Proactive Financial Reporting Enforcement and Shareholder Wealth

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Abstract: Within the UK's proactive financial-reporting enforcement regime, we examine the effect of increased regulatory scrutiny on equity values. We find that a fourfold increase in the likelihood of regulator-initiated reviews of financial reports reduces equity values by 1.3% on average. Reductions in equity values are largest for firms with strong private oversight that likely ensures that they are closer to their equity-value-maximizing level of transparency. Additional evidence suggests that competition increases and that managers' investment horizons decrease in industries selected for increased regulatory scrutiny, consistent with direct compliance costs not fully explaining the reduction in equity values.

Keywords: Financial reporting enforcement, costs and benefits of enforcement, proprietary costs, managerial short-termism, unintended consequences of regulation.

JEL Classification: G14, G18, G38, K22, M41

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1. Introduction

Public enforcement of financial-reporting regulation has dramatically increased over the past two decades. Many recent initiatives require regulators to *proactively* identify reporting deficiencies, instead of only reacting to specific complaints or imposing sanctions for previously identified instances of misreporting.[1] Although prior research documents significant capital-market benefits of broad changes in the enforcement of securities regulation, there are potentially also significant direct and indirect costs associated with increased enforcement. Such costs could be particularly pronounced for proactive financial-reporting enforcement, which presumes that regulators can identify deficiencies in (publicly observable) accounting policies that other stakeholders cannot. In this paper, we provide evidence on the effect of increased regulatory scrutiny on equity values within the context of the UK's proactive financial-reporting enforcement regime (hereafter PFRE).

Increased public oversight of financial reporting is typically justified by individual firms' failure to internalize positive reporting externalities and by the difficulty of privately enforcing disclosure-policy commitments (e.g., Shleifer 2005). In the presence of such frictions, a publicly enforced mandatory reporting regime could increase equity market values by bringing disclosure closer to its value-maximizing level. However, the case for public enforcement relies on the inability of private contracting and market discipline to mitigate the factors that could give rise to suboptimal financial reporting transparency (Coase 1960) and on the existence of a benevolent and capable government to oversee enforcement efforts (Stigler 1971). Although private solutions are likely imperfect, the economic importance of the frictions these mechanisms leave unresolved is unclear. Moreover, even if public regulators are effective in enforcing penalties for reported violations, it is less clear whether they have the resources and expertise to *proactively* identify and

[1] For example, the Sarbanes-Oxley Act requires the SEC to proactively review the disclosures of all publicly traded issuers. It also established the Public Company Accounting Oversight Board, which is required to inspect all audit firms of SEC-registered companies on an ongoing basis. Similarly, the Transparency Directive requires each European Union member state to establish a proactive review process for financial reports.

correct suboptimal accounting policies that market forces cannot—especially in countries with highly developed capital markets. Increasing transparency (beyond the observed market-equilibrium level) could impose significant direct (e.g., compliance) and indirect (e.g., proprietary, managerial myopia) costs on firms. Ultimately, there are arguments both for and against increasing proactive public oversight, making it an empirical question whether and under what circumstances the benefits to shareholders of PFRE outweigh the costs.

To provide evidence on this question, we examine the proactive, focus-sector review program of the Financial Reporting Review Panel (FRRP)—a UK regulatory body (similar to the SEC Enforcement Division's Financial Reporting and Audit Group) charged with ensuring compliance with financial-reporting regulations. From 2004 to 2011, the FRRP annually selected "focus sectors;" companies in these focus industries were over four times more likely to be subject to an FRRP review of their financial statements. The reviews assess firm compliance with existing reporting standards and, given the exposure of deficiencies, require firms to prospectively change their accounting policies (e.g., by expanding segment and/or related-party disclosures). To encourage preemptive reporting changes, the FRRP publicly announces its focus sectors (usually four to five targeted industries) in a press release before the end of the fiscal year subject to review. This setting has two key advantages: first, the changes in PFRE are not bundled with other regulatory changes; second, there are multiple shocks to expected enforcement intensity around the eight focus-sector announcements. The main challenge is that the focus-sectors are not selected purely at random—an issue that we address in our empirical analyses.

We start by examining how the FRRP selects focus sectors. We find that characteristics such as low stock returns, negative media attention, and the public revelation of accounting deficiencies (measured over the year leading up to the announcement) are significant determinants of the targeted industries. That the focus sectors are determined (in part) by these publicly observable factors suggests that regulators choose sectors where other market participants are likely also aware of the reporting deficiencies and have adjusted prices accordingly.

To identify the effect of PFRE on equity values, we exploit the abrupt changes in enforcement intensity for focus-sector relative to non-focus-sector firms and estimate short-window stock returns around the eight annual FRRP focus-sector announcements (from 2004 to 2011).[2] To mitigate the potential influence of confounding industry events that coincide with the announcements (e.g., negative performance trends), we adjust UK returns using either US or non-UK-European returns from the same industry. Hence, our identification strategy controls for concurrent events that are UK specific (by comparing focus to non-focus-sector returns) and industry-specific events (by comparing UK to US/European returns in the same industry). Using a five-day return window centered on the focus-sector announcement date, we find that an approximately fourfold increase in PFRE intensity leads to an average reduction of 1.3% in the market value of equity. Looking individually at each of the eight announcements, we find that the average return is negative in all but one year and significantly negative in five, indicating that the sign of the effect is unambiguous and independent from macroeconomic conditions. Additional evidence indicates that this significant decline in equity value occurs even though focus-sector firms experienced an improvement in liquidity similar to that documented by prior research in other settings (e.g., Christensen et al. 2016, Silvers 2018).

To further assess the economic significance of the observed decrease in equity values, we estimate focus-sector announcement returns by quintiles of firm market capitalization and find that the magnitude of the average stock return decreases monotonically in firm size. For the smallest quintile of firms (average market capitalization of $15 million), the average price response is -2.4% (i.e., a reduction in equity value of approximately $350,000). For the largest quintile of firms (average market capitalization of $3.4 billion), the average price response is -0.6% (i.e., a reduction in equity value of approximately $19.0 million).

[2] Because the treatment is determined by industry, our estimates of PFRE's impact include any positive within-industry externalities from increased disclosure but exclude externalities for firms in other industries. Prior research typically assumes that positive externalities from disclosure accrue within an industry (e.g., Wang 2014, Breuer 2018, Shroff et al. 2017). Relatedly, our estimates do not include the economic benefits of transparency that accrue to stakeholders other than shareholders, such as tax authorities and bondholders (e.g., Chow 1983).

As noted above, since focus sectors are not selected (completely) randomly, attributing the reduction in equity values to the costs of PFRE is complicated by the possibility that the negative market reaction could be caused by the FRRP's revelation of private information about financial-reporting quality in the focus-sector industries. Institutionally, this explanation seems unlikely because the accounting policies the FRRP examines are publicly observable, the FRRP's proposed changes are almost always prospective, and the focus-sector announcements are for entire industries and not specific firms. It seems unlikely that the FRRP would know more about industry-wide, (value-relevant) accounting-policy deficiencies than would sophisticated market participants in the UK.

We also provide empirical evidence inconsistent with a private-information-based explanation of our results. First, within the sample of reviewed firms, the FRRP detects fewer reporting weaknesses for focus-sector than for non-focus-sector firms. Second, we find that focus-sector announcement returns are less negative for highly regulated focus-sector industries (e.g., financial services, insurance, utilities) and for firms (within the same industry) listed on a more heavily regulated segment of the London Stock Exchange (LSE) where it is more likely that the FRRP could obtain private information from other regulators. Third, we find that announcement returns are more negative for firms with large external blockholders, where private oversight likely ensures that firms are closer to their optimal level of transparency and where it is less likely that the FRRP has private information unknown to investors. Fourth, we find no evidence of changes in information asymmetry around the focus-sector announcements. Fifth, we find that a firm's inclusion in a focus sector has a smaller effect on audit committee turnover than does a restatement. Although none of these analyses definitively rule out a private-information-based explanation, taken together, they reduce its plausibility.

Our analyses suggest that for shareholders of firms in targeted industries, the costs of additional enforcement scrutiny outweigh the benefits. Next, we explore some specific costs that could explain the observed reduction in equity values. In addition to direct compliance costs,

increased disclosure can also have an indirect effect on reporting firms and their competitors. We examine two potential value-decreasing real effects: increased competition and changes in managers' investment horizons. Although the theoretical foundations for these effects are well established, empirically identifying them is challenging because of the subtlety of their manifestation in the data and the indirectness of the empirical proxies for the constructs of interest. For these reasons, our tests necessarily have low power. That said, our results suggest that both proprietary costs and changes in investment horizons are potential (partial) explanations for the observed reductions in equity values for focus-sector firms. Consistent with focus-sector firms incurring proprietary costs from increased disclosure (through increased competition), we find that the largest equity-value reductions occur among firms with abnormally high profits before the focus-sector announcements and that profitability mean reverts faster in focus-sector industries after the FRRP targets them for increased scrutiny. Consistent with managers adopting a shorter-term investment horizon, we find an increase (decrease) in the association between current-period capital expenditures and one-year-ahead (two-year-ahead) cash flows after the FRRP's focus-sector reviews.

Overall, our analyses suggest that by increasing transparency beyond the value-maximizing level, greater PFRE intensity reduces the equity values of firms targeted for increased regulatory scrutiny. Because an increase in enforcement is often motivated by a desire to protect shareholders, isolating the net effect on equity values is important. Yet despite the significant evidence on capital-market benefits, prior research provides few assessments of the net effect of securities regulation on equity values, particularly in the context of financial reporting enforcement. Existing work that does examine the net benefits to shareholders focuses primarily on major reforms that simultaneously affect many aspects of regulation, including both financial-reporting requirements and the enforcement thereof (e.g., the Securities Acts of 1933 and 1934, the Sarbanes-Oxley Act, Regulation Fair Disclosure, or the Multilateral Memorandum of

Understanding).[3] In contrast to this prior work, we examine the impact of increased financial-reporting enforcement in isolation—holding disclosure rules constant, independent of other regulatory changes—and document an instance where, despite greater transparency, increasing PFRE intensity decreases equity values.

Our paper also provides evidence that increased proprietary costs and changes in managers' investment horizons are potential channels through which increased PFRE intensity can reduce equity values. There is an extensive literature examining how competition affects disclosures (see Beyer et al. 2010 for a review), but there is limited evidence on how disclosure affects competition (Bernard 2016, Breuer 2018, Berger et al. 2019, and Glaeser and Omartian 2019 are recent exceptions) and, to our knowledge, there is no evidence that isolates the effects of increased financial reporting enforcement on competition. Prior work also shows that increased disclosure can improve outside investors' ability to monitor management's investment policies and thus can increase firm value (e.g., Bushman and Smith 2001, Healy and Palepu 2001, Biddle and Hillary 2006, Lambert et al. 2007, Biddle et al. 2009, Goodman et al. 2013, Cho 2015). However, there is little evidence on how greater financial reporting enforcement, along with any accompanying increase in disclosure, can affect managers' investment horizons (Kraft et al. 2017 is an exception).

There is also a vast, related literature that examines the effects of ex-post remedial actions undertaken by the SEC and FRRP, including enforcement actions and comment letters.[4] In contrast, we examine an *ex-ante* increase in expected enforcement intensity. While an ex-post enforcement action is unambiguously bad news for a firm, our ex-ante focus allows us to speak to

[3] See, for example: Stigler (1964); Benston (1969) and (1973); Jarrell (1981); Chow (1983); Shehata (1991); Heflin et al. (2003); Bushee et al. (2004); Gintschel and Markov (2004); Chen and Yuan (2004); Bushee and Leuz (2005); Francis et al. (2006); Mahoney and Mei (2006); Chhaochharia and Grinstein (2007); Wang (2007); Zhang (2007); Li et al. (2008); Doidge et al. (2010); Coates and Srinivasan (2014); Khan et al. (2018); and Silvers (2018).

[4] Regarding enforcement actions, see, for example: Feroz et al. (1991); Dechow et al. (1996); Beatty et al. (1998); Bonner et al. (1998); Beneish (1999); Hines et al. (1999); Peasnell et al. (2001); and Farber (2005). Regarding comment letters, see, for example: Robinson et al. (2011); Cassell et al. (2013); Dechow et al. (2015); Bens et al. (2015); Kubick et al. (2016); Laurion et al. (2016); Johnston and Petacchi (2017); and Ryans (2019). Florou et al. (2019) find that the FRRP focus-sector review program leads to an increase in audit fees and a decrease in earnings management, but it does not speak to the effect of PFRE on equity values.

shareholders' expectations of the net benefits from an increase in enforcement intensity, which are likely contingent on PFRE mitigating a friction not addressable through private contracting. We find that although increased financial reporting enforcement leads to greater disclosure and an increase in stock-market liquidity, within the UK's PFRE regime, the costs of additional regulatory scrutiny appear to outweigh the benefits to shareholders.

Importantly, because we conduct our analyses in the UK, which is one of the world's most transparent and informationally efficient capital markets, our findings are specific to this particular institutional context and might not carryover to other settings with less transparency or weaker existing regulations. However, we document capital-market benefits that are comparable to those in prior studies, suggesting that our enforcement shock is similar in at least one important way. Additionally, our analyses suggest that PFRE imposes the highest costs on the most transparent firms and can increase competition and affect managers' investment horizons; these findings highlight mechanisms that could be generalizable to other settings.

2. Costs and Benefits of Public Enforcement

Conceptually, there are arguments both for and against securities regulation and its enforcement. On the benefits side, numerous papers highlight the potential for positive externalities, the limitations of private enforcement, the need for a binding commitment mechanism, and cost savings (e.g., Coffee 1984, Easterbrook and Fischel 1984, Johnson et al. 2002, Zingales 2009, Leuz and Wysocki 2016). On the costs side, the literature points out the difficulties of ensuring effective enforcement and the potential for regulatory capture (e.g., Stigler 1971, Posner 1974, Peltzman 1976, Becker 1983). Ultimately, as argued by Djankov et al. (2003), lawmakers face a tradeoff between "disorder and dictatorship" when designing regulatory framework; thus, determining the benefits of publicly enforced securities regulation is largely an empirical matter.

In the specific context of financial reporting, public enforcement is generally motivated by either the failure of individual firms to internalize positive reporting externalities or the lack of

private commitment mechanisms for disclosure policies (e.g., Kothari et al. 2010). Reporting externalities arise when corporate disclosures provide information that is relevant to the valuation of other firms (e.g., Foster 1981, Dye 1990, Admati and Pfleiderer 2000, Shroff et al. 2017). The lack of a private commitment mechanism for managers and the ex-post coordination problems among dispersed shareholders leads to a suboptimal level of disclosure (Coffee 1984). This opacity could exacerbate agency conflicts between managers and shareholders, leading investors to price protect; this, in turn, prevents the financing of otherwise profitable investments. Although the conceptual arguments for these mechanisms are strong, their economic importance is less clear when it comes to the public enforcement of accounting standards. For instance, there are many market-based solutions that could mitigate commitment and coordination problems (e.g., auditors, analysts, outside blockholders, private litigation).

If the market frictions created by externalities and commitment problems are not as pervasive as predicted by the proponents of greater public enforcement, increasing the level of regulatory oversight might provide few benefits while imposing significant costs on firms. The direct costs of disclosure include, for example, expanded information systems, a more thorough (external or internal) audit, and increased managerial scrutiny of financial reports. Indirect costs (i.e., real effects) could include proprietary disclosure costs and an increase in myopic investing.

If PFRE leads to an increase in disclosure that a firm's competitors can profitably exploit, it could escalate competition in the focus-sector industries (i.e., an increase in proprietary disclosure costs as shown in Verrecchia 1983, Feltham et al. 1992, and Hayes and Lundholm 1996). Many of the FRRP's specific recommendations for remediation (see Appendix A) involve information that is unlikely to be publicly available elsewhere (e.g., segment disclosures), and thus addressing these suggestions could potentially provide useful information to competitors (Berger and Hann 2007).

In addition to proprietary costs, increased disclosure could also lead to changes in firms' investment policies. On the one hand, increased disclosure could improve outside investors' ability

to monitor management's investment policies and increase firm value (e.g., Bushman and Smith 2001, Healy and Palepu 2001, Biddle and Hillary 2006, Lambert et al. 2007, Biddle et al. 2009, Goodman et al. 2013, Cho 2015). However, indefinitely increasing disclosure is unlikely to be value maximizing and could lead to myopic investment. Stein (1989) shows that an increase in the informativeness of current earnings can increase the price pressure that managers face, leading to less efficient, short-term-focused investing. In the Stein (1989) model, if short-term investments translate into earnings faster than long-term investments and if investors can observe current investments but not their payout horizon, managers have an incentive to choose shorter-term investments even if these investments reduce firm value.[5]

In the end, there are arguments both for and against increasing public oversight and, as with securities regulation more generally, it is an empirical question as to whether the benefits to shareholders of increased proactive financial reporting enforcement outweigh the costs.

3. Institutional Setting

For (at least) two reasons, the settings examined in prior research have precluded researchers from drawing conclusions about PFRE's net effect on equity values. First, the regulations examined in prior research (e.g., SOX, IFRS, Reg. FD) often bundle many different changes, both to required disclosures and their enforcement. This makes it difficult to isolate the effect of PFRE.[6] Second, the costs of enforcement are rarely observable and most research, though acknowledging the existence of costs, only examines the benefits of enforcement. Our setting allows us to overcome these issues, though it has some important limitations. Below, we provide institutional details about the FRRP's focus-sector review program and discuss the pros and cons

[5] Using a similar theoretical framework, Ewert and Wagenhofer (2005) focus on the effect of tightening accounting standards and Gigler et al. (2014) examine the effect of increasing reporting frequency; both reach the conclusion that increased disclosure can have real effects that reduce firm value.

[6] It is conceptually difficult to distinguish between the content of disclosure rules and the enforcement of those rules because the effect of enforcement will naturally depend on the content of the rules. However, this argument does not imply that studying the effect of public enforcement is the same as studying the effect of disclosure rules. For instance, rules can have an effect without public enforcement if they change market participants' coordination abilities or litigation outcomes. Similarly, public enforcement (even of "good" rules) may have no (or a negative) effect if public bureaucracies are inefficient or captured by special interests.

of this setting.

3.1 History of the FRRP

The FRRP was established in 1991 as a subsidiary of the Financial Reporting Council (FRC) and is responsible for the enforcement of financial reporting rules for publicly listed and large private firms in the UK. From its inception until the early 2000s, the FRRP was a reactive body that reviewed financial reports only in response to investor complaints, referrals from other regulators, or press reports (Brown and Tarca 2007). In 2002, following the collapse of Enron and the ensuing equity-market turbulence, the Chancellor of the Exchequer ordered a review of financial regulation in the UK, including financial reporting and auditing standards (Fearnley and Hines 2003). In 2003, the UK announced major reforms that, in addition to the existing complaint and referral system, included an explicit mandate for the proactive monitoring of financial statements by the FRRP.

