2} = 1$	-0.0789*** (-2.99)	-0.0716*** (-2.73)	0.0652** (2.48)	0.0608** (2.35)
$Peer * Year_{t+3} = 1$	-0.0381 (-1.39)	-0.0332 (-1.22)	0.0395 (1.51)	0.0443* (1.74)
$Peer * Year_{t+4} = 1$	-0.0130 (-0.47)	-0.0009 (-0.34)	0.0277 (0.95)	0.0219 (0.77)
Sales		-0.0016 (-0.17)		0.0656*** (6.36)
Market-to-Book		0.0518*** (16.97)		-0.0119*** (-4.45)
Profitability		0.0230 (0.61)		-0.1858** (-5.23)
Tangibility		-0.3284*** (-5.23)		0.0664 (1.03)
Investment		0.0763*** (7.68)		0.1891*** (18.12)
Z score		-0.0005 (-0.16)		-0.1591*** (-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