# Mutual fund transparency and corporate myopia

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**Abstract:** Pressure from institutional money managers to generate profits in the short run is often blamed for corporate myopia. Theoretical research suggests that money managers' short term focus stems from their career concerns and greater fund transparency can amplify these concerns. Using a difference-in-differences design around a regulatory shock that increased transparency about fund managers' portfolio choices, we examine whether increased transparency encourages myopic corporate investment behavior. We find that corporate innovation declines following the regulatory shock. Moreover, evidence from mutual fund trading behavior corroborates that these results are driven by increased short-term focus of money managers.

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**Mutual fund transparency and corporate myopia**

## 1. Introduction

Innovation is critical for business success and economic development across the globe. Numerous anecdotes and research-based evidence suggest that pressure from institutional investors to report good short run financial performance can hinder investment in innovative projects that hurt short-term profits but generate value in the long run.[1] However, what incentivizes institutional investors to put excessive focus on short-run results? Are there features of a country's regulatory and institutional environment that contribute to this impatience?

We explore the role of mandated frequent disclosures of portfolio holdings by mutual fund managers in shaping their emphasis on short-term corporate performance and the concomitant myopic underinvestment in innovative activities by investee firms' managers. Our focus on portfolio disclosures is motivated by prior research that suggests that money managers' short-term focus stems from their career concerns (e.g., Shleifer and Vishny, 1990) and that greater transparency about their portfolio choices can amplify these concerns. The underlying premise is that fund investors are unsure about the fund managers' stock picking ability and learn about it gradually over time. For fund managers of uncertain ability, promises of returns far in the future cannot justify the authority to manage a large portfolio or high compensation. Therefore, fund managers have incentives to demonstrate their ability early on by reporting superior investment choices in the short run. A fund manager who is more tolerant of innovative, long-run corporate investments, risks fund outflows (and possible job termination) as fund

[1] In their influential survey, Graham, Harvey, and Rajgopal (2005) find 78% of corporate managers admitting to sacrificing economic value to achieve quarterly earnings targets and they consider pressure from institutional investors as one of the main reasons for this myopic behavior. Bushee (1998) finds that managers cut down on R&D in the presence of transient institutional investors. Fang, Tian, and Tice (2014) find that firms file fewer patents due to increased trading by transient investors following increases in stock liquidity. Bernstein (2015) and Asker, Farre-Mensa, and Ljungqvist (2015) find that public firms produce lower quality innovations and reduce capital expenditures, consistent with relatively less liquid, long-term private ownership being more conducive for investment in long-term projects. In support of the academic evidence, many reports from the business community and regulators highlight corporate myopia as a widespread problem and consider short-termist pressure from money managers as an important driver of this problem (e.g., Barton and Wiseman (2014); Aspen Institute's (2009) report on short-termism; European Union's Green paper (2011) on corporate governance; Kay report (2012) instituted by the UK government).

investors may mistakenly attribute investment stock picks with inferior short-term stock returns to low ability and poor stock selection. Thus, career concerns reduce fund managers' willingness to "ride-out" the declines in short-term performance of investee firms.

Theoretical work (e.g., Prat, 2005; Hermalin and Weisbach, 2012; Gigler et al., 2014; Edmans, Heinle, and Huang, 2016) posits that greater disclosure that facilitates tighter monitoring of fund managers' actions can amplify their career concerns and consequently their short-termist behavior. The intuition is that the agent (fund manager) is aware that the principal (fund investor) is closely scrutinizing his actions. This incentivizes the agent to take actions that are viewed to indicate high ability even if his private information suggests some other action is optimal. Thus, as fund investors gain access to more frequent and reliable information on fund managers' stock picks, fund managers may focus excessively on holding stocks that appear as "winners" in these short-term disclosures. Consequently, frequent portfolio disclosures can reduce the fund manager's tolerance for having investee firms in the portfolio that pursue policies that will generate value in the long run, but can appear as "poor" stock picks in short-term portfolio disclosures.  This increased short-term focus of fund managers can create pressure on managers of investee firms to behave myopically.[2]

Considerable anecdotal evidence exists in support of the above arguments. The following quote from a money manager in Lakonishok et al. (1991) directly highlights how the intense pressure to show winning stocks in their quarterly portfolio disclosures can cause money managers to myopically ignore the future potential of some stocks:

> "Nobody wants to be caught showing last quarter's disasters.... You throw out the duds because you don't want to have to apologize for and defend a stock's presence to clients even though your investment judgment may be to hold."

[2] As discussed in detail in section 3.2, short-termism of fund managers can cause corporate managers to behave myopically either by directly intervening in corporate policies or indirectly through the threat of exiting the firms. We consider threat of exit as the more plausible mechanism in our context because, unlike other institutional investors such as hedge funds or pension funds, mutual funds are more known to affect firm policies by voting with their feet.