The FRRP selects accounts for review using a risk-based approach that considers the probability of a breach of accounting requirements and the impact such a breach would have on the market and on investor confidence. If the FRRP identifies a possible breach, it engages the management of the (potentially) non-compliant firm through a comment and review process. If, after review, the FRRP still believes the company is not compliant, it will encourage the company to change its accounting policies either through a restatement or through a prospective change in reporting practices (in reality, changes are almost always prospective). While the FRRP has no direct authority to force firms to cooperate, it can seek a court order requiring that the changes be made if firms resist requests to make the changes voluntarily. If the court approves the FRRP's request, the company's directors are personally liable for any court costs and expenses incurred (Companies Act 2006). To date, however, the FRRP has never taken a case to court.

3.2 FRRP Reviews and Changes to Accounting Policies

In Table 1 Panel A, we provide descriptive statistics on the annual review activity of the

FRRP in total and separately for the Main Market, AIM, and large unlisted (i.e., private) firms.[7] Panel A reports that, on average, the FRRP performs 205 proactive and 45 complaint/referral-based reviews per year, and that 126 (26) Main Market, 54 (10) AIM, and 25 (10) unlisted companies are proactively reviewed (reviewed based on a complaint or referral) each year. The large number of proactive reviews, both in absolute terms and relative to the complaint/referral-based reviews, indicates that there was a significant increase in enforcement activity under the focus-sector review regime. The time-series variation in the number of approached companies indicates that the FRRP's proactive review activity increased notably beginning in 2006.

The FRRP approaches approximately 48% of all reviewed companies regarding potential reporting issues. In Appendix A, we provide details on some of the specific reporting issues identified by the FRRP inspections.[8] The importance of the accounts with identified issues and the lasting nature of the changes requested (e.g., revenue recognition practices and liability and segment disclosures) make it plausible that firms' responses to the FRRP reviews would be substantial and lead to significant increases in disclosure.

The summaries in Appendix A also show that the overwhelming majority of issues identified by the FRRP's proactive focus-sector reviews leads to prospective changes in firms' accounting policies but rarely to the retroactive restatements of accounts (e.g., due to reporting fraud, unintentional errors, or other issues that might be unobservable to investors). The policies that the FRRP targets are publicly observable, suggesting that sophisticated investors would likely already be aware of any significant, value-relevant weaknesses in these policies. This feature of the setting makes it plausible that our empirical analyses reflect the effect of enforcement rather than the revelation of the FRRP's private information.

[7] In the FRRP annual reports, there is no specific reference to a bright-line size threshold for a "large" private firm. The FRC's current (2018) definition is a firm with 1) more than 2,000 employees globally, or 2) turnover of more than £200 million and total assets of more than £2 billion globally.

[8] The FRRP activity reports do not specifically disclose which issues were identified as a result of the proactive focus-sector reviews and which arose from other targeted reviews.

3.3 Focus Sectors and their Selection

From 2004 to 2011, one key aspect of the FRRP's proactive review process was the selection of "focus sectors" that would be subject to enhanced regulatory scrutiny through a higher likelihood of facing a proactive FRRP review of the next year's financial statements.[9] To deter misreporting and to give companies a chance to improve their compliance before their accounts are published, focus sectors are announced (through a press release on the FRRP's website) several months before the release of the financial statements for the fiscal year during which the sector is subject to the increased likelihood of inspection.[10] Hence, when the FRRP announces the focus sectors, there is an abrupt increase in the probability of a financial statement review for firms in the announced focus sectors and an abrupt decrease in the probability of inspection for non-focus-sector firms. Importantly, because all focus-sector firms are subject to an increased likelihood of FRRP inspection, treatment is not limited to firms reviewed ex-post.

The FRRP releases an annual public report with summaries of the previous year's reviews, giving some insight into the results of the FRRP's enforcement activity. Using these disclosures, we estimate the increase in the probability of a future review and the likelihood that companies agree to changes in their financial reporting for 2005 and 2006—the only years the FRRP publicly disclosed the exact number of focus-sector reviews.[11] In Table 1 Panel B, we present an estimate of the change in enforcement intensity by comparing the probability of a review for focus-sector and non-focus-sector firms (conditional on knowing which sectors the FRRP intends to target) to the probability of a review *not* distinguishing between focus-sector and non-focus-sector firms.

[9] In 2012, the FRRP was replaced by the Monitoring Committee as part of a change in the overall structure of the FRC. Under the Monitoring Committee, the practice of focus sectors still exists, although the Monitoring Committee does not announce them in press releases. For this reason, we limit our analyses to the eight announcements by the FRRP from 2004 to 2011.

[10] In these press releases, the FRRP also mentions focus areas that are not sector-specific. However, these additional areas of focus are difficult to attribute to specific firms. For example, in 2011, the FRRP announced that they would focus on "risk disclosures." In 2010, they announced a focus on "environmental matters."

[11] Based on other indicators of the FRRP's level of proactive review activity (see Table 1 Panel A), we think it is reasonable to generalize the level of 2005 and 2006 focus-sector enforcement intensity to other years in the sample. However, we acknowledge that the actual increase in enforcement intensity could be different from our estimates.

Based on this approach and under the assumption that there is no anticipation of the selected sectors, the estimated change in the probability of a review after the announcement is 251.6% for focus-sector firms and -34.0% for non-focus-sector firms; the estimated change is 432.4% for focus-sector relative to non-focus-sector firms. Of course, it is highly unlikely that there is no anticipation of the selected focus sectors. Thus, this estimate represents an upper bound on the news conveyed by the focus-sector announcements regarding the expected change in enforcement intensity. Table 1 Panel B also shows that within the sample of reviewed firms, the likelihood that the FRRP detects a material reporting weakness is lower if the company is in a focus sector (15%) than if it is selected as part of the FRRP's general review program (29%), illustrating the challenges that regulators face in identifying potential reporting issues for an entire sector.

Table 2 reports the announcement dates, sector names, and the total number of sample firms in focus and non-focus sectors each year.[12] The FRRP selects some sectors in multiple years—the most frequent is retail (6 years)—while it selects other sectors only once (telecommunications, advertisement, recruitment, information technology, and insurance). Over the sample period, there are 16 unique focus sectors and 1,551 focus-sector firm-year observations. Even a casual look at Table 2 reveals that the selection of focus sectors is not random and suggests that some of the selected sectors could likely be anticipated (e.g., banks were selected in 2007 and 2008 at the onset of the financial crisis).[13] However, as long as the market cannot perfectly predict which sectors the FRRP will select, the announcements will still convey some new information.

To provide descriptive evidence on how the FRRP selects focus sectors, we estimate a regression model using observable characteristics (measured before the selection announcements) to predict which sectors are targeted for focus. We estimate the following linear probability model

[12] We match the focus sectors in the FRRP press releases to the most similar ICB industry codes. The focus sectors vary in the specificity of the industry selected (e.g., retail versus house builders). Thus, a single focus sector can include one or multiple four-digit ICB industry codes. In Table IA1 of the Internet Appendix, we provide a detailed breakdown of the ICB codes we include in each focus sector.

[13] To avoid contaminating effects from the financial crisis, we exclude banks from the analysis. However, in Section IA2 of the Internet Appendix, we show that our results are not sensitive to this restriction.

at the four-digit ICB-industry-code (i.e., sector) level:

$$Focus\ Sector_{j,t} = \sum \beta_i Predictors_{j,t} + \sum \alpha_i Fixed\ Effects + \varepsilon_{j,t} \quad (1)$$

$Focus\ Sector_{j,t}$ is an indicator variable equal to one if industry *j* is selected as a focus sector in year *t*, and zero otherwise. We choose three predictors (measured at the industry level over the year before the focus-sector announcements, *t-1* to *t*) that are likely associated with the perception of industry financial-reporting risk: stock performance, industry visibility, and the existence of publicly observable accounting issues. We include stock returns (*Prior Return*) to capture performance; the number of negative media mentions about the industry or about a firm in that industry (*Negative Media Mentions*) to capture industry visibility; and the proportion of firms in the industry with an accounting restatement (*% Industry Restatements*) to capture accounting issues. We provide further details on the construction of these variables in Appendix B. We include industry fixed effects so that our estimates reflect deviations from industry averages and cluster standard errors by industry.

We report descriptive statistics in Table 3 Panel A. The median sector has a return of 2.2% in the prior year, is mentioned negatively in the press 7.0 times, and 30.0% of the firms issue a restatement. We report regression results for Eq. (1) in Table 3 Panel B. Consistent with the FRRP selecting sectors with deteriorating stock-market performance, a high level of (negative) media attention, and publicly observable accounting issues, we find that *Prior Return*, *Negative Media Mentions*, and *% Industry Restatements* are all significant determinants of focus-sector selection at the 10% level (at least). The fact that the focus sectors are determined (in part) by these publicly observable factors suggests that regulators allocate their resources to firms where other market participants are also likely aware of potential reporting deficiencies. Moreover, the fact that the FRRP systematically selects industries that are performing poorly suggests that it is important to industry adjust returns when analyzing the stock-price response to the focus-sector announcements. Together, these observable characteristics (along with industry fixed effects) explain around 25% of the variation in the selected sectors. While the remaining variation is

consistent with a significant element of randomness in the selection process, it also raises concerns about selection on unobservables (e.g., the FRRP's private information). As discussed in Section 4, we alleviate this concern in various ways in our empirical analyses.

3.4 The Effect of Increased PFRE Intensity on Financial Reporting

In this section, we compare the FRRP focus-sector review program (as a shock to financial reporting enforcement) to settings used in prior studies by reproducing their results in our setting. Specifically, we examine the effect of PFRE on three firm-level financial reporting outcomes: 1) financial statement length; 2) audit fees; and 3) stock-market liquidity.

First, to provide direct evidence on the impact of the FRRP reviews on firm disclosure, in Section IA4 of the Internet Appendix, we examine changes in the length of focus-sector firms' financial statements (Guay et al. 2016, Dyer et al. 2017). Consistent with firms making substantial disclosure changes in response to increased PFRE intensity, we find that the length of focus-sector firms' financial statements increases by a statistically significant 580 words relative to non-focus sector firms in the fiscal year following the focus-sector announcement—an approximately 3.8% increase in length.

Second, we examine the impact of increased enforcement intensity on regulatory compliance costs. Greater enforcement likely creates an incentive to increase expenditures on reporting systems in order to ensure compliance with accounting regulations. In Section IA5 of the Internet Appendix, we use audit fees (which are observable and likely positively associated with the quantity and quality of a wide range of firm-disclosure-related activities) to proxy for the increase in compliance costs connected to PFRE (Iliev 2010). Similar to Florou et al. (2019), the results of this analysis indicate that relative to firms not inspected by the FRRP, focus-sector firms experience a sustained 6.7% increase in audit fees (approximately $80K annually).

Finally, to compare the effect of PFRE to studies that document an increase in transparency following stronger regulatory enforcement (see Leuz and Wysocki 2016 for a review), in Section IA6 of the Internet Appendix, we examine the effect of increased PFRE intensity on stock-market

liquidity. Using a within-industry, difference-in-differences design that identifies the change in liquidity based on variation in firms' fiscal year-ends (Daske et al. 2008), we find that focus-sector firms experience an approximately 5% increase in stock-market liquidity.

Overall, these results suggest that the FRRP's increase in enforcement intensity leads to a significant change in focus-sector firms' financial reporting. However, although PFRE appears to have financial-reporting benefits similar to those documented in prior studies, increases in disclosure also likely have costs. An advantage of our setting is that the abrupt changes in enforcement intensity for focus-sector relative to non-focus-sector firms allows us to examine the effect of PFRE on equity values. We turn to this analysis next.

4. Increased PFRE Intensity and Equity Value

4.1. Research Design and Identification Strategy

To assess the effect of increased PFRE intensity on firms' equity values, we examine short-window returns (i.e., over five days centered on the announcement date) around the FRRP's eight annual focus-sector announcements (from 2004 to 2011). We rely on the abrupt change in enforcement intensity around the announcement dates for focus-sector relative to non-focus-sector firms—and the assumption that investors have rational expectations and correctly anticipate the equity-value implications of increased regulatory scrutiny—to identify the impact of increased PFRE intensity.

We obtain stock-return data from 2004 to 2012 from *Refinitiv's Datastream* database. Non-focus-sector firms provide a natural benchmark to control for any contemporaneous, UK economy-wide return news. However, because the focus-sector reviews affect all UK firms in a particular industry, no UK control group can address the potential confounding effect of contemporaneous, industry-specific news, which is particularly important given that the FRRP chooses sectors based on factors correlated with past industry-level performance (Section 3.3). Therefore, we industry adjust returns by subtracting the return of US (or non-UK-European) firms within the same four-

digit ICB industry.[14] To estimate the market reaction to the focus-sector announcements, we use a pooled OLS panel regression that compares daily, firm-level returns for focus-sector and non-focus-sector firms:

$$\begin{aligned} Abnormal\ Return_{i,t} = \alpha_0 + \alpha_1 20\ Days\ Before\ Announcement_{i,t} + \alpha_2 Announcement_{i,t} \\ + \alpha_3 20\ Days\ After\ Announcement_{i,t} + \sum \beta_i Fixed\ Effects + \varepsilon_{i,t} \end{aligned} \quad (2)$$

$Abnormal\ Return_{i,t}$ is the daily industry-adjusted abnormal return for firm i on day t in basis points. $Announcement_{i,t}$, the main variable of interest, is an indicator equal to one if day t is in the event window (i.e., the five-day period from t-2 to t+2 where t is the focus-sector announcement date) and firm i is in an announced focus sector, and zero otherwise. *20 Days Before (After)* $Announcement_{i,t}$ is an indicator equal to one for focus-sector firms in the 20 days before (after) the announcement window and zero otherwise, and is included to indicate the sharpness of the treatment effect. We include fixed effects for each day to control for contemporaneous market-wide return news and cluster standard errors by day to account for the cross-sectional correlation in daily returns. To limit the influence of extreme observations and very small firms, we drop observations with daily returns greater than 100% and firms with a market capitalization under $10 million.[15]

4.2. Stock Market Responses to the Announcement of the Focus Sectors

Table 4 Panel A reports results based on estimations of Eq. (2); Column (1) reports results for our main specification. The PFRE *Announcement* coefficient is -26.98 basis points (p-value<0.01). Adding the daily returns over the event window, this amounts to an average reduction of 1.3% in market capitalization for focus-sector firms. The coefficient on *20 Days Before (After) Announcement* is statistically insignificant and relatively small in magnitude, indicating that the

[14] One potential issue with using foreign returns as a benchmark is that foreign firms might benefit from increased PFRE in the UK (e.g., by profitably exploiting the additional disclosures made by focus-sector firms). If this is the case, our US and non-UK-European adjusted returns will overstate the effect of PFRE on the targeted firms. However, we expect that the impact of any such effect is likely to be small because the number of foreign firms far exceeds the number of UK firms (i.e., the positive effect on the equity prices of foreign firms should be small).

[15] In Panel A of Table IA2 of the Internet Appendix, we report all sample-selection criteria.

observed effect occurs sharply around the focus-sector announcement dates.

We also report results from four sensitivity tests in Columns (2)-(5). Column (2) presents results using unadjusted returns (i.e., without subtracting the US industry return, but still relative to non-focus-sector UK firms). The estimated *Announcement* coefficient is -16.08 basis points (p-value<0.05). In Column (3), we present results using US (instead of UK) firms as the benchmark. The estimated *Announcement* coefficient is -39.66 (p-value<0.10). Column (4) reports results using a European sample of firms to industry-adjust returns.[16] The estimated *Announcement* coefficient is -18.45 basis points (p-value<0.05). Column (5) provides results using the Fama-MacBeth (1973) approach and the same return adjustment as Column (1). The estimated *Announcement* coefficient is -23.09 basis points (p-value<0.01).

Although inferences are similar across all five specifications, results based on US-industry-adjusted returns are our preferred specification as this is the only benchmark that appears to adequately control for (negative) performance trends (i.e., the coefficient on the pre- and post-announcement period returns are statistically insignificant). In the UK-benchmark-only specification (i.e., Column 2), for example, the *20 Days Before Announcement* coefficient is negative and significant and the *20 Days After Announcement* coefficient is negative. However, this is not surprising given that the FRRP selects focus sectors (in part) based on prior performance (see Table 3).[17]

[16] The European benchmark is based on an equal-weighted average of daily returns on the primary exchange in the following countries: Belgium, France, Germany, Italy, Luxembourg, Netherlands, Denmark, Ireland, Greece, Portugal, Spain, Austria, Finland, and Sweden.

[17] In Table IA2 Panel B of the Internet Appendix, we report the results of several additional sensitivity tests for our main specification in which we successively exclude each of our discretionary sample-selection criteria and impose a more stringent cutoff on extreme return values. None of these research design choices materially affect our inferences. We also conduct several untabulated robustness tests. First, we examine median focus-sector returns. Although the large number of day fixed effects in Eq. (2) limits our ability to assess quantile differences between treatment and control groups, using a Wilcoxon rank-sum test and a nonparametric test of the equality of the distributions of the focus-sector-announcement-day returns, we can reject the null that the medians and distributions of the returns are equal for focus-sector and non-focus-sector firms. Second, we compare announcement returns from the first time a firm's industry is selected as a focus sector with the returns for subsequent selections and find that the return magnitudes are similar. This suggests that firms make additional reporting changes each time their industry is included in a focus sector and that these additional disclosure changes have incremental costs. However, it is worth noting that the majority (over 70%) of focus-sector firms in our sample are selected two times or fewer. Finally, we confirm that our results are similar when excluding the retail sector, which was selected as a focus sector six times.

Next, to assess the consistency of the pooled results, we separately examine each of the eight annual FRRP focus-sector announcements. Table 4 Panel B presents results with separate indicators for each of the eight announcements using our main specification (Column 1 of Table 4 Panel A). The focus-sector announcement returns in 2004 and 2005 are small in magnitude (-4.26 and 4.01 basis points, respectively) and statistically insignificant. From 2006 to 2011, the returns range in magnitude from -20.17 (2008) to -45.77 (2007) and are statistically significant (at the 5% level, at least) in five of the six years, suggesting that PFRE reduces equity values regardless of the prevailing macroeconomic conditions at the time of increased enforcement intensity. The apparent two-year lag in the effect of PFRE is consistent with an increase in the resources expended by the FRRP for focus-sector reviews after 2005 (as discussed in Section 3.2) as well as the possibility that the market takes time to understand and react to the impact of the focus-sector announcements.

To provide a more nuanced assessment of the economic significance of the observed decrease in equity values, in Table 5, we estimate separate regressions based on quintiles of firm market capitalization as of the first trading day of the announcement year. From the smallest quintile of firms (with an average market capitalization of about $15 million) in Column (1) to the largest quintile of firms (with a market capitalization of about $3.4 billion) in Column (5), the focus-sector announcement return increases from -47.15 basis points (-2.4% over five days) to (a statistically insignificant) -11.29 basis points (-0.56% over five days). This translates into a decrease in equity value of approximately $350,000 for the smallest firms and $19 million for the largest firms (although the decrease for the largest firms is not statistically significant). The weighted (by the number of firms) average decline in market value across all quintiles is about $4.8 million. By comparison, in Section IA5 of the Internet Appendix, we find that PFRE leads to an average increase of about $80,000 per year in audit fees—or, if we assume a permanent increase and a 10% cost of capital, $800,000 in net present value terms. While these costs are a nontrivial proportion of the total decline in equity values (and are likely correlated with other compliance

costs), an increase in compliance costs is likely not enough to explain the total reduction. We investigate other potential costs of increased PFRE intensity in Section 5.

Overall, the results in this section provide evidence of a robust, economically significant, negative market reaction to increased PFRE intensity, suggesting that for shareholders of UK firms selected for greater regulatory scrutiny, the costs of additional enforcement outweigh the benefits.

4.3 Assessing the Potential Revelation of Private Information by the FRRP

Attributing the observed reduction in equity values to the costs of enforcement is complicated by the possibility that the negative market reactions could be attributable to the revelation of value-relevant private information by the FRRP. Importantly, the fact that the FRRP selects focus sectors based on risk is not an issue for our analysis as long as these risk factors are publicly observable to investors; if markets are efficient, equity prices should incorporate these factors before the focus-sector announcements. Instead, the concern is that the FRRP selects sectors based on factors unobservable to investors (i.e., private information).

Based on the institutional details discussed in Section 3, this explanation seems unlikely because 1) the accounting policies examined by the FRRP are publicly observable, 2) the FRRP generally recommends prospective changes to accounting policies (i.e., they are not uncovering fraud or past misconduct), and 3) the FRRP announcements are for entire industries as opposed to specific firms. It is unlikely that the FRRP would know more about industry-wide (value-relevant) accounting-policy deficiencies than would sophisticated market participants (particularly those in the UK, which is one of the most transparent and informationally efficient capital markets in the world).[18] Consistent with this argument, the FRRP is less likely to detect a material financial reporting weakness in a company selected as a part of a focus-sector review than for firms selected as part of the risk-based proactive review process (see Table 1). Nevertheless, to provide empirical

[18] A conversation with a former member of the FRRP focus-sector selection panel also supports this conjecture. Specifically, the panel member noted that while the selection is not completely random (i.e., there is some consideration of sectors where it is public knowledge that there might be an accounting issue), there is "some arbitrariness in the choice for at least some of the sectors" and little indication that the selection group knows "anything significant the market didn't already know."

support for this conjecture, we perform several additional analyses to assess whether the focus-sector announcements reveal private information.

First, we investigate cross-sectional heterogeneity in focus-sector announcement returns in settings where the FRRP is most likely to have private information. One potential source for such private information is other regulators.[19] If other regulators obtain private information from their investigations and share this knowledge with the FRRP, the selection of focus sectors could convey information to the market or could signal to investors that those industries will be under scrutiny from other regulatory bodies in the future.

To investigate this possibility, we partition our sample in two ways. First, we compare focus-sector industries that are highly regulated (i.e., financial services, utilities, telecommunications, insurance) to those that are less heavily regulated (i.e., all other industries). Second, we compare firms on the LSE's two core segments: the Main Market and AIM. The Main Market is an EU-regulated market, which means that it is subject to all EU securities regulation. AIM, on the other hand, is a less-regulated exchange exempt from many EU regulatory provisions (Gerakos et al. 2013). It would likely be easier for the FRRP to obtain private information from other regulators about firms on the Main Market, given its higher degree of ongoing scrutiny. We present results in Table 6 Columns (1)-(4). Inconsistent with the negative announcement returns being attributable to the revelation of private information obtained from other regulatory bodies, we find that returns are *less* negative for more regulated industries and firms.[20] The difference is statistically significant in the Main Market versus AIM partition (p-value 0.02) but falls short of conventional levels of significance in the most- versus least-regulated industries partition (p-value

[19] Our analyses of the FRRP's annual reports indicate that the FRRP receives referrals from other regulators but does not disclose the identities of those regulators. The only exception is the 2009 activity report, where the FRRP states that "the Panel has worked closely with the FSA extracting data and information from the accounts of UK banks and insurers." For this reason, we base our tests on several well-known high-regulation industries.

[20] Firms listed on the Main Market are, on average, much larger than firms listed on AIM. In Internet Appendix Section IA9, we examine whether firm size differences explain these results, and conclude that they do not.

0.198).[21]

A concern with the regulatory splits is that regulated and unregulated firms and industries vary along other dimensions besides the likelihood of the FRRP obtaining private information. For instance, regulated firms likely have more transparent financial reporting with less scope for obtaining value-relevant private information. To address this issue, we examine cross-sectional variation within the focus sectors in announcement returns based on the presence of a strong private external monitor. Strong private oversight limits the FRRP's scope to obtain private information and makes it more likely that firms are already close to their equity-value-maximizing level of transparency.

Our proxy for the presence of a strong external monitor is the existence of an outside blockholder (i.e., a shareholder with ownership greater than 5% of shares outstanding). Blockholders' large ownership positions give them a strong incentive to monitor management (e.g., Shleifer and Vishny 1997, Edmans 2009, Edmans and Manso 2011), and in settings where large blockholders are present, additional enforcement efforts are more likely to be redundant (Dechow et al. 1996). We expect more negative focus-sector announcement returns for firms with large independent blockholders if increased enforcement (rather than FRRP private information) drives the observed market reactions.

We obtain data on share ownership from the *Argus Vickers Owners Service Share Register Analysis System* (*AVSR*) and define the indicator *Blockholder* as equal to one if an investor owns 5% or more of shares outstanding, and zero otherwise. In Table 6 Columns (5) and (6), we present *Announcement* returns separately for focus-sector firms with *Blockholder(s)* and with *No Blockholders*. Consistent with negative focus-sector returns being driven by the costs of increased enforcement rather than by the revelation of private information, we observe a negative market reaction to the focus-sector announcements only for firms with at least one blockholder (-28.74

[21] An alternative (or additional) explanation for AIM firms' more negative focus-sector announcement returns is that firms self-selecting into a less regulated exchange segment experience higher costs of additional regulatory scrutiny.

versus 0.42, difference p-value=0.01).

Next, we examine changes in information asymmetry following the focus-sector announcements. In the presence of traders with private information about reporting deficiencies, market makers will address the adverse selection problem it creates by increasing bid-ask spreads. Thus, a public announcement that reveals private information should reduce information asymmetry and lead to a decline in bid-ask spreads (e.g., Diamond and Verrecchia 1991). Analyzing spreads around management earnings forecasts, Coller and Yohn (1997) find evidence consistent with this prediction: after a management forecast, forecasting firms experience a reduction in information asymmetry relative to non-forecasting firms. If the FRRP focus-sector announcements reveal some information regarding disclosure quality previously known only by relatively informed traders, this should lead to a decline in information asymmetry and a reduction in bid-ask spreads.[22]

To investigate this possibility, we examine changes in bid-ask spreads around the FRRP announcements for focus-sector relative to non-focus-sector firms. To increase the likelihood that some investors have private information about reporting quality, we limit our sample to firms with external blockholders (although our results are similar if we include all firms). Section IA7 of the Internet Appendix presents the details of this analysis. Inconsistent with the revelation of private information by the FRRP, we find no evidence of significant changes in bid-ask spreads around the focus-sector announcements.

Finally, we examine the effect of focus-sector selection on audit-committee turnover. If the focus-sector announcements convey news about the quality of financial reporting to investors, this should have labor market consequences for those overseeing firms' financial reporting. The high cost of changing audit committee members reduces the power of this test, so to provide a

[22] Theoretically, it is also possible that if some traders have a superior ability to process public information, it could lead to a temporary information advantage. In such a case, bid-ask spreads could increase around the focus-sector announcements in the short term (Kim and Verrecchia 1994). To abstract from any such effects, we examine spreads over a window of 20 days before and after the focus-sector announcements, a period long enough that any such effects would likely have subsided.

benchmark, we compare the estimated effect of PFRE to that of a restatement; this allows us to assess the magnitude of the information conveyed by the FRRP's focus-sector announcements relative to the information conveyed by a restatement.

Section IA8 of the Internet Appendix presents the details of this analysis. We find no evidence that focus-sector firms have statistically significantly higher audit committee turnover than do non-focus-sector firms. However, an accounting restatement increases the likelihood of turnover for an audit-committee member by a statistically significant 4.5%. Thus, we can conclude that the focus-sector announcements have less impact on audit committee turnover and likely convey less information about financial reporting quality (i.e., the type of information that the FRRP could obtain privately) than do restatements.

Overall, although no single analysis in this section definitively rules out the possibility that the FRRP announcements convey private information about industry-wide accounting deficiencies, the weight of the evidence across all these analyses suggests that such an explanation is unlikely to play a major role in explaining our results.

5. Increased PFRE Intensity and Real Effects

The results in Section 3.4 suggest that focus-sector firms increase disclosure and have greater transparency. However, in Section 4, we find that for equity holders, these benefits do not appear to offset the costs. Internet Appendix Section IA5 provides evidence that audit fees, one of the potential costs likely to be positively associated with other direct compliance costs, increase by 6.7% for firms in sectors selected by the FRRP. However, our returns analysis suggests that on a value-weighted basis, the increased likelihood of an FRRP-initiated financial-statement review leads to a decline in equity values of approximately $4.8 million. An increase in direct compliance costs is unlikely to fully explain this decline. In addition to its direct costs, greater transparency can also lead to changes in competitor and reporting-firm behavior. In this section, we examine two potential value-decreasing real effects: proprietary disclosure costs (i.e., increased competition) and changes in managers' investment horizons.

While the theoretical relationship between transparency and the proprietary costs of disclosure (e.g., Verrecchia 1983) and managerial myopia (e.g., Stein 1989) is well-established (see Section 2), empirically detecting these effects in our setting is challenging. First, neither construct can be measured directly and our proxies are noisy. Second, the empirical manifestations of these effects are likely to be subtle and of uncertain timing and duration. Thus, the findings in this section should be interpreted with these limitations in mind.

5.1 Proprietary Costs of Increased Disclosure

In Section IA4 of the Internet Appendix, we find that focus-sector firms increase the length of their financial statements by about 3.8%. In addition to preparation and auditing costs, this increase in disclosure could have proprietary costs if the firm's competitors are able to profitably exploit this information (e.g., Feltham et al. 1992, Hayes and Lundholm 1996).[23] Although the relationship between proprietary costs and voluntary disclosure is complicated and likely to be setting specific (e.g., Verrecchia 1990, Wagenhofer 1990), the proprietary costs of mandatory increases in disclosure, such as those arising from increased enforcement by the FRRP, are less ambiguous. That is, if the increase in enforcement leads firms to disclose useful information (not available in the prior disclosure equilibrium) about the product market in the focus sector, we expect the level of competition within the targeted sector to increase. The increase in competition could come from both targeted and non-targeted firms (e.g., foreign firms, small private UK firms, and/or other potential market entrants) who can exploit the additional information.

Prior research indicates that accounting rates of return mean-revert faster in industries with greater competition (e.g., Lev 1983, Cheng 2005, Li et al. 2013). We use this insight to investigate whether greater competition is a potential (partial) explanation for the observed reduction in equity

[23] Many of the FRRP's specific recommendations for remediation (see Appendix A) suggest changes to disclosures that could plausibly provide useful information to competitors. For example, in nearly every year, the FRRP focus-sector-review reports indicate that disclosures related to consolidations and acquisitions are inadequate, particularly regarding information about unconsolidated entities. In later years, the FRRP notes many issues regarding insufficient segment and related-party disclosures. It is this type of information, which cannot be readily obtained from other sources, that is likely to have the greatest proprietary costs (Berger and Hann 2007).

values by examining whether firms with abnormally high profits before the focus-sector announcements have more negative announcement returns and whether profitability mean reverts faster for firms in focus-sector industries.

To analyze cross-sectional variation in focus-sector announcement returns, we define abnormally high-profit firms as those with a return on assets above the 75th percentile of industry ROA in a particular year.[24] Table 7 Panel B reports the results. Consistent with PFRE imposing the greatest costs on firms where increased competition would likely have the greatest impact on profitability, we find that the focus-sector announcement returns are -37.90 basis points for *High ROA* firms versus -18.27 for *Low ROA* firms. The difference of -19.63 between the two groups is statistically significant at the 5% level.

Next, in order to examine changes in the rate of mean reversion in profitability, we follow a two-step approach in which we first estimate the ROA mean-reversion rate for each industry and year and then examine how this rate changes after the focus-sector-selection and review period.[25] Specifically, we calculate a mean-reversion coefficient for each four-digit ICB industry code and year by estimating the following OLS regression:[26]

$$\Delta ROA_{i,t\ to\ t+1} = \beta_1 ROA_{i,t} + \beta_2 \%\Delta TA_{i,t-1\ to\ t} + \varepsilon_{i,t+1} \quad (3)$$

$\Delta ROA_{i,t\ to\ t+1}$ is the change in *ROA* for firm *i* from year *t* to *t+1* and *ROA* is operating income after depreciation divided by average total assets. $\%\Delta TA_{i,t-1\ to\ t}$ is the percentage change in total assets for firm *i* from year *t-1* to *t*. We include $\%\Delta TA$ to control for changes in the rate of return on new

[24] We also consider alternative percentile cutoffs for abnormal ROA (i.e., 50th, 60th, 70th, and 80th). In all cases, returns are larger for firms with higher abnormal ROA. However, the difference between the two groups is statistically significant only for the 70th and 80th percentiles.

[25] We measure ROA mean reversion at the industry-year level using a two-stage approach so that we can 1) control for industry and year fixed effects in the ROA-mean-reversion rate in the second-stage regression and 2) truncate extreme values of the estimated mean reversion coefficients.

[26] For this analysis, we impose the following sample-selection criteria: 1) we exclude firms with negative operating income (e.g., Fairfield and Yohn 2001, Li et al. 2013); 2) we require total assets greater than $0.5 million; 3) we exclude firms with ROA greater than 100% (Li et al. 2013). We also require at least 15 firms in a given industry and year in order to estimate Eq. (3).

investments. Prior research shows that the marginal return to new investment decreases as competition increases (e.g., Fairfield et al. 2003, Li et al. 2013). Because we do not have a specific prediction about how PFRE affects returns to new investment, we focus on the profitability of existing assets. β_1 captures the rate of ROA mean reversion (*Mean Reversion of ROA*) for each industry and year and is predicted to be negative.

We present descriptive statistics for (firm-level) *ROA*, *TA*, and the resulting (industry-year-level) coefficients in Table 7 Panel A. The median firm has a return on assets of 12.4% and total assets of $129 million. The median industry-year in our sample has a *Mean Reversion of ROA* coefficient of -0.190, which is consistent with prior studies (e.g., Li et al. 2013).

We use the estimates of *Mean Reversion of ROA* to assess the impact of increased PFRE intensity on competition:

$$Mean\ Reversion\ of\ ROA_{j,t+1} = \beta_1 PFRE_{j,t} + \sum \delta_i Fixed\ Effects + \varepsilon_{j,t+1} \quad (4)$$

Mean Reversion of ROA is the estimated β_1 coefficient from Eq. (3) for industry *j* in year *t+1*. *PFRE* equals one if an industry is included in an FRRP focus sector in year *t*, and zero otherwise. Because competitors cannot observe any disclosure changes precipitated by the focus-sector reviews until the inspection-year financial statements are publicly released, we examine the effect of period *t=0*'s focus-sector announcement on period *t+1*'s *Mean Reversion of ROA*. We weight the regression by the number of firms in a given industry and year because the precision of the estimated β_1 coefficients likely varies based on the number of firms used in estimating Eq. (3) (Lewis and Linzer 2005). We include industry fixed effects to control for static differences in the mean reversion in profitability across industries and include year fixed effects to flexibly account for changes in mean reversion over time. We truncate *Mean Reversion of ROA* at the 1% level and estimate heteroscedasticity-consistent standard errors clustered by industry.[27]

[27] Weighting by the number of firms and using heteroscedasticity-consistent standard errors helps to address the potential inconsistency in standard errors arising from the fact that Eq. (4) uses an estimated dependent variable (Lewis

Because it is not clear exactly how long it would take for any effect of increased disclosure on competition to manifest (or how long the effect would last), we estimate an additional regression model that includes PFRE indicators for the three periods prior (i.e., $t \leq -4$ to $t-2$) and subsequent ($t=0$ to $t \geq 3$) to the focus-sector announcements; the indicator for year $t-1$ is omitted and serves as the benchmark against which all other periods are compared.

Table 7 Panel C presents regression results for Eq. (4) and Figure 1 plots the coefficient estimates in event time. In Column (1), the *PFRE* coefficient estimate (-0.09) is negative and statistically significant at the 5% level. The coefficient magnitude indicates that ROA mean reverts about 45% faster (relative to the sample mean *Mean Reversion of ROA*) the year after an industry is selected as a focus sector. The results in Column (2) and Figure 1 indicate that this effect is short lived, as the change in mean reversion is statistically indistinguishable from zero in the next three periods. The PFRE coefficients in the years before focus-sector selection are small in economic magnitude and statistically insignificant.

Overall, the evidence presented in this section suggests that the firms with the largest abnormal profits experience the greatest reductions in equity values around the announcement of the focus-sectors and that profitability in focus-sector industries mean reverts faster after the year of inspection. Taken together, these results suggest that proprietary costs of increased disclosure (in the form of an increase in competition) are one potential explanation for PFRE's overall negative impact on equity values. However, because the proxy we use for increased competition is noisy, we urge caution in interpreting these results.

5.2 Changes in Managers' Investment Horizons

A focus on short-term profits at the expense of long-run value (i.e., managerial myopia) is another potential cost of PFRE. Faced with fewer options for meeting reporting objectives and greater capital market scrutiny following an increase in disclosure, managers might substitute

and Linzer 2005). In additional (untabulated) analyses, we confirm that results are robust to clustering by industry and year.

(more profitable) long-term investment activities for projects that have relatively short-term cash flows in order to boost current period reported earnings (Stein 1989). Although conceptually appealing, empirically identifying myopia is a difficult task that requires an assessment of investment efficiency. Instead, we examine changes in managers' investment horizons. That is, while we cannot measure investment efficiency, we can use the benefit of hindsight (i.e., data not available to investors at the time investments are made) to assess the timing of investment payoffs by examining changes in the association between investment and future cash flows. Coupled with our other evidence, a switch to a shorter investment horizon would be consistent with myopic investment by managers.

To assess changes in investment horizon after the focus-sector-selection and review period, we follow a two-stage approach similar to the one in Section 5.1 and calculate the association between current period capital expenditures and cash flows over the next two years for each four-digit ICB industry code and year:[28]

$$CAPEX_{i,t} = \alpha + \beta_1 CFO_{i,t+1} + \beta_2 CFO_{i,t+2} + \varepsilon_{i,t} \quad (5)$$

CAPEX is capital expenditures for firm *i* in year *t*.[29] *CFO* is operating cash flow for firm *i* measured one and two years after the fiscal year where we measure *CAPEX* (i.e., *t+1* and *t+2*).[30] β_1 and β_2 capture the association between current capital expenditures and future cash flows and are expected to be positive. We compare β_1 and β_2 before and after an industry is included in an

[28] We impose the same sample restrictions as in Section 5.1.

[29] We look at *changes* in the association between investment and future cash flows (i.e., managers' investment horizons) rather than the *level* of investment because the level of investment is immediately publicly observable, which means that investors could detect any suboptimal changes in investment levels. We examine capital expenditures and not R&D investment because future cash flows from current R&D expenditures are typically realized over a relatively long horizon (compared to CAPEX) and changes in managers' investment horizons would be difficult to detect using our empirical approach. Recent papers (e.g., Glaeser et al. 2019) have used alternative proxies for investment horizon (e.g., incentives around seasoned equity offerings) that are difficult to operationalize in our setting.

[30] Most capital expenditures affect cash flows for more than two years. However, even though it does not provide a complete picture of the change in investment policy, comparing changes in the following one- and two-year coefficients should indicate any shift in the horizon of the payouts from future cash flows. We choose to look only one and two periods ahead for simplicity and expect any noise in this approach to bias the coefficient estimates downwards. If the measurement error is constant over our sample period, it will be differenced out by our changes design.

FRRP focus sector to assess changes in investment horizon.

In Table 8 Panel A, we report descriptive statistics for the firm-level variables included in Eq. (5) as well as for the industry-year-level estimates of β_1 and β_2, which we refer to as *Investment Horizon Yr. 1* and *Investment Horizon Yr. 2*, respectively. The median firm has *CAPEX* of $1.1 million per year and CFO of $2.3 million. The median *Investment Horizon Yr. 1* (*Investment Horizon Yr. 2*) coefficient is 0.123 (0.087) and indicates that each dollar of investment is associated with 12 cents in cash flow during the following year (and 9 cents in the year after that).

We use an approach very similar to the one in Section 5.1 and assess the impact of increased PFRE intensity on managers' investment horizons using estimates of *Investment Horizon Yr. 1* and *Investment Horizon Yr. 2* (separately) as dependent variables in the following regression:

$$Investment\ Horizon\ Yr.1(2)_{j,t} = \beta_1 PFRE_t + \sum \delta_i Fixed\ Effects + \varepsilon_{j,t} \quad (6)$$

Investment Horizon is either the estimated β_1 or β_2 coefficient from Eq. (5) for industry *j* in year *t*. *PFRE* is an indicator equal to one for the fiscal year after a firm's inclusion in a focus sector, and zero otherwise.[31] We include industry and year fixed effects and weight the regression by the number of firms in a given industry-year. We truncate *Investment Horizon Yr. 1* and *Investment Horizon Yr. 2* at the 1% level and estimate heteroscedasticity-consistent standard errors clustered by industry. As in the proprietary cost analysis, we also estimate an additional specification in which we present the treatment effects in event time.

Table 8 Panel B presents regression results for Eq. (6) and Figure 2 plots the coefficient estimates in event time. In Column (1), the *PFRE* coefficient estimate (0.15) is positive, statistically significant (at the 10% level), and indicates an *increase* in the association between *CAPEX* and *CFO* in period *t+1*. In Column (3), the *PFRE* coefficient estimate (-0.14) is negative,

[31] We only examine the fiscal year after a firm's inclusion in a focus sector because if investments in year *t* produce lower cash flows in *t+2*, then in the year following, one-year-ahead cash flows are necessarily lower.

statistically significant (at the 10% level), and indicates a *decrease* in the association between *CAPEX* and *CFO* in period *t+2*. Taken together, these results are consistent with managers shifting away from longer-term and toward shorter-term investments following an increase in PFRE intensity. The results in Column (2), Column (4), and Figure 2 indicate that in the post-period, this effect is significant only in the year after the focus-sector selection. The PFRE coefficients in the years before focus-sector selection are statistically insignificant (except for the ≤4 indicator in *Investment Horizon Yr. 1*).

Overall, the evidence presented in this section suggests that following an increase in PFRE intensity (accompanied by a corresponding increase in financial reporting transparency), firms in our sample have shorter-term investment horizons. When viewed in isolation, this result does not necessarily imply a decrease in investment efficiency (i.e., we provide no direct evidence that the change in investment policy is costly to shareholders), but coupled with the negative market reactions to the focus-sector announcements, the observed change in investment behavior likely represents a deviation from the policy that was optimal before PFRE.

6. Conclusion

Despite the rapid increase in proactive financial-reporting enforcement over the past two decades, we have almost no direct evidence on its net effect on equity values (absent simultaneous changes in accounting standards or other securities regulations). Because increases in enforcement are often motivated by a desire to protect shareholders, isolating the effect on equity values is important. Although prior research documents many potential benefits of financial reporting enforcement, it is not clear whether the benefits of requiring regulators to take a more *proactive* role in identifying potential reporting deficiencies outweigh the costs.

We find that an over fourfold higher likelihood of being selected for regulator-initiated reviews of financial reports reduce equity values by 1.3%, on average. Reductions in equity values are largest for 1) the least regulated firms and industries, where it is less likely that the FRRP could obtain private information from other regulators, and 2) where, because of a high level of private

oversight, firms are likely already closer to their value-maximizing level of transparency before the increase in PFRE. We present evidence that is consistent with increased compliance costs, higher proprietary costs, and a shorter-term investment focus contributing to the observed decline in equity values.

We acknowledge that our results are subject to several caveats. First, the economic magnitude of the reduction in equity values that we document should be interpreted with caution. Investors can likely (to some extent) anticipate the selected focus-sectors, which means we could be underestimating the total impact of the increase in PFRE intensity. Moreover, if the firms included in our return-adjustment benchmarks benefit from PFRE, this could overstate the effect of the increase in regulatory scrutiny on targeted firms.

Second, it is impossible to definitively rule out the possibility that the observed response to the focus-sector announcements is partly attributable to the revelation of private information by the FRRP. However, features of our setting (e.g., that the FRRP focuses on prospective changes in publicly observable accounting policies rather than retroactive restatements of accounts) and the weight of our empirical evidence suggest that this is unlikely to explain our results. Specifically, if the revelation of private information is the reason for the observed reduction in equity values, it is difficult to explain: 1) that returns are lowest for the firms and industries where it is likely the most difficult for the FRRP to obtain private information; 2) that the announcement effect is more negative for firms with stronger private oversight (where firms are likely closer to their equity-value-maximizing level of transparency); 3) the absence of significant changes in information asymmetry around the focus-sector announcements; and 4) that audit committee turnover increases more after a restatement than after a firm is included in an FRRP focus sector.

Third, maximizing equity values is not equivalent to maximizing social welfare. Positive financial-reporting externalities can arise from network effects or because financial statement users cannot pass the costs of poor financial reporting on to shareholders (e.g., the IRS). To the extent that externalities occur within the focus sectors and are passed on to shareholders, our analysis

incorporates them. However, our analysis does not speak to cross-sector externalities or the economic benefits of transparency that do not accrue to shareholders.

Finally, we observe the change in proactive enforcement in only one equity market, the UK's LSE. Because the effects we document might depend on the initial level of enforcement, it is unclear to what extent these results can be generalized to other settings with different preexisting levels of enforcement or to the initial announcement of a proactive enforcement regime when no such regime existed previously. However, our analyses indicate that the costs of increased PFRE intensity are greatest for firms that 1) have high proprietary costs of disclosure, 2) have high preexisting levels of private oversight, and 3) choose to list on a less-regulated exchange. Thus, while our findings are necessarily specific to the particular institutional setting we examine, they indicate the types of firms likely to suffer most from an increase in proactive financial reporting enforcement, potentially allowing our findings to be generalized beyond the UK's FRRP focus-sector reviews. It is also worth noting that our study documents capital market benefits of enforcement that are similar to those shown in prior research. Thus, at least from the perspective of shareholders, our results suggest that the benefits documented in prior literature do not (alone) justify further increases in enforcement.

Although a complete accounting of the costs and benefits of disclosure regulation is beyond the scope of any single paper, understanding the net costs or benefits of regulation for directly targeted parties is a key component of understanding optimal regulatory policy. Although our results cannot speak to the potential costs or benefits for the shareholders of firms not targeted for PFRE (e.g., foreign competitors) or non-shareholder stakeholders (e.g., employees or debtholders), our results nonetheless suggest that an increase in PFRE intensity imposes significant net costs on the shareholders of targeted firms. It is an open question whether the benefits to parties other than shareholders of directly targeted firms can justify these costs and the costs of administering and overseeing PFRE.

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Appendix A: Issues identified by the FRRP reviews

Issue	*Explanation*
Accounting policies	Insufficient detail to enable reasonably knowledgeable users to understand the policy applied in practice. Revenue recognition is mentioned in all years, but other issues also fall into this category.
Associates and joint ventures	Additional disclosures are required when certain thresholds are exceeded.
Borrowing costs	Insufficient disclosure of the capitalization rate.
Business combinations and intangible assets	Insufficient disclosure of the factors related to the cost of a business and information on the combined entity. Insufficient disclosure of the recognition and measurement criteria.
Capital disclosures	Insufficient disclosures to help users evaluate a company's objectives, policies, and processes for managing capital.
Cash flow statements	Inconsistencies, incorrect classification of cash flows, insufficient disclosures.
Consolidation / acquisitions	Some entities not consolidated and insufficient disclosure of information for unconsolidated entities. Incorrect calculation of acquisition value or insufficient disclosure of that acquisition value.
Derivatives and other financial instruments	Insufficient disclosure of all the necessary information relevant to holding or issuing of financial instruments during the year.
Fair values in acquisition accounting	Lack of further clarification of the accounting treatments.
Financial instruments disclosures and presentation	Insufficient disclosure about risk arising from financial instruments.
Goodwill and intangible assets	Weak disclosures on the presumptions of accounting treatments.
Impairment of assets	Insufficient disclosures regarding the assumptions underlying management's cash flow projection, discount, and growth rates.
Income taxes, deferred tax	Unable to recognize deferred tax expenses completely.
Inventories	Insufficient disclosure of recognition and valuation.
Judgements, estimates, and risks	Disclosures tend towards boiler-plate and do not refer to the specific issues faced by individual companies.
Leases	Failure to disclose significant leasing arrangements (i.e., the total of future minimum lease payments).
Related-party disclosures	Lack of disclosures for non-executive directors. Insufficient disclosures regarding the nature of certain relationships and management compensation.
Reporting financial performance	Failure to present separately the aggregate results for continuing operations, acquisitions, and discontinued operations.
Retirement benefits	Insufficient disclosure of accounting measurements.
Revenue	Insufficient disclosure of practices and procedures (e.g., how the stage of completion was established and warranties and sales returns).
Segment disclosures	Segments omitted although it is clear that the entity has multiple entities from qualitative discussion of business. Insufficient explanations of the decision to aggregate operating segments.
Share-based payment	Insufficient disclosure of practices.
The effects of changes in foreign exchange rates	Insufficient disclosure of recognition and measurement.

Notes: This table presents a (non-exhaustive) list of issues identified by FRRP reviews from 2004 to 2011, collected from the FRRP's annual reports.

Appendix B: Variable Definitions

Focus-sector selection analysis:

Focus Sector	An indicator coded as one if industry *j* is part of an announced focus sector in year *t*, and zero otherwise (based on the four-digit ICB industry code).
Prior Return	The cumulative, unadjusted stock-market return for industry *j* (calculated as the equal-weighted average of all the firms in a particular 4-digit ICB industry) over the year prior to the date of a given year's focus-sector announcement.
Neg. Media Mentions	Number of negative media mentions about a 4-digit ICB industry *j* or about a firm in that industry over the year prior to the focus-sector announcement. We collect negative media mentions from Factiva.
% Ind. Restatements	The total number of accounting restatements (from *Worldscope*) for all firms in 4-digit ICB industry *j*, scaled by the total number of firms in industry *j*, over the year prior to the focus-sector announcement.

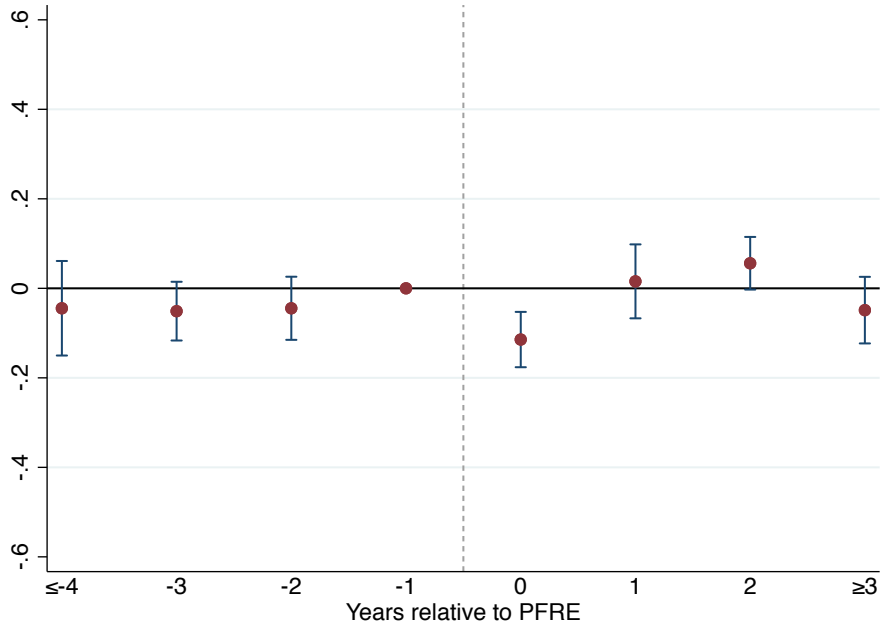
Focus-sector announcement-return analysis:

Abnormal Return	The daily adjusted return for firm *i* on day *t* in basis points (from *Datastream*). In the main specification, returns are adjusted by subtracting the average return for US (and alternatively UK) firms in the same four-digit ICB industry. In Table 4, we use three alternative abnormal-return measures, defined in the table notes.
Announcement	An indicator coded as one if day *t* is part of the (-2,+2)-day window around a focus-sector announcement and firm *i* is in an announced focus sector, and zero otherwise.
20 Days Before Announcement	An indicator coded as one for focus-sector firms over the 20 days *before* the focus-sector announcement window, and zero otherwise.
20 Days After Announcement	An indicator coded as one for focus-sector firms over the 20 days *after* the focus-sector announcement window, and zero otherwise.
Most (Least) Regulated Industries	An indicator coded as one for focus-sector industries that are heavily (not heavily) regulated, and zero otherwise. Heavily regulated industries include the financial services, utilities, telecommunications, and insurance industries.
Most Regulated Firms (MM)	An indicator coded as one for focus-sector firms traded on the LSE's Main Market exchange segment, identified using the market segment code (micdesc) from Refinitiv's QAD database, and zero otherwise. We include firms with the following segment codes: SET0, SET1, SET3, SSMM, and STMM.
Least Regulated Firms (AIM)	An indicator coded as one for focus-sector firms traded on the LSE's AIM exchange segment, identified using the market segment code (micdesc) from Refinitiv's QAD database, and zero otherwise. We include firms with the following segment codes: AIM, AIMI, and AMSM.
Most (Least) Oversight Blockholders (No Blockholders)	An indicator coded as one for focus-sector firms with (without) at least one external blockholder, where a blockholder is an indicator variable that equals one for each unique equity owner holding a position larger than 5% of shares outstanding (from the *Argus Vickers Owners Service Share Register Analysis System* (*AVSR*)), and zero otherwise.
High (Low) ROA	An indicator coded as one for focus-sector firms whose ROA is above (below) the 75th percentile of industry ROA in a particular year, and zero otherwise. ROA is defined as operating income after depreciation divided by average total assets.

Increased PFRE intensity and real effects analysis:

Mean Reversion of ROA	Mean-reversion rate for each four-digit ICB industry code *j* and year *t*; calculated by regressing the forward change in ROA on ROA. ROA is calculated by dividing operating income (after depreciation) by average total assets. The forward change in ROA is the change in ROA from year *t* to year *t+1*.
Investment Horizon Yr.1	CFO_{t+1} coefficient estimate from a regression of capital expenditures (*CAPEX*) in year *t* on cash flow from operations (*CFO*) in years *t+1* and *t+2* for each four-digit ICB industry and year.
Investment Horizon Yr.2	CFO_{t+2} coefficient estimate from a regression of capital expenditures (*CAPEX*) in year *t* on cash flow from operations (*CFO*) in years *t+1* and *t+2* for each four-digit ICB industry and year.

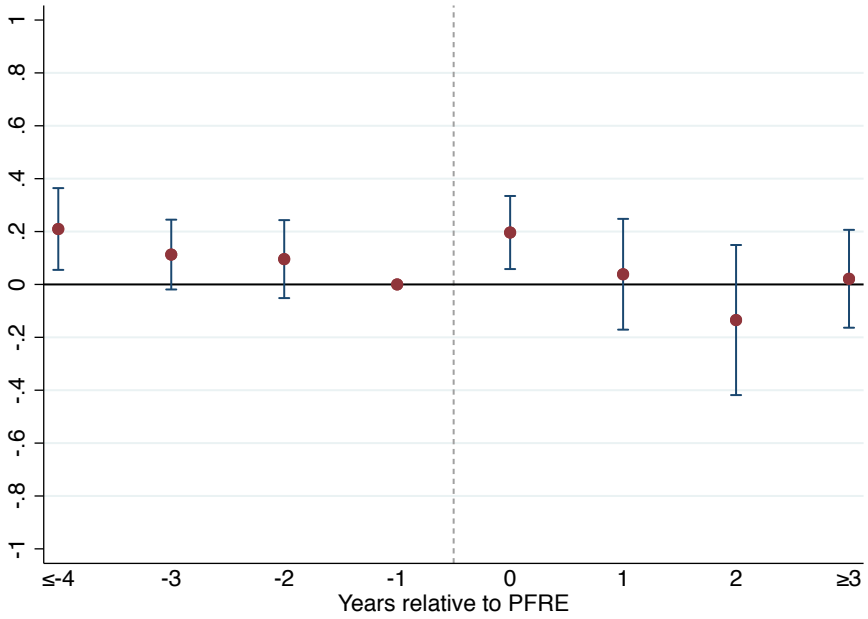
Figure 1: Pattern of Mean Reversion of ROA around PFRE (in Event Time)



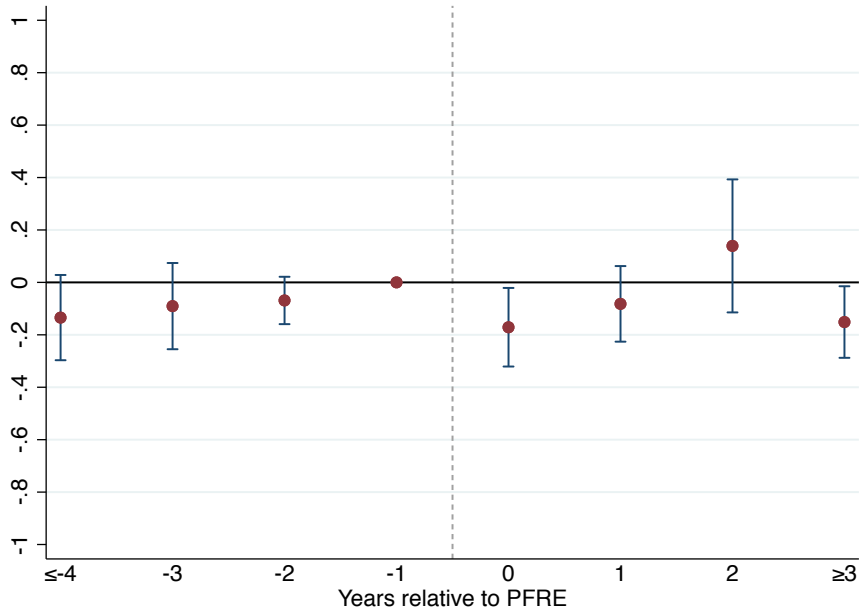
Notes: This figure plots the regression coefficient estimates and two-tailed 90% confidence intervals (based on standard errors clustered at the industry level) for the analysis of mean reversion in ROA in Table 7 Panel C. We map out the estimated counterfactual treatment effects in event time. We include, in one regression, indicators for all the years relative to PFRE with the exception of one year prior, which serves as the benchmark period (i.e., the coefficient is constrained to equal zero). We provide a detailed description of the variables in Appendix B and specific regression results in Table 7 Panel C.

Figure 2: Pattern of Investment Horizon Yr. 1 and Yr. 2 around PFRE (in Event Time)

Investment Horizon Yr. 1



Investment Horizon Yr. 2



Notes: This figure plots the regression coefficient estimates and two-tailed 90% confidence intervals (based on standard errors clustered at the industry level) for the analysis of investment horizon in Table 8 Panel B. We map out the estimated counterfactual treatment effects in event time. We include, in one regression, indicators for all the years relative to PFRE with the exception of one year prior, which serves as the benchmark period (i.e., the coefficient is constrained to equal zero). We provide a detailed description of the variables in Appendix B and specific regression results in Table 8 Panel B.

Table 1: FRRP Review Activity and Enforcement Intensity

Panel A: Review activities by year

Year	*All Reviews of UK Entities*			*Main Market*		*AIM*		*Unlisted*	
	Proactive	*Complaint/ Referral*	*Company Contacted*	*Proactive*	*Complaint/ Referral*	*Proactive*	*Complaint/ Referral*	*Proactive*	*Complaint/ Referral*
2005	n.d.	n.d.	n.d.	n.d.	n.d.	n.d.	n.d.	n.d.	n.d.
2006	149	59	64	101	25	34	9	14	25
2007	216	42	124	153	21	42	6	21	15
2008	156	65	127	102	44	14	16	40	5
2009	225	49	111	115	33	73	16	37	0
2010	216	44	143	109	22	77	12	30	10
2011	232	29	141	139	22	80	3	13	4
2012	243	27	129	166	13	55	11	22	3
Average	205	45	120	126	26	54	10	25	9

Panel B: Enforcement intensity for focus and non-focus sectors

	All firms	*Focus-Sector firms*	*Non-Focus-Sector firms*
Review-year 2005:			
Number of reviews	135	66	69
Improvements because of reviews	30	10	20
Improvement percentage	22%	15%	29%
Number of firms	2,486	191	2,295
Review-year 2006:			
Number of reviews	195	72	123
Improvements because of reviews	82	22	60
Improvement percentage	42%	31%	49%
Number of firms	2,483	400	2,083
Probability of Review Calculation:			
Average probability of review	6.6%	23.4%	4.4%
Incremental probability relative to average	—	251.6%	-34.0%
Incremental probability for focus relative to non-focus sector firms	—	432.4%	—

Notes: This table reports details on the FRRP's review activities by year from 2005 to 2012, corresponding to the focus-sector announcements from 2004 to 2011 (Panel A) and to enforcement intensity for focus and non-focus sectors in 2005 and 2006 (Panel B). Information on review activity and enforcement activity is collected from FRRP annual reports. In Panel A, "n.d." indicates that the information was not disclosed in the FRRP annual report for that year. In Panel B, the *Incremental probability relative to average* is calculated by comparing the probability of a review (conditional on knowing the focus sectors and assuming there is no anticipation of the selected sectors) to the unconditional *Average probability of review* (e.g., (23.4%-6.6%)/6.6%=251.6%). The *Incremental probability for focus relative to non-focus sector firms* is calculated by comparing the *Average probability of review* for focus-sector to non-focus-sector firms conditional on knowing the focus sectors and assuming there is no anticipation of the selected sectors (i.e., 23.4%-4.4%)/4.4%=432.4%).

Table 2: Focus Sectors from 2004 to 2011

Periods		*Focus Sectors (treated)*					*N (firms)*	
Announcement Dates	*Fiscal Year*	*First*	*Second*	*Third*	*Fourth*	*Fifth*	*Focus Sector*	*Non-Focus Sector*
		(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*
December 21, 2004	2004/05	Automobile 3	Pharma-ceutical 19	Retail 46	Transport 12	Utility 12	92	675
December 12, 2005	2005/06	Automobile 3	Pharma-ceutical 23	Retail 47	Transport 12	Utility 15	100	817
December 11, 2006	2006/07	Travel and leisure 63	Retail 47	Utility 17	Telecom-munications 16	Media 62	205	859
November 9 2007	2007/08	Banking 112	Retail 53	Travel and leisure 69	Commercial property 52	House builders 12	298	883
October 30, 2008	2008/09	Banking 106	Retail 54	Travel and leisure 68	Commercial property 51	House builders 11	290	888
December 9, 2009	2009/10	Commercial Property 34	Advertising 23	Recruitment 12	Media 25	Information technology 75	169	735
November 25, 2010	2010/11	Commercial Property 39	Insurance 26	Support services 113	Travel 20	—	198	805
December 9, 2011	2011/12	Commercial Property 37	Retail 47	Support services 115	—	—	199	829

Notes: This table presents the announcement dates, review years, focus sectors, and number of treated and non-treated firms included in our sample each year from 2004-2011. Announcement dates and focus sectors are collected from FRRP press releases.

Table 3: Observables Explaining Focus-Sector Selection

Panel A: Descriptive Statistics for Selection-Model Analysis

	Mean	*Std. Dev.*	*Median*
Prior Return	0.011	0.279	0.022
Neg. Media Mentions	11.374	14.480	7.000
% Ind. Restatements	0.328	0.271	0.300

Panel B: Focus-Sector Selection Model

Dependent Variable: Focus Sector	
Prior Return	-0.129**
	(-2.43)
Neg. Media Mentions	0.002***
	(2.85)
% Ind. Restatements	0.067*
	(1.70)
Fixed effects:	
Industry	Yes
Observations	791
Adj. R-squared	0.254

Notes: This table reports results from the focus-sector selection model. The sample period is from 2004 to 2011. Panel A presents descriptive statistics and Panel B presents OLS regression-coefficient estimates. The dependent variable, *Focus Sector*, is a binary indicator variable that takes the value of one for years when a given industry is selected as a focus sector (based on the four-digit ICB industry code), and zero otherwise. See Appendix B for further details on the variable definitions. We cluster standard errors by industry and report t-statistics in parentheses. The regression includes industry fixed effects. *, **, and *** indicate statistical significance (two-sided) at the 10%, 5%, and 1% levels, respectively.

Table 4: Increased PFRE Intensity and Equity Returns

Panel A: Average Stock-Market Responses to the Announcement of the Focus Sectors

Dependent Variable: Abnormal Return	*Main Specification*	*Sensitivity Tests*			
	UK & US Ind. adj.	*UK adj. only*	*US adj. only*	*Europe Ind. adj.*	*FMB*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
20 Days Before Announcement	-5.45	-6.31**	-5.53	-5.41*	-4.39
	(-1.20)	(-2.16)	(-0.45)	(-1.73)	(-0.94)
Announcement	-26.98***	-16.08**	-39.66*	-18.45**	-23.09***
	(-4.50)	(-2.33)	(-1.71)	(-2.54)	(-3.91)
20 Days After Announcement	2.90	-3.06	-2.08	-1.49	0.77
	(0.76)	(-1.46)	(-0.21)	(-0.61)	(0.12)
Fixed effects:					
Day	Yes	Yes	Yes	Yes	Yes
Observations	1,997,445	1,997,445	3,425,749	1,997,445	8
Adj. R-squared	0.118	0.053	0.112	0.041	0.116

Panel B: Stock-Market Responses to the Individual Focus-Sector Announcements

Dependent Variable: Abnormal Return (UK & US Ind. adj.)	
Treatment effect by year:	
2004 Announcement	-4.26
	(-0.77)
2005 Announcement	4.01
	(0.30)
2006 Announcement	-38.29***
	(-5.36)
2007 Announcement	-45.77***
	(-3.81)
2008 Announcement	-20.17
	(-0.80)
2009 Announcement	-20.88***
	(-3.12)
2010 Announcement	-26.57**
	(-2.40)
2011 Announcement	-32.80***
	(-3.14)
Fixed effects:	
Day	Yes
Observations	1,997,445
Adj. R-squared	0.118

Table 4 continued

Notes: This table reports results from our analysis of the effect of increased PFRE intensity on equity returns. The sample period is from 2004 to 2012. Panel A presents average stock market responses; Panel B presents the stock market responses individually for each of the eight focus-sector announcements. The dependent variable is the daily return for firm *i* on day *t* in basis points, adjusted as indicated in each column. In Column (1), returns are industry-adjusted by US returns and the benchmark is other, non-focus-sector UK firms. Column (2) presents results using unadjusted returns (i.e., without subtracting the US industry return, but still relative to non-focus-sector UK firms). Column (3) presents results without subtracting the US industry return, but using US firms as the benchmark (instead of non-focus-sector UK firms). Column (4) presents results using a European (instead of a US) return adjustment (based on an index consisting of firms located in countries which joined the EU prior to 2000). Column (5) presents results using the Fama-MacBeth (1973) approach. The variable of interest, *Announcement*, is a binary indicator that takes the value of one if day *t* is part of the (-2,+2)-day window around a focus-sector announcement and firm *i* is in an announced focus sector, and zero otherwise. *20 Days Before (After) Announcement* is an indicator coded as one for focus-sector firms during the 20 days before (after) the announcement window, and zero otherwise. In Panel B, we present results for the main specification where we replace the single *Announcement* indicator with a separate indicator for each of the eight focus-sector announcements. See Appendix B for further details on the variable definitions. We cluster standard errors by day and report t-statistics in parentheses. All regressions include day fixed effects. *, **, and *** indicate statistical significance (two-sided) at the 10%, 5%, and 1% levels, respectively.

Table 5: Increased PFRE Intensity and Equity Returns Conditional on Market Capitalization

Dependent Variable: Abnormal Return (UK & US Ind. adj.)	*Size Q1* *(1)*	*Size Q2* *(2)*	*Size Q3* *(3)*	*Size Q4* *(4)*	*Size Q5* *(5)*
Announcement	-47.15***	-42.28***	-30.61***	-18.88*	-11.29
	(-4.15)	(-3.41)	(-3.21)	(-1.66)	(-0.83)
Economic Significance:					
Mean Market Capitalization Jan. 1	15.02	33.23	85.35	302.70	3,371.00
Absolute Decrease in Mrkt. Cap. ($m)	-0.354	-0.702	-1.306	-2.857	-19.029
Fixed effects:					
Day	Yes	Yes	Yes	Yes	Yes
Observations	399,570	399,627	399,495	399,385	399,368
Adj. R-squared	0.105	0.120	0.131	0.135	0.249

Notes: This table reports results from our analysis of the effect of increased PFRE intensity on equity returns conditional on market capitalization. The sample period is from 2004 to 2012. The dependent variable is the daily UK- and US-industry-adjusted return for firm *i* on day *t* in basis points. Columns (1)-(5) present the results for firms of different sizes by limiting the sample to firms in each of the five quintiles of market capitalization. The variable of interest, *Announcement*, is a binary indicator that takes on the value of one if day *t* is part of the (-2,+2)-day window around a focus-sector announcement and firm *i* is in an announced focus sector, and zero otherwise. We cluster standard errors by day and report t-statistics in parentheses. All regressions include day fixed effects. *, **, and *** indicate statistical significance (two-sided) at the 10%, 5%, and 1% levels, respectively.

Table 6: Increased PFRE Intensity and Equity Returns Conditional on Prior Enforcement

	Public Oversight				*Private Oversight*	
	Non-Securities Regulators		*Securities Regulator*		*Corporate Governance*	
Dependent Variable: Abnormal Return (UK & US Ind. adj.)	*Most Regulated Industries*	*Least Regulated Industries*	*Most Regulated Firms (MM)*	*Least Regulated Firms (AIM)*	*Most Oversight (Blockholders)*	*Least Oversight (No Blockholders)*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Announcement	-8.96	-28.53***	-11.69	-44.52***	-28.74***	0.42
	(-0.71)	(-4.01)	(-1.45)	(-3.88)	(-4.26)	(0.04)
Difference (p-value)	0.198		0.020		0.010	
Fixed effects:						
Day	Yes	Yes	Yes	Yes	Yes	Yes
Observations	283,050	1,714,395	876,665	817,008	1,813,921	183,524
Adj. R-squared	0.145	0.117	0.168	0.111	0.115	0.234

Notes: This table reports results from our analysis of the effect of increased PFRE intensity on equity returns conditional on prior enforcement. The sample period is from 2004 to 2012. The dependent variable is the daily UK- and US-industry-adjusted return for firm *i* on day *t* in basis points. Columns (1) and (2) compare the results for the most regulated and least regulated focus-sector industries, where the most regulated industries include financial services, utilities, telecommunications, and insurance and the least regulated are all remaining industries. Columns (3) and (4) compare the results for the most and least regulated firms, determined by their LSE market segment, where MM is the Main Market, an EU-regulated exchange, and where AIM is a less-regulated exchange exempt from many EU regulatory provisions. Columns (5) and (6) compare the results for firms with and without at least one blockholder, where 5% equity ownership constitutes a blockholder. The variable of interest, *Announcement*, is a binary indicator that takes the value of one if day *t* is part of the (-2,+2)-day window around a focus-sector announcement and firm *i* is in an announced focus sector, and zero otherwise. We cluster standard errors by day and report t-statistics in parentheses. All regressions include day fixed effects. *, **, and *** indicate statistical significance (two-sided) at the 10%, 5%, and 1% levels, respectively.

Table 7: Increased PFRE Intensity and Proprietary Costs

Panel A: Descriptive Statistics for Proprietary Cost Analysis

	N	*Mean*	*Std. Dev*	*Median*
ROA	7,258	0.143	0.102	0.124
Total Assets ($million)	7,258	1,251.688	6,681.470	128.680
Mean Reversion of ROA [β_1 from Eq. (3)]	283	-0.226	0.240	-0.190

Panel B: Increased PFRE Intensity and Equity Returns Conditional on Return on Assets

Dependent Variable:	*High ROA*	*Low ROA*
Abnormal Return (UK & US Ind. adj.)	*(1)*	*(2)*
Announcement	-37.90***	-18.27***
	(-3.93)	(-3.61)
Difference (p-value)	0.025	
Fixed effects:		
Day	Yes	Yes
Observations	378,301	876,733
Adj. R-squared	0.157	0.161

Panel C: Increased PFRE Intensity and Competition

Dependent Variable:	*Only Year t=0*	*All Periods*
Mean Reversion of ROA	*(1)*	*(2)*
$Year_{t\leq -4}$		-0.04
		(-0.69)
$Year_{t=-3}$		-0.05
		(-1.28)
$Year_{t=-2}$		-0.04
		(-1.04)
$Year_{t=-1}$ (Benchmark)		0.00
		(--)
$Year_{t=0}$	-0.09**	-0.11***
	(-2.22)	(-3.05)
$Year_{t=1}$		0.02
		(0.31)
$Year_{t=2}$		0.06
		(1.57)
$Year_{t\geq 3}$		-0.05
		(-1.08)
Fixed effects:		
Industry	Yes	Yes
Year	Yes	Yes
Observations (industry-years)	283	283
Adj. R-squared	0.181	0.181

Notes: This table reports results from our analysis of increased PFRE intensity and proprietary costs. The sample period is from 2001 to 2016. Panel A presents descriptive statistics. Panel B presents average stock market responses for *High* and *Low ROA* sample partitions. The dependent variable is the daily UK- and US-industry-adjusted return for firm *i* on day *t* in basis points. The variable of interest, *Announcement*, is a binary indicator that takes the value of one if day *t* is part of the (-2,+2)-day window around a focus-sector announcement and firm *i* is in an announced focus sector, and zero otherwise. The *High (Low) ROA* partition includes firms whose return on assets is above (below) the 75th percentile of industry ROA in a particular year. Panel C reports the results of a weighted least-squares regression (where the weighting factor is the number of firms in an industry-year) of *Mean Reversion of ROA* on a single indictor for the year subsequent to the focus-sector reviews (Column 1) and separate yearly indicators for the four periods prior and subsequent to the focus-sector reviews (Column 2). See Appendix B for further details on variable definitions. We cluster standard errors by industry and report t-statistics in parentheses. All regressions include industry and year fixed effects. *, **, and *** indicate statistical significance (two-sided) at the 10%, 5%, and 1% levels, respectively.

Table 8: Increased PFRE Intensity and Investment Horizons

Panel A: Descriptive Statistics for Investment Horizons Analyses

	N	*Mean*	*Std. Dev*	*Median*
CAPEX ($million)	12,500	24.253	84.284	1.132
CFO ($million)	12,500	49.923	181.697	2.322
Invest Horizon Yr. 1 [β_1 from Eq. (5)]	426	0.184	0.454	0.123
Invest Horizon Yr. 2 [β_2 from Eq. (5)]	426	0.136	0.439	0.087

Panel B: Change in Investment Horizon Around Increased PFRE Intensity

Dependent Variable:	*Invest Horizon Yr. 1*		*Invest Horizon Yr. 2*	
Invest Horizon Yr.1 or Yr. 2	*t=0 only*	*All periods*	*t=0 only*	*All periods*
	(1)	*(2)*	*(3)*	*(4)*
$Year_{t\leq -4}$		0.21**		-0.13
		(2.23)		(-1.36)
$Year_{t=-3}$		0.11		-0.09
		(1.41)		(-0.91)
$Year_{t=-2}$		0.10		-0.07
		(1.07)		(-1.26)
$Year_{t=-1}$ (Benchmark)		0.00		0.00
		(--)		(--)
$Year_{t=0}$	0.15*	0.20**	-0.14*	-0.17*
	(2.03)	(2.34)	(-1.80)	(-1.88)
$Year_{t=1}$		0.04		-0.08
		(0.30)		(-0.94)
$Year_{t=2}$		-0.13		0.14
		(-0.78)		(0.90)
$Year_{t\geq 3}$		0.02		-0.15*
		(0.19)		(-1.82)
Fixed effects:				
Industry	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Observations (industry-years)	426	426	426	426
Adj. R-squared	0.039	0.057	-0.003	0.000

Notes: This table reports results from our analysis of increased PFRE intensity and investment horizons. The sample period is from 2001 to 2016. Panel A presents descriptive statistics. Panel B reports the results of a weighted least-squares regression (where the weighting factor is the number of firms in an industry-year) of *Investment Horizon Yr. 1* (Columns 1 and 2) or *Investment Horizon Yr. 2* (Columns 3 and 4) on a single indictor for the year subsequent to the focus-sector reviews (Columns 1 and 3) and separate yearly indicators for the four periods prior and subsequent to the focus-sector reviews (Columns 2 and 4). See Appendix B for further details on variable definitions. We cluster standard errors by industry and report t-statistics in parentheses. All regressions include industry and year fixed effects. *, **, and *** indicate statistical significance (two-sided) at the 10%, 5%, and 1% levels, respectively.

For Online Publication

Internet Appendix for "Proactive Financial Reporting Enforcement and Shareholder Wealth"

We summarize the content of the tables as follows:

Section IA1: Breakdown of Focus-Sector to ICB Code Mapping

Section IA2: Sample Selection Criteria and Sensitivity Analyses for the Stock-Market-Response Analysis

Section IA3: Internet Appendix Variable Definitions

Section IA4: Increased PFRE Intensity and Firm Disclosure

Section IA5: Increased PFRE Intensity and Regulatory Compliance Costs

Section IA6: Increased PFRE Intensity and Stock Market Liquidity

Section IA7: Changes in Information Asymmetry around the Focus-Sector Announcements

Section IA8: Labor Market Consequences of Focus-Sector Selection

Section IA9: AIM vs. Main Market and the Role of Firm Size

Section IA1: Breakdown of Focus-Sector to ICB Code Mapping

In this section, we provide a breakdown of our mapping of the FRRP's disclosed focus sectors (as reported in Table 1 of the paper) to an Industry Classification Benchmark (ICB) code—which are the primary industry identifiers in the *Infinitiv Datastream* and *Worldscope* databases.

Table IA1: Focus-Sector to ICB Code Mapping

Year	ICB Code	ICB Description	Focus Sector
2004	33	Automobiles & Parts	First
	4577	Pharmaceuticals	Second
	53	Retail	Third
	277	Industrial Transportation	Fourth
	7	Utilities	Fifth
2005	33	Automobiles & Parts	First
	4577	Pharmaceuticals	Second
	53	Retail	Third
	277	Industrial Transportation	Fourth
	7	Utilities	Fifth
2006	575	Travel & Leisure	First
	53	Retail	Second
	7	Utilities	Third
	6	Telecommunications	Fourth
	55	Media	Fifth
2007	83, 87	Banks, Financial Services	First
	53	Retail	Second
	575	Travel & Leisure	Third
	863	Real Estate Investment & Services	Fourth
	3728	Home Construction	Fifth
2008	83, 87	Banks, Financial Services	First
	53	Retail	Second
	575	Travel & Leisure	Third
	863	Real Estate Investment & Services	Fourth
	3728	Home Construction	Fifth
2009	863	Real Estate Investment & Services	First
	55	Media (including Advertising)	Second & Fourth
	2793	Business Training & Employment Agencies	Third
	9	Technology	Fifth
2010	863	Real Estate Investment & Services	First
	85	Insurance	Second
	279	Support Services	Third
	5759	Travel & Tourism	Fourth
	5751	Airlines	
	5753	Hotels	
2011	863	Real Estate Investment & Services	First
	53	Retail	Second
	279	Support Services	Third

Section IA2: Sample Selection Criteria and Sensitivity Analyses for the Main Stock-Market-Response Analysis

In this section, we describe our sample selection criteria and assess the sensitivity of our results to these choices (where possible). Table IA2 Panel A reports the sample selection criteria for our main return analyses in Table 4. Data were downloaded from the *Refinitive Datastream Quantitative Analytics Database* (QAD) in December of 2017. Our downloading procedure imposed the following restrictions: 1) must be a firm's 'primary' security (*IsPrimQt=1*), 2) the security type must be equity (*TypeCode=EQ*), 3) must be a firm's 'major' security (*IsMajorSec=Y*), 4) firm's primary activities must be in the U.K. (*CmpyCtryCode=GB*), 5) must be the firm's primary exchange listing (*IsPrimExchQt=Y*), 6) the security must be traded on the London Stock Exchange (LSE) (*exchname=London*). This download procedure leads to an initial sample of 3,612,700 firm-day observations over *Enforcement Years* 2004 to 2012 (where an *Enforcement Year* begins on July 1st of year *t* and ends on June 30 of year *t+1*).

There are 458,592 observations that do not have the returns, market capitalization, or industry data necessary to estimate our primary specification. There are 1,156,663 observations that we exclude for the following reasons: 1) missing *miscdesc*, *miscdesc* identifies the LSE exchange segment and is required to identify firms traded on the AIM versus the Main Market; 2) to limit the influence of small firms; 3) to avoid extreme returns for the banking sector during the 2008 financial crisis and beyond; 4) to limit the influence of extreme return observations. However, it is still possible to estimate our main specification in Table 4 Panel A of the Manuscript excluding each of these four criteria. In Table IA2 Panel B, we present results for our main specification (Column 1 of Table 4) where we sequentially omit each of these selection criteria. Our inferences are not materially affected by any of these choices.

In Row (5) of Table IA2 Panel B, we additionally assess the robustness of our results to imposing a stricter cutoff of ± 10% on return values and find that our inferences are unchanged.

Table IA2: Sample Selection Criteria and Sensitivity Analyses

Panel A: Sample Selection Criteria for Main Specification in Table 4 of the Manuscript

	Observations lost	*Sample*
	(1)	*(2)*
Unique Dscode-Date Observations for Enforcement Years 2004-2011		3,612,700
Non-discretionary Sample Retrictions:		
Drop U.K. Market Holidays	-85,228	
Missing Return or Market Cap. Data	-59,303	
Post-Delisting Observations	-48,005	
Not Listed for the Full Enforcement Year	-125,569	
Non-trading firms	-6,383	
Missing Industry ICB code	-134,104	-458,592
Sample befor Discretionary Sample Restrictions		3,154,108
Discretionary Sample Restrictions:		
1. Missing micdesc	-374,091	
2. Below $10 million Market Cap.	-772,195	
3. Banks	-10,326	
4. Drop Daily Return > 1	-51	-1,156,663
Final Sample		1,997,445

Table IA2 Panel B: Stock Market Return Sensitivity Analyses

Panel B: Sensitivity Analyses

Dependent Variable: Abnormal Return (UK & US Ind.-adj.)	*Results After Sample Restriction*
Sample Restriction Removed:	
1. Missing micdesc	-24.48***
	(-4.23)
2. Below $10 million Market Cap.	-25.82***
	(-4.07)
3. Banks	-26.25***
	(-3.99)
4. Drop Daily Return > 1	-28.54***
	(-4.51)
Sample Restriction added:	
5. Drop Daily Returns less than -0.10 or greater than 0.10	-13.81**
	(-2.46)

Section IA3: Internet Appendix Variable Definitions

In this section, we define the variables used in the analyses reported in this Internet Appendix.

Table IA3: Variable Definitions

Focus-sector selection analysis:	
Focus Sector	An indicator coded as one if industry *j* is part of an announced focus sector in year *t*, and zero otherwise (based on the four-digit ICB industry code).
Prior Return	The cumulative unadjusted stock market return for industry *j* (calculated as the equal-weighted average of all the firms in a particular 4-digit ICB industry) over the year prior to the date of a given year's focus-sector announcement (i.e., *t-1*).
Negative Media Mentions	Number of negative media mentions of an industry or of firms in that industry over the year prior to the date of a given year's focus-sector announcement (i.e., *t-1*). We collect negative media mentions from Factiva.
% Industry Restatements	The total number of accounting restatements (from *Worldscope*) for all firms in industry *j*, scaled by the total number of firms in industry *j*, over the year prior to the date of a given year's focus-sector announcement (i.e., *t-1*).
Firm disclosure analysis:	
FS Length	Word count of the financial statement component of the annual report, including the accompanying notes to the financial statements. Based on data from the UK Annual Report Narrative Disclosure Scores Database available at: http://reshare.ukdataservice.ac.uk/851859/.
Narrative Length	Word count of all sections of the annual report other than the financial statements section. Based on data from the UK Annual Report Narrative Disclosure Scores Database available at: http://reshare.ukdataservice.ac.uk/851859/.
Audit fee analysis:	
Audit Fee	Total annual audit fees in millions of USD (from *Worldscope*).
ln(Total Assets)	The natural log of total assets in millions of USD (from *Worldscope*).
Leverage	The ratio of total liabilities to total assets (from *Worldscope*).
ROA	The ratio of net income to total assets (from *Worldscope*).
Current Ratio	The ratio of current assets to current liabilities (from *Worldscope*).
Foreign-Sales Ratio	The ratio of foreign sales to total net sales (from *Worldscope*).
Loss	An indicator coded as one if the firm has negative net income, and zero otherwise (from *Worldscope*).
Cash Flow	The ratio of cash flow from operations to total assets (from *Worldscope*).
BigN Auditor	An indicator coded as one if the firm has a BigN auditor (from *Worldscope*). We classify Deloitte, PwC, EY, Arthur Andersen, and KPMG as BigN Auditors.
Working Capital	The ratio of accounts receivable and inventory to total assets (from *Worldscope*).
Restatement	An indicator coded as one if the firm issues an accounting restatement, and zero otherwise (from *Worldscope*).
Liquidity analysis:	
Zero Returns	The proportion of trading days with zero daily stock returns out of all potential trading days in a given quarter (from *Datastream*).

ln(Market Value)	The natural log of the market value measured at the end of the quarter. Market value is stock price times the number of shares outstanding measured at the end of the quarter (from *Datastream*).
ln(Share Turnover)	The natural log of the quarterly mean of daily turnover, calculated as trading volume divided by market value of equity (from *Datastream*).
ln(Return Variability)	The natural log of the standard deviation of daily stock returns over a given quarter (from *Datastream*).

Bid-ask spread analysis:

Bid-Ask Spread	The daily quoted percentage spread (from *Datastream*), calculated as the difference between the ask and the bid prices divided by the mid-point (i.e., (Ask-Bid)/((Ask+Bid)/2)).
ln(Market Value)	The natural log of the daily market value of equity (from *Datastream*). Market value is stock price times the number of shares outstanding.
ln(Share Turnover)	The natural log of daily turnover, calculated as daily trading volume divided by market value of equity (from *Datastream*).
ln(Return Variability)	The natural log of the square of daily stock return (from *Datastream*).

Audit committee turnover analysis:

Audit Committee Turnover	An indicator coded as one if any audit committee ("AC") members leaves the firm in a given year (from *Board-Ex*).
Restatement	An indicator coded as one if the firm receives a restatement in a given year (from *Worldscope*).
Prior Return	The stock market return over the year prior to the date of a given year's focus-sector announcement (from *Datastream*).

Focus-sector announcement return analysis:

Abnormal Return	The daily industry-adjusted abnormal return for firm *i* on day *t* in basis points (from *Datastream*). Returns are adjusted by subtracting the average return for U.S. firms in the same four-digit ICB industry.
Announcement	An indicator coded as one if day *t* is part of the specified event window and firm *i* is in an announced focus sector, and zero otherwise
MM	Firms traded on the LSE's Main Market exchange segment, identified using the market segment code (micdesc) from Refinitiv's QAD database. We include firms with the following segment codes: SET0, SET1, SET3, SSMM, and STMM.
AIM	Firms traded on the LSE's Alternative Investment Market exchange segment, identified using the market segment code (micdesc) from Refinitiv's QAD database. We include firms with the following segment codes: AIM, AIMI, and AMSM.
AIM-big (AIM-small)	An indicator coded as one for if the firm is above (below) the median market capitalization for all AIM firms, measured at the beginning of the year, and zero otherwise.
MM-big (MM-small)	An indicator coded as one for if the firm is above (below) the median market capitalization for all Main Market firms, measured at the beginning of the year, and zero otherwise.

Section IA4: Increased PFRE Intensity and Firm Disclosure

In this section, to provide direct evidence on the impact of the FRRP review process on firm disclosure, we examine changes in the length of focus-sector firms' annual financial reports.[32] Most of the issues identified by the FRRP's proactive focus-sector reviews lead to future changes in disclosure, rather than retroactive restatements of accounts. Thus, an examination of subsequent changes in financial-statement length provides a preliminary way to gauge the magnitude of firms' responses to increased PFRE intensity.

We measure changes in financial-statement length based on the number of words in the financial statement section of the annual report, including the accompanying notes, but excluding other narrative components of the annual report (*FS Length*). We employ a difference-in-differences design that compares the change in *FS Length* for firms included in FRRP focus sectors relative to non-focus sectors and estimate the following OLS regression:

$$FS\ Length_{i,t} = \beta_1 PFRE_{j,(t=1\ to\ t=n)} + \beta_2 Narrative\ Length_{i,t} + \sum \gamma_i Controls_{i,t}$$

$$+ \sum \delta_i Fixed\ Effects + \varepsilon_{i,t}$$

For focus-sector firms, $PFRE_{j,(t=1\ to\ t=n)}$ takes the value of one in the first fiscal year after the focus-sector announcement and for all fiscal years thereafter, and zero otherwise. To control for the non-random selection of the focus sectors and changes in financial statement length unrelated to PFRE, we include the word count of all sections, other than the financial statements, within the same annual report, $Narrative\ Length_{i,t}$, as well as industry and year fixed effects. We use industry (rather than firm) fixed effects because we have insufficient observations for individual firms to estimate reliable treatment effects with firm fixed effects.

Descriptive statistics in Panel A of Table IA4 indicate that, on average, a firm's annual report contains a financial statement section of approximately 15,500 words and a narrative section of approximately 11,900 words. Table IA4 Panel B presents OLS regression coefficient estimates. The coefficient on $PFRE_{(t=1\ to\ t=n)}$ in Column (1) indicates that the length of focus-sector firms' financial statements increases by a statistically significant 580 words relative to non-focus sector firms in the fiscal year following the focus-sector announcement—an approximately 3.8% increase in the length of the financial statements.

Because we expect financial statement length to change proportionally in response to PFRE, we do not take the natural log in our main analysis. However, in untabulated robustness tests, we

[32] Data for this analysis are from the *UK Annual Report Narrative Disclosure Scores Database* available at: http://reshare.ukdataservice.ac.uk/851859/. We are grateful to Martin Walker, Steven Young, Paul Rayson, El-Haj Mahmoud, Vasiliki Athanasakou, and Thomas Schleicher for making this data available and for providing additional information useful in conducting this analysis.

confirm that our inferences are very similar if we instead use the natural log of financial statement length as the dependent variable.

The possibility that firms endogenously respond to the issues identified as determinants in the focus-sector-selection model (i.e., poor stock performance, negative media mentions, and restatements) or other (unobservable) factors by altering their reporting policies poses a challenge in attributing any observed changes in firm disclosure to the effects of PFRE. To address this issue, we include controls for the observable determinants of focus-sector selection identified in the prediction model in Table 3 of the Manuscript. Controlling for these characteristics directly addresses selection on these observables, and, to the extent the observable and unobservable characteristics that determine focus-section selection are correlated, provides a way to gauge the potential selection on unobservable characteristics. Inconsistent with the increase in disclosure being attributable to an endogenous response to the prior year's performance, the results in Column (2) of Table IA4 Panel B indicate that the inclusion of *Prior Return*, *Negative Media Mentions*, and *% Industry Restatements* has little effect on the estimated magnitude of $PFRE_{(t=1 \text{ to } t=n)}$. In fact, the magnitude of the treatment effect actually increases, suggesting that any potential selection on unobservable industry characteristics would have to exhibit a correlation of the opposite sign as that between *FS Length* and *Prior Return*, *Negative Media Mentions*, *and % Industry Restatements* and be quite large in magnitude to explain the estimated treatment effect (a possibility that seems unlikely).

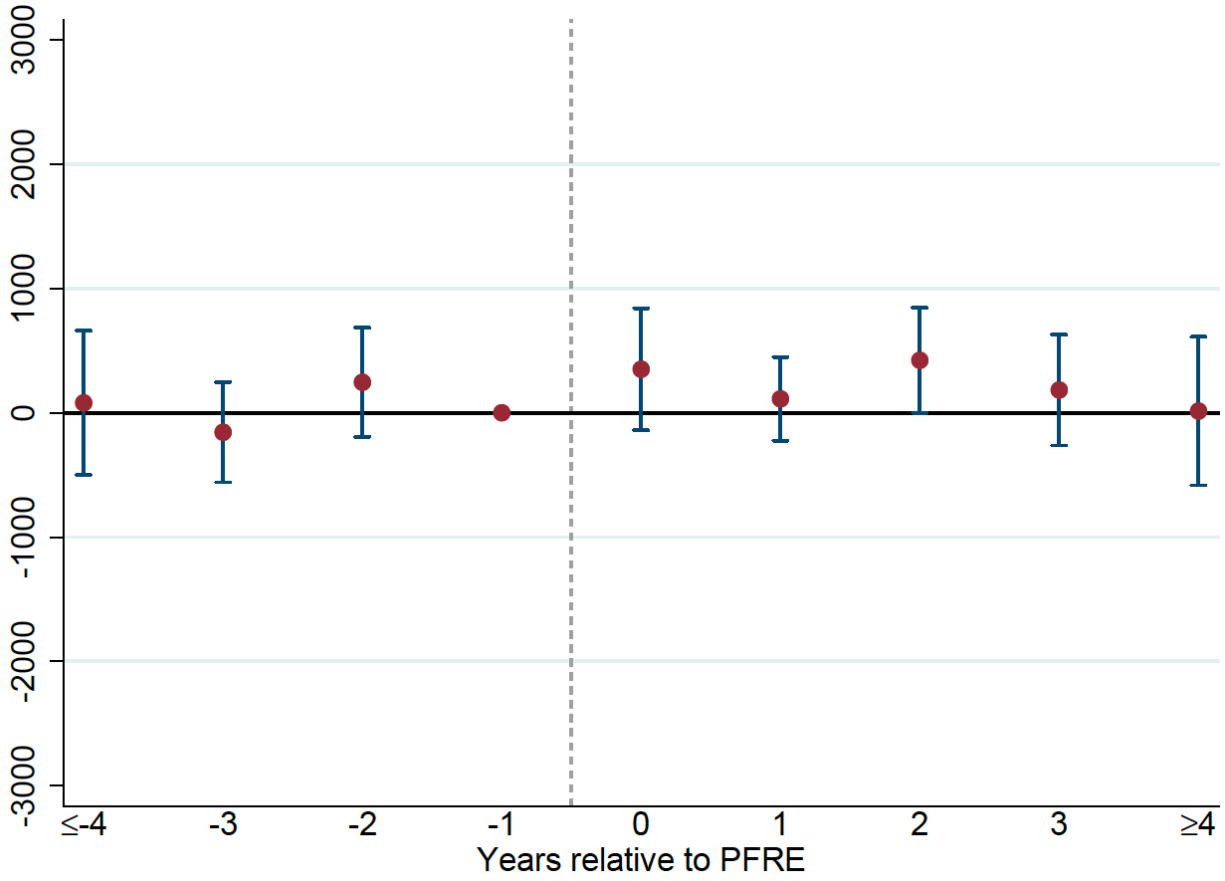
To test whether the effect persists beyond the enforcement year, we partition $PFRE_{(t=1 \text{ to } t=n)}$ into two separate indicators, $PFRE_{(t=1)}$ and $PFRE_{(t=2 \text{ to } t=n)}$, where, for focus-sector firms, $PFRE_{(t=1)}$ ($PFRE_{(t=2 \text{ to } t=n)}$) takes the value of one during the year of the increased FRRP scrutiny (for all years subsequent to the increase in FRRP scrutiny), and zero otherwise. In this way, we separately estimate the disclosure response for the first fiscal year-end following focus-sector selection and the change for all years thereafter. Results in Column (3) of Table IA4 Panel B indicate that *FS Length* increases significantly in the year of the FRRP focus-sector reviews and persists beyond the year of focus-sector selection. Results in Column (4) again suggest that the inclusion of the control variables from the selection model has little effect on the estimated coefficients.

To assess the validity of the parallel-trends assumption and illustrate the persistence of the increase in financial statement length following PFRE, in Figure IA4, we plot the estimated counterfactual treatment effects in event time for the entire sample period by including separate indicators for all years before and after PFRE, except *t-1*, which we use as the benchmark. The estimated effects are close to zero during the four-year period leading up to PFRE. There is a relatively sharp increase after PFRE. In year *t+1* and beyond, the estimated effect is not statistically significant for all years, but is above zero throughout the sample period, however there is some evidence that the effect of PFRE dissipates over periods of four years and longer (however, note that it is difficult to interpret the ≥4 coefficient because it includes a sample composition effect, i.e., there are more observations

from focus sectors picked in the earlier years than those picked in later years).

Overall, the results in this section are consistent with focus-sector firms making substantial disclosure changes in response to an increase in PFRE intensity.

Figure IA4: Pattern of Financial Statement Length around PFRE (in Event Time)



Notes: This figure displays OLS regression coefficient estimates and two-tailed 90% confidence intervals based on standard errors clustered at the industry level for the analysis of changes in financial statement length around PFRE. We map out the estimated counterfactual treatment effects in event time. We include, in one regression, indicators for all years relative to FPRE except one year prior to PFRE, which serves as the benchmark period (i.e., the coefficient is constrained to equal zero). We provide a detailed description of the variables in Section IA3.

Table IA4: Increased PFRE Intensity and Firm Disclosure

Panel A: Descriptive Statstics for Financial Statement Length Analyses

Variable	*Mean*	*Std. Dev*	*Median*
FS Length	15,452	7,897	13,867
Narrative Length	11,882	8,802	8,870

Panel B: Change in Financial Statement Length Around Increased Enforcement Intensity

	All Years		*First vs. Subsequent Years*	
Dependent Variable: FS Length	*Baseline*	*+ Selection Controls*	*Baseline*	*+ Selection Controls*
	(1)	*(2)*	*(3)*	*(4)*
Treatment effects:				
PFRE (t=1 to t=n)	580.504**	647.919**		
	(2.17)	(2.40)		
PFRE (t=1)			577.428**	618.370**
			(2.12)	(2.24)
PFRE (t=2 to t=n)			584.497*	688.885**
			(1.72)	(2.09)
Control variables:				
Narrative Length	0.531***	0.531***	0.531***	0.531***
	(30.06)	(30.06)	(30.04)	(30.06)
Prior Return		-643.234		-660.257
		(-1.38)		(-1.45)
Negative Media Mentions		-7.110		-6.933
		(-1.13)		(-1.09)
% Industry Restatements		-524.646		-540.248
		(-0.91)		(-0.94)
Fixed effects:				
Industry	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Observations	7,879	7,879	7,879	7,879
Adj. R-squared	0.567	0.567	0.567	0.567

Notes: This table reports the change in the number of words in the financial statements for firms in focus sectors relative to those in non-focus sectors. Panel A presents descriptive statistics and Panel B presents OLS regression coefficient estimates. The dependent variable is *FS Length*, which captures the word count of the financial statement component of the annual report. $PFRE_{(t=1\ to\ t=n)}$ is a binary indicator variable that takes the value of one for focus-sector firms in the first fiscal year-end following a focus-sector announcement and for all fiscal year-ends thereafter. $PFRE_{(t=1)}$ is a binary indicator variable that takes the value of one for focus-sector firms only in the first fiscal year-end following a focus-sector announcement. $PFRE_{(t=2\ to\ t=n)}$ is a binary indicator variable that takes the value of one for all focus-sector firms' fiscal year-ends after the initial announcement year and for all fiscal year-ends thereafter. Additional variable definitions are provided in Section IA3. We cluster standard errors by industry and report t-statistics in parentheses. All regressions include industry and year fixed effects. *, **, and *** indicate statistical significance (two-sided) at the 10%, 5%, and 1% levels, respectively.

Section IA5: Increased PFRE Intensity and Regulatory Compliance Costs

In this section, we examine changes in audit fees after a firm is subject to increased regulatory scrutiny by an FRRP focus-sector review. Greater enforcement likely creates an incentive to increase expenditures on reporting systems to ensure compliance with accounting regulations. For instance, additional expenditures might be incurred to deal with the immediate administrative burden associated with the increased regulatory scrutiny during FRRP inspection years (e.g., De George et al. 2012; Kim et al. 2012) and/or as a way to commit to an ongoing higher level of disclosure (e.g., Ball et al. 2012). We use audit fees, which are observable and likely positively associated with the quantity and quality of a wide range of firm-disclosure-related activities to proxy for these compliance costs (Iliev 2010).

We obtain data on audit fees for a panel of firm-year observations from 2000 through 2017 from *Worldscope*. Unlike the financial statement length analyses in Section IA4, for this sample we have sufficient observations to follow a constant sample of firms, which greatly improves our ability to reliably map out our treatment effects over our sample period. To do this, we require data to be available in 15 years out of the 18-year sample period—reflecting a subjective assessment of the trade-off between having a reasonably stable sample and the loss of observations.[33]

For this sample, we employ a difference-in-differences design that compares the change in audit fees for firms included in FRRP focus sectors to those that were not and estimate the following OLS regression:

$$Ln(Audit\ Fees)_{i,t} = \alpha_i + \beta_1 PFRE_{j,(t=1\ to\ t=n)} + \sum \gamma_i Controls_{i,t} + \sum \delta_i Fixed\ Effects + \varepsilon_{i,t}$$

Ln(Audit Fees) is the natural log of audit fees measured in millions of USD. $PFRE_{j,(t=1\ to\ t=n)}$, the variable of interest, is an indicator coded as one for focus-sector firms beginning in the first fiscal year the firm's financials are subject to FRRP review and for all subsequent years, and zero otherwise. Following prior research (e.g., Craswell et al. 1995; Beck and Mauldin 2014; Huang et al. 2014), we control for firm characteristics known to be determinants of the level of audit fees, which we define in Section IA3. We also include firm and year fixed effects to control for static differences in audit fees across firms and to flexibly account for changes in audit fees over time. We cluster standard errors by industry.

Table IA5 Panel A presents descriptive statistics. The median firm pays audit fees of $1.2 million per year and has $1.5 billion in total assets. Table IA5 Panel B presents regression results. The coefficients on the control variables are consistent with prior research (e.g., Chaney et al. 2004, Choi et al. 2009, and Iliev 2010)—indicating that, for example, audit fees are higher for larger firms with lower profitability. The coefficient of interest, $PFRE_{(t=1\ to\ t=n)}$, is positive and statistically significant (p-value<0.05), indicating that, relative to firms not inspected by the FRRP, focus-

[33] The results are similar using alternative cutoffs between 10 and 18 observations.

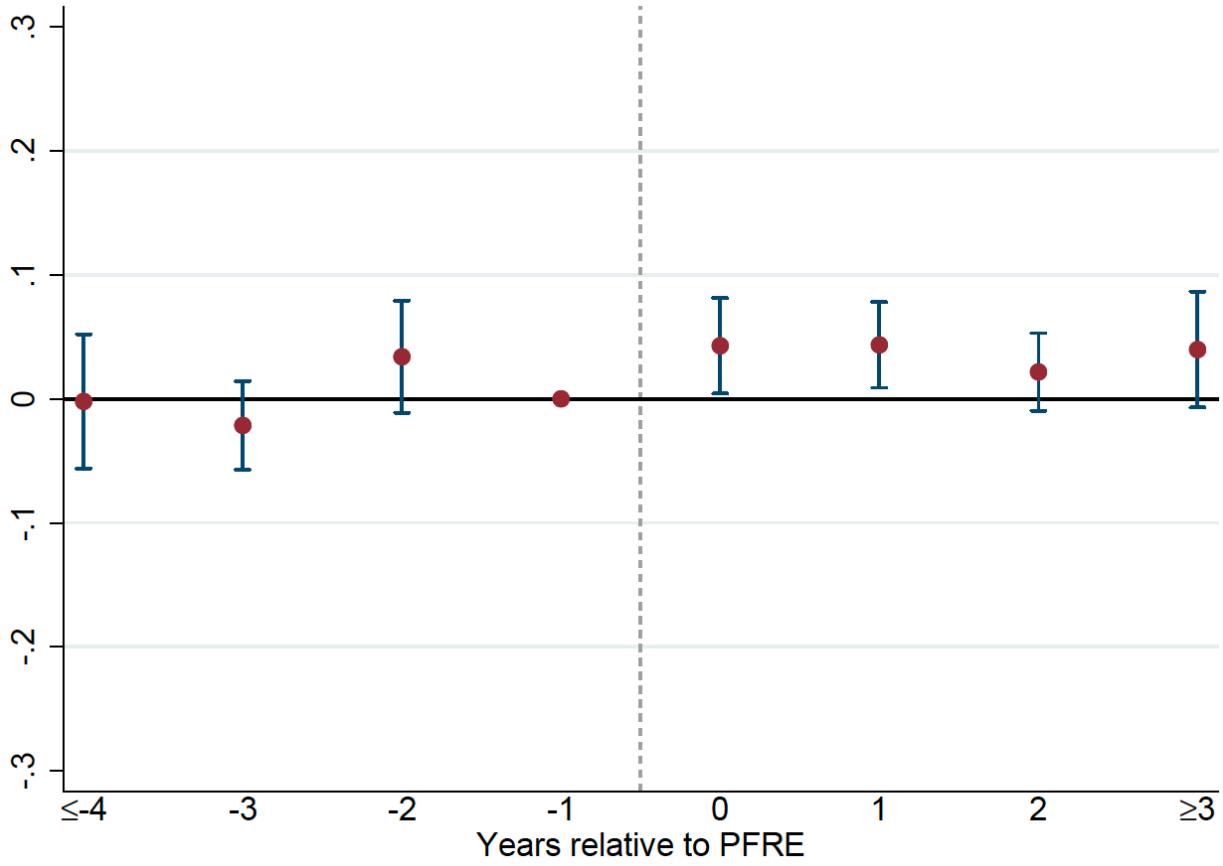
sector firms experience a 6.7% increase in audit fees (approximately $150K). This result is broadly consistent with Florou et al. (2019), who also examine changes in audit fees following FRRP focus-sector announcements.

Next, we address the possibility that the observed increase in audit fees could be attributable to poor firm performance in the year prior to focus-sector selection, in Column (2) of Table IA5 Panel B, we control for the determinants in the focus-sector-selection model in Table 3 of the Manuscript (i.e., *Prior Return*, *Negative Media Mentions*, and *% Industry Restatements*). The magnitude of the coefficient on $PFRE_{(t=1\ to\ t=n)}$ is almost unchanged by including these controls and thus provides no evidence that the increase in audit fees is attributable to an endogenous response to the prior year's performance. We also test whether the observed increase in audit fees persists beyond the year of the increase in enforcement scrutiny. We partition $PFRE_{(t=1\ to\ t=n)}$ into two separate indicators, $PFRE_{(t=1)}$ and $PFRE_{(t=2\ to\ t=n)}$,. Inconsistent with the increase in audit fees being (solely) attributable to an endogenous response to the prior year's performance, the results reported in Column (3) of Table IA5 Panel B indicate that audit fees remain persistently higher in years subsequent to the FRRP focus-sector reviews. Results in Column (4) suggest that the inclusion of the selection model controls has little effect on the estimated coefficients.

To assess the validity of the parallel-trends assumption and illustrate the persistence of the increase in audit fees following PFRE, in Figure 1, we plot the estimated counterfactual treatment effects in event time for the entire sample period by including separate indicators for all years before and after PFRE, except *t-1*, which we use as the benchmark. The estimated effects are close to zero during the four-year period leading up to PFRE. There is a relatively sharp increase in audit fees after PFRE. In year *t+1* and beyond, the estimated effect is not statistically significant for all years but is above zero throughout the sample period.

Overall, the results in this section are consistent with focus-sector firms incurring significant additional compliance costs in response to an increase in PFRE intensity.

Figure IA5: Pattern of Audit Fees around PFRE (in Event Time)



Notes: This figure displays OLS regression coefficient estimates and two-tailed 90% confidence intervals based on standard errors clustered at the industry level for our analysis of change in audit fees around PFRE. We map out the estimated counterfactual treatment effects in event time. We include, in one regression, indicators for all years relative to FPRE except one year prior to PFRE, which serves as the benchmark period (i.e., the coefficient is constrained to equal zero). We provide a detailed description of the variables in Section IA3.

Table IA5: Increased PFRE Intensity and Regulatory Compliance Costs

Panel A: Descriptive Statstics for Audit Fee Analysis

Variable	*Mean*	*Std. Dev*	*Median*
Audit Fee (mil.)	1.212	2.190	0.384
Total Assets (mil.)	1,480.409	3,956.996	188.272
Leverage	0.521	0.284	0.511
ROA	-0.010	0.261	0.042
Current Ratio	2.041	2.567	1.421
Foreign-Sales Ratio	0.358	0.353	0.262
Loss	0.256	0.436	0.000
Cash Flow	0.050	0.184	0.077
BigN Auditor	0.591	0.492	1.000
Working Capital	0.296	0.197	0.274
Restatement	0.302	0.459	0.000

(continued)

Table IA5 continued

Panel B: Change in Audit Fees around Increased Enforcement Intensity

	All Years		*First vs. Subsequent Years*	
Dependent Variable: Ln(Audit Fee)	*Baseline*	*+ Selection*	*Baseline*	*+ Selection*
	(1)	*(2)*	*(3)*	*(4)*
Treatment Effects:				
PFRE (t=1 to t=n)	0.065**	0.066***		
	(2.56)	(2.65)		
PFRE (t=1)			0.075***	0.074***
			(3.03)	(3.04)
PFRE (t=2 to t=n)			0.058*	0.061**
			(1.92)	(2.03)
Control Variables:				
Ln(Total Assets)	0.552***	0.552***	0.552***	0.552***
	(29.57)	(29.37)	(29.43)	(29.29)
Leverage	0.053	0.053	0.053	0.053
	(1.31)	(1.34)	(1.31)	(1.33)
ROA	-0.248***	-0.247***	-0.248***	-0.247***
	(-6.20)	(-6.16)	(-6.21)	(-6.17)
Current Ratio	-0.010**	-0.010**	-0.010**	-0.010**
	(-2.22)	(-2.22)	(-2.21)	(-2.21)
Foreign-Sales Ratio	0.167***	0.167***	0.167***	0.167***
	(3.40)	(3.39)	(3.40)	(3.39)
Loss	0.054***	0.054***	0.054***	0.054***
	(2.87)	(2.84)	(2.85)	(2.83)
Cash Flow	-0.196***	-0.196***	-0.196***	-0.196***
	(-3.06)	(-3.07)	(-3.05)	(-3.06)
BigN Auditor	0.146***	0.147***	0.146***	0.147***
	(5.26)	(5.28)	(5.27)	(5.28)
Working Capital	0.335***	0.336***	0.336***	0.336***
	(2.80)	(2.80)	(2.81)	(2.80)
Restatement	0.028*	0.030*	0.028*	0.030*
	(1.75)	(1.81)	(1.75)	(1.81)
Prior Return		-0.012		-0.010
		(-0.30)		(-0.26)
Negative Media Mentions		0.001		0.001
		(1.26)		(1.21)
% Industry Restatements		-0.052		-0.050
		(-0.94)		(-0.92)
Fixed effects:				
Firm	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Observations	8,214	8,214	8,214	8,214
Adjusted R-squared	0.911	0.911	0.911	0.911

(continued)

Table IA5 continued

Notes: This table reports results from our analysis of the effect of increased PFRE intensity on regulatory compliance costs, which we proxy for using audit fees. The sample period is from 2000 to 2017. Panel A presents descriptive statistics and Panel B presents OLS regression estimates. The dependent variable is the natural log of total annual audit fees in millions of USD. $PFRE_{(t=1 \text{ to } t=n)}$ is a binary indicator variable that takes the value of one for focus-sector firms in the first fiscal year-end following a focus-sector announcement and for all fiscal year-ends thereafter. $PFRE_{(t=1)}$ is a binary indicator variable that takes the value of one for focus-sector firms only in the first fiscal year-end following a focus-sector announcement. $PFRE_{(t=2 \text{ to } t=n)}$ is a binary indicator variable that takes the value of one for all focus-sector firms' fiscal year-ends after the initial announcement year and for all fiscal year-ends thereafter. Additional variable definitions are provided in Section IA3. We cluster standard errors by industry and report t-statistics in parentheses. All regressions include industry and year fixed effects. *, **, and *** indicate statistical significance (two-sided) at the 10%, 5%, and 1% levels, respectively.

Section IA6: Increased PFRE Intensity and Stock Market Liquidity

In this section, in order to compare the effect of PFRE to prior studies that document an increase in transparency following increases in securities regulation enforcement (see Leuz and Wysocki 2016 for a review), we examine the effect of increased PFRE intensity on stock market liquidity. Because focus sectors have worse performance (in the year prior to the focus-sector announcement) and performance is likely correlated with liquidity, we cannot estimate the impact of PFRE on liquidity by simply comparing focus-sector to non-focus-sector firms. Instead, we limit the sample to focus-sector firms the first calendar year after the FRRP announcements and employ a within-industry difference-in-differences design that identifies the change in liquidity based on variation in firms' fiscal year-ends:

$$Zero\ Returns_{i,t} = \alpha_i + \beta_1 PFRE_{j,(t=1)} + \sum \gamma_i Controls_{i,t} + \sum \delta_i Fixed\ Effects + \varepsilon_{i,t}$$

As a proxy for liquidity, we use the proportion of zero-return trading days, *Zero Returns*, which is available for most of the firms in our sample and is commonly used as a proxy for liquidity in prior research (e.g., Lesmond et al., 1999; Bekaert et al., 2007). We include controls for the *Market Value* (of equity), *Share Turnover*, and *Return Variability* (e.g., Chordia, et al. 2000; Leuz and Verrecchia 2000). We include fixed effects for industry×year×quarter (to control for the average level of liquidity within a particular industry, year, and quarter and to isolate the variation in liquidity within focus sectors based only on differences in the firms' fiscal year-ends) and firm (to control for static firm-level differences in liquidity). Accordingly, this design controls for the possibility that liquidity changes for other reasons besides the increase in PFRE intensity by exploiting the fact that any liquidity effect related to the increased disclosures following the increase in PFRE intensity should occur after the disclosure of firms' financial statements and that the timing of these disclosures varies across firms within a given focus sector based on firms' fiscal year-ends (Daske et al. 2008).

Panel A of Table IA6 presents descriptive statistics and Panel B presents OLS regression coefficient estimates. Consistent with an increase in transparency (i.e., a reduction in zero returns), the coefficient on PFRE is 0.016, which translates into an increase in liquidity of approximately 5%. While the economic magnitude of the effect is somewhat smaller than the effects estimated in prior research (e.g., Christensen et al. 2013, 2016), it is important to recognize that we estimate the effect for a change in the intensity of proactive enforcement (i.e., the announcement of specific focus sectors) rather than the effect of the entire regime change (i.e., the initiation of the FRRP proactive review program).

Table IA6: Increased PFRE Intensity and Stock Market Liquidity

Panel A: Descriptive Statstics for Liquidity Analyses

Variable	*Mean*	*Std. Dev.*	*Median*
Zero Returns	0.368	0.300	0.375
ln(Market Value)	1.688	2.409	1.502
ln(Share Turnover)	-3.572	2.171	-3.824
ln(Return Variability)	-3.635	0.745	-3.682

Panel B: Change in Liquidity Around Increased Enforcement Intensity

Dependent Variable: Zero Returns	
Treatment effect:	
PFRE	-0.016**
	(-2.36)
Control variables:	
ln(Market Value$_{t-1}$)	-0.040***
	(-5.52)
ln(Share Turnover$_{t-1}$)	-0.002
	(-0.43)
ln(Return Variability$_{t-1}$)	-0.023***
	(-5.97)
Fixed effects:	
Industry×Year×Quarter	Yes
Firm	Yes
Observations	7,545
Adj. R-squared	0.835

Notes: This table reports results from our analysis of the effect increased PFRE intensity on stock market liquidity. The sample period is from 2005 to 2012. Panel A presents descriptive statistics for firms included in the analyses and Panel B presents OLS regression estimates. The dependent variable is *Zero Returns*, the proportion of trading days with zero daily stock returns out of all potential trading days in a given quarter. The variable of interest, *PFRE*, is a binary indicator that takes the value of one for focus-sector firms beginning one quarter after the fiscal year-end quarter following a focus-sector announcement, and zero otherwise. Additional variable definitions are provided in Section IA3. We cluster standard errors by industry and report t-statistics in parentheses. The regression includes industry×year×quarter and firm fixed effects. *, **, and *** indicate statistical significance (two-sided) at the 10%, 5%, and 1% levels, respectively.

Section IA7: Changes in Information Asymmetry around the Focus-Sector Announcements

In this section, we examine changes in information asymmetry following the focus-sector announcements to address the potential concern that the negative returns observed around the focus-sector announcements are attributable to the revelation of private information rather than the expected costs of increased enforcement intensity. If there are traders who potentially have private information about the extent of firms' reporting deficiencies, market makers will address this adverse-selection problem by increasing bid-ask spreads. A public announcement that reveals some of this private information should reduce information asymmetry and lead to a decline in bid-ask spreads (e.g., Diamond and Verrecchia 1991). Analyzing spreads around management earnings forecasts, Coller and Yohn (1997) find evidence consistent with this prediction—forecasting firms experience a reduction in information asymmetry relative to non-forecasting firms after a management forecast. If the FRRP focus-sector announcements reveal some information regarding disclosure quality previously known only by relatively informed traders, this should lead to a decline in information asymmetry and a reduction in bid-ask spreads following the announcements.[34] To increase the likelihood that there are some investors that have private information about reporting quality, we limit our sample to firms with external blockholders.

To investigate this possibility, we examine changes in bid-ask spreads around the FRRP announcements for focus-sector relative to non-focus-sector firms. We use a panel regression structure similar to our main return specification in the Manuscript and estimate the following OLS regression:

$$\begin{aligned} ln(Spread)_{i,t} = {} & \beta_1 Spread\ Difference\ Pre\text{-}Announcement_{i,t} + \\ & \beta_2 Spread\ Difference\ Post\text{-}Announcement_{i,t} + \beta_3 ln(Market\ Value)_{i,t} + \\ & \beta_4 ln(Share\ Turnover)_{i,t} + \beta_5 ln(Return\ Variability)_{i,t} + \\ & \sum a_i Fixed\ Effects + e_{i,t} \end{aligned}$$

$ln(Spread)_{i,t}$ is the natural log of firm i's bid-ask spread on day t. $Spread\ Difference\ Pre\text{-}Announcement_{i,t}$ $(Post\text{-}Announcement_{i,t})$ is an indicator coded as one for focus-sector firms over the n days before (after) the indicated announcement window (where n is alternatively 20, 5 including the focus-sector announcement window, or 5 days excluding the focus-sector announcement window), and zero otherwise. For the 5-day window that excludes the focus-sector announcement window, to address the possibility of short-term increases in spreads immediately around the announcement (i.e., if some traders are superior processors of the information contained in the focus-sector announcements), we exclude the day before (-1), day of (0), and day after ($+1$)

[34] Theoretically, it is also possible that, if some traders have a superior ability to process public information, this could lead to a temporary information advantage—meaning that, in the short term, bid-ask spreads could increase around the focus-sector announcements (Kim and Verrecchia 1994). To abstract from any such effects, we examine spreads over windows of 20 and 5 days after the focus-sector announcements, during which it is likely that any such effects will have subsided. We also consider another window that excludes the 3-day announcement window.

the focus-sector announcement. Following prior studies (e.g., Coller and Yohn 1997; Acker et al. 2002), we control for the market value of equity (*ln(Market Value)*), daily trading volume divided by the market value (*ln(Share Turnover)*), and the square of daily stock return (*ln(Return Variability)*). We include firm and day fixed-effects to control for static firm-level differences in spreads (e.g., differences in microstructure across exchanges) and time-specific, market-wide shocks to liquidity. We cluster standard errors by day.

Table IA7 Panel A presents descriptive statistics. The median firm has a bid-ask spread of 1.8% of price (based on a *ln(Spread)* of -3.998). Table IA7 Panel B presents regression results. Inconsistent with the FRRP's focus-sector announcements revealing private information, across all three event windows, we find no evidence of significant changes in spreads around the focus-sector announcements. For example, in Column (1), in the 20 days leading up to the focus-sector announcements, the difference in *ln(Spread)* for focus-sector relative to non-focus-sector firms is -0.025. In the 20 days following the announcement the difference in spreads is -0.023, indicating that spreads changed by a statistically insignificant 0.002 (p-value=0.71) compared to non-focus-sector firms. Results are similar in the other announcement windows. Overall, the lack of changes in bid-ask spreads around the focus-sector announcements is inconsistent with the revelation of private information by the FRRP that is known by some market participants.

Importantly, the evidence presented in this Section does not address concerns about private information *possessed only by the FRRP*. Although we think it is unlikely that the FRRP has industry-level private information about industries that no other market participants are aware of, we nonetheless caution that the evidence must be interpreted with this caveat in mind.

Table IA7: Changes in Bid-Ask Spreads Around Focus-Sector Announcements

Panel A: Descriptive Statistics for Bid-Ask Spread Analysis

	Mean	*Std. Dev.*	*Median*
ln(Bid-Ask Spread)	-4.320	1.653	-3.998
ln(Market Value)	5.001	2.064	4.971
ln(Share Turnover)	-6.612	2.132	-6.762
ln(Return Variability)	-8.469	2.170	-8.317

Panel B: Change in Bid-Ask Spread Around Focus-Sector Announcements

Dependent Variable: ln(Bid-Ask Spread)	*(-20, +20)*	*(-5, +5)*	*(-7 to -2, +2 to +7)*
	(1)	*(2)*	*(3)*
Spread Difference Pre-Announcement	-0.025***	-0.018	-0.019*
	(-4.41)	(-1.51)	(-1.85)
Spread Difference Post-Announcement	-0.023***	-0.021**	-0.022**
	(-4.44)	(-2.23)	(-2.29)
Test of difference in coefficients (p-value):			
Pre vs. Post	0.71	0.88	0.87
Control variables:			
ln(Market Value)	-0.616***	-0.616***	-0.616***
	(-427.81)	(-427.60)	(-427.61)
ln(Share Turnover)	-0.080***	-0.081***	-0.080***
	(-97.81)	(-97.78)	(-97.78)
ln(Return Variability)	0.037***	0.037***	0.037***
	(80.34)	(80.34)	(80.33)
Fixed effects:			
Firm	Yes	Yes	Yes
Day	Yes	Yes	Yes
Observations	1,690,544	1,690,544	1,690,544
Adj. R-squared	0.812	0.812	0.812

Notes: This table reports results from our analysis of changes in bid-ask spreads around focus-sector announcements. The sample period is from 2004 to 2012. Panel A presents descriptive statistics and Panel B presents OLS regression estimates of the effect of PFRE on bid-ask spreads. The dependent variable, *log(Bid-Ask Spread)*, is the natural log of firm *i*'s bid-ask spread on day *t*, where the bid-ask spread is the daily quoted percentage spread (from Datastream), calculated as the difference between the ask and the bid prices divided by the mid-point (i.e., (Ask-Bid)/((Ask+Bid)/2)). The variables of interest, *Spread Difference Pre-Announcement (Post-Announcement)* are binary indicator variables coded as one for focus-sector firms over the n days before (after) the indicated announcement window, and zero otherwise. Additional variable definitions are provided in Section IA3. In Columns (1) to (3), we use announcement windows of 20, 5 days including the announcement window, and 5 days excluding the announcement window, respectively. For the 5-day window that excludes the focus-sector announcement window, we exclude the day before (*-1*), day of (*0*), and day after (*-1*) the focus-sector announcement. We cluster standard errors by day and report t-statistics in parentheses. All regressions include firm and day fixed effects. *, **, and *** indicate statistical significance (two-sided) at the 10%, 5%, and 1% levels, respectively.

Section IA8: Labor Market Consequences of Focus-sector Selection

In this section, we examine the effect of focus-sector selection on audit-committee turnover to address the potential concern that the negative returns observed around the focus-sector announcements are attributable to the revelation of private information rather than the expected costs of increased enforcement intensity. If the focus-sector announcements convey news about the quality of financial reporting to investors, this revelation should have labor market consequences for those responsible for overseeing firms' financial reporting. We test this prediction by examining changes in the rate of audit committee turnover following PFRE.

The fact that it is costly to change audit committee members reduces the power of this test. Thus, to provide a benchmark, we compare the estimated effect of PFRE to that of a restatement, which allows us to assess the magnitude of the information conveyed by PFRE relative to the information conveyed by a restatement. Specifically, to examine the association between PFRE and audit committee turnover, we estimate the following OLS regression:

$$AC\ Turnover_{i,t+1} = \beta_1 PFRE_{i,t} + \beta_2 Restatement_{i,t} + \sum \gamma_i Controls_{i,t} + Fixed\ Effects + \varepsilon_{i,t}$$

$AC\ Turnover_{i,t+1}$ is an indicator variable equal to one if any audit committee member leaves firm *i* in year *t+1*, and zero otherwise. $PFRE_{i,t}$ is an indicator variable that equals one if firm *i* is in a selected focus sector in year *t*, and zero otherwise. $Restatement_{i,t}$ is an indicator for whether firm *i* issues an accounting restatement in year *t*. To control for changes in firm performance that could also lead to greater audit committee turnover, we include stock returns over year *t* ($Prior\ Return_{i,t}$). We also include industry and year fixed effects and cluster standard errors by industry.

Panel A of Table IA8 reports descriptive statistics for our audit committee sample, which we obtain from *Board-Ex*. On average, in 25.6% of firm years at least one audit committee member leaves—the relatively high frequency suggests that the cost of changing members of an audit committee is fairly low. Panel B of Table IA8 reports regression results. We find no evidence that focus-sector firms have significantly higher audit committee turnover relative to non-focus-sector firms (i.e., the *PFRE* coefficient is 0.016 and statistically insignificant). In contrast, the coefficients on *Restatement* and *Prior Return* are both statistically significant (at least) at the 5% level. Our results indicate that an accounting restatement increases the likelihood of the turnover of an audit committee member by 4.6%. The coefficient on *PFRE* and *Restatement* are significantly different at the 5% level (p-value=0.03). Thus, we can conclude that the focus-sector announcements at least have less impact on audit-committee turnover and thus likely convey less information about financial reporting quality than a restatement.

Table IA8: Audit Committee Turnover after Focus-Sector Announcements

Panel A: Descriptive Statistics for Audit Committee Turnover Analyses

	Mean	*Std. Dev.*	*Median*
AC Turnover	0.256	0.437	0.000
Restatement	0.356	0.479	0.000
Prior Return	0.593	2.124	0.233

Panel B: Audit Committee Turnover After Increased Enforcement Intensity

Dependent Variable: AC Turnover	
PFRE	0.016
	(1.39)
Restatement	0.046***
	(4.76)
Prior Return	-0.004**
	(-2.01)
Test of difference in coefficients (p-value):	
PFRE vs. Restatement	0.029
Fixed Effects:	
Industry	Yes
Year	Yes
Observations	10,423
Adj. R-squared	0.017

Notes: This table reports results from our analysis of audit committee turnover after the FRRP focus-sector announcements. The sample period is from 2004 to 2012. Panel A presents descriptive statistics and Panel B presents OLS regression estimates. The dependent variable is *AC Turnover*, which is an indicator coded as one if any audit committee member leaves firm *i* in year *t+1*, and zero otherwise. The variable of interest, *PFRE* is a binary indicator variable that equals one if firm *i* is in a selected focus sector in year *t*, and zero otherwise. Additional variable definitions are provided in Section IA3. We cluster standard errors by industry and report t-statistics in parentheses. All regressions include industry and year fixed effects. *, **, and *** indicate statistical significance (two-sided) at the 10%, 5%, and 1% levels, respectively.

Section IA9: AIM vs. Main Market and the Role of Firm Size

In Table 6 of the Manuscript, we show that the market reactions to the FRRP focus-sector announcements are more negative for firms listed on AIM than for firms listed on the Main Market. Because, AIM firms are significantly smaller than firms listed on the Main Market—with an average (median) market capitalization (as of 2004) of $40 million ($22 million) and $2.2 billion ($312 million), respectively—the stronger market reactions for AIM firms could be attributable to a size effect (e.g., if additional enforcement imposes high fixed costs). In Table IA9 Columns (1) through (4) we partition the sample based on median size for each exchange. Columns (1) and (2) show that, inconsistent with a size effect, returns for above (*AIM-big*) and below (*AIM-small*) median AIM firms are very similar (-42.95 and -46.72 basis points, respectively) and statistically indistinguishable (p-value 0.94). Results for partitions based on Main Market firm size in Columns (3) and (4) are also statistically indistinguishable (p-value 0.57), although only the return for small Main Market firms (*MM-small*) is statistically significant (-17.06 basis points).

Although the within-exchange comparisons are inconsistent with a purely size-based explanation, large (i.e., above median market capitalization) AIM firms are still much smaller, and experience a significantly larger reduction in equity value, than small (i.e., below median market capitalization) Main Market firms. To facilitate a more direct comparison of similar-size firms, we match *AIM-big* to *MM-small* firms (within a particular year) based on market capitalization using coarsened exact matching (CEM). We coarsen our sample into 100 bins, which reflects a tradeoff between preserving observations and the ex-post similarity of the distribution of size across the two groups. We then use the resulting CEM weights in estimations of our main specification. Prior to CEM matching, "big" AIM firms have an average market capitalization of about $100 million compared to $200 million for a "small" Main Market firm. After applying the CEM weights, the average size is approximately $84 million (and statistically indistinguishable) for both groups.

Columns (5) and (6) of Table IA8 present results for our main specification for the CEM-matched sample of AIM and MM firms, respectively. The FRRP announcement return for the AIM market is -41.78 basis points (p-value<0.01). For the Main Market the announcement return is a statistically insignificant 3.05 basis points. Inconsistent with the larger decline in shareholder wealth on the less heavily regulated AIM market being driven by differences in size, the difference between the two virtually identically sized groups is economically large (44.83 basis points) and statistically significant (p-value<0.10). Overall, these results suggest that additional enforcement is more costly for firms with less public oversight.

Table IA9: Increased PFRE Intensity and Shareholder Wealth Conditional on an AIM vs. Main Market Listing and Firm Size

Dependent Variable: *Abnormal Return (UK & US Ind.-adj.)*	*MM, AIM, and Market Capitalization*				*CEM Match (Mrkt. Cap.)*	
	AIM-big	*AIM-small*	*MM-big*	*MM-small*	*AIM*	*MM*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Announcement	-42.95***	-46.72***	-8.76	-17.06*	-41.78***	3.05
	(-2.82)	(-4.10)	(-0.62)	(-1.79)	(-2.66)	(0.15)
Statistical differences:						
AIM=MM (Big vs. Small)	0.94		0.57		0.08	
Fixed effects:						
Day	Yes	Yes	Yes	Yes	Yes	Yes
Observations	408,326	408,682	438,170	438,495	399,021	382,374
Adj. R-squared	0.117	0.108	0.249	0.144	0.119	0.154

Notes: This table reports results from our analysis of the effect of increased PFRE intensity on shareholder wealth conditional on an AIM market listing and firm size. The sample period is from 2004 to 2012. The variable of interest, *Announcement*, is a binary indicator that takes on the value of one if day *t* is part of the (-2,+2) event window and firm *i* is in an announced focus sector, and zero otherwise. In Columns (1)-(4), we partition the sample based on whether a firm in on the AIM or Main Market segment of the London Stock Exchange and also based on whether a firm is above the median market capitalization for firms listed on that exchange at the beginning of the year. In Columns (5) and (6) we report a comparison of FRRP-announcement returns where firm on the Main Market have been matched to firms on the AIM using coarsened exact matching. We cluster standard errors by day and report t-statistics in parentheses. All regressions include day fixed effects. *, **, and *** indicate statistical significance (two-sided) at the 10%, 5%, and 1% levels, respectively.

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