Furthermore, many commentators from the business and academic community as well as regulators have suggested that such short-term performance evaluation pressures faced by fund managers might lie at the root of myopic decision making at the investee firms.[3]

To examine whether transparency about portfolio choices of fund managers creates short-term pressure on corporate managers, we exploit a key securities market regulation in 2004 that altered the reporting frequency of portfolio holdings by mutual fund managers from semi-annual to quarterly reporting. This is a suitable setting for several reasons. First, consistent with some of the above arguments, the fund managers' association, Investment Company Institute (ICI), opposed this regulation (Tyle, 2001) stating, "[it] would focus undue attention on individual portfolio securities and could encourage a short-term investment perspective." Second, the regulation affects mutual funds, which represent an important class of portfolio managers with about half of US household investments being directed through mutual funds (ICI 2014 Fact Book). Finally, prior research shows that portfolio disclosures contain useful information (incremental to fund returns), and fund managers behave as if they are evaluated based on their stock picks and not just fund returns (see section 2 for more details).

We use a difference-in-differences (DiD) approach to test the effect of fund managers' portfolio disclosure frequency on the innovation of investee firms. We examine how the change in firm-level innovation (first difference) around the SEC regulation varies with the ownership levels of actively managed funds (second difference) that were forced to increase their disclosure frequency.

Using patent-based measures of firm-level innovation, we find evidence of a significantly larger decline in innovation following the regulation for firms with high fund ownership. The decline in innovation output is economically significant: our estimates suggest that firms with above-median

[3] While arguing how short-term focus of fund managers contribute to corporate short-termism, Porter (1992) in his influential piece notes: "Because [fund] managers are measured on their short-term performance, their investment goals understandably focus on the near-term appreciation of shares." See also European Union's Green paper (2011) on the corporate governance framework, Aspen Institute's (2009) report on short-termism, Kay report (2012) instituted by the UK government, and Mercer and IRRC institute's (2010) survey on investment horizons for arguments on how performance evaluation pressures faced by fund managers contribute to corporate short-termism.

ownership on average file nearly one fewer (citation-weighted) patent compared to firms with below-median ownership following the SEC regulation. We also find that the innovation output of high and low ownership firms moves similarly in periods prior to the regulation, supporting the parallel trends assumption that underlies the DiD design. This evidence is consistent with more frequent portfolio disclosures inhibiting innovation by increasing the capital market pressure on corporate managers.

A potential concern is that firms with high and low fund ownership are systematically different and these differences cause the trends in innovation output of these firms to diverge around the 2004 regulation for reasons unrelated to disclosure requirements. Findings from a battery of tests suggest that this is not the case. First, we absorb any differential trends in innovation output that might result from observable differences between treatment and control firms. We do so by including interaction terms for a variety of firm characteristics (including industry membership) with year indicators. Second, we conduct our DiD analysis using plausibly exogenous variation in treatment assignment obtained using S&P 500 index inclusion as an instrumental variable (IV) for fund ownership (Aghion, Reenen, and Zingales, 2013). By identifying variation in treatment assignment that is exogenous with respect to future innovation output, we further mitigate concerns that our results are due to differences in treatment and control firms that are predictive of trends in innovation. Finally, similar to the approach in Agarwal et al. (2015), we conduct placebo-like tests based on ownership by three control groups of institutional investors whose frequency of portfolio disclosures was unaffected by the regulation. To the extent any trends in innovation output (unrelated to the regulation) affect both the firms owned by mutual funds affected by the regulation and firms owned by placebo groups, change in innovation activity of firms owned by placebo groups serves to control for the effect of such trends. The three placebo groups are: (i) mutual funds that did not change the reporting frequency because they were already voluntarily reporting on a quarterly basis prior to the regulation, (ii) non-mutual fund institutions, and (iii) hedge funds. We find that the affected mutual fund ownership has an economically much larger effect on innovation than does ownership by any of the placebo groups.

Several patterns in the cross-sectional variation in the innovation decline corroborate our story. The innovation decline is greater when CEO welfare is more closely tied to stock price performance - such CEOs are more likely to yield to the pressure from short-term oriented fund managers to avoid their exit and the resultant decrease in stock price. The innovation decline is greater for firms with higher ownership by more career concerned younger fund managers who would have greater incentives to signal their ability by appearing to make smart portfolio choices. Finally, the decline is steeper for innovations that are likely to yield more radical business opportunities whose profit potential may take longer to realize and would therefore be more vulnerable to myopic pressures.

In our final set of analyses, we buttress our story by exploring the trading behavior of fund managers and fund investors. First, we directly test our underlying premise that the SEC regulation would make it more difficult for fund managers to make long-term bets because of increased concerns about adverse reactions from fund investors following disclosure of losing stocks in their portfolios. We find that mutual fund outflows are relatively more sensitive to the presence of "losing" stocks in fund portfolios after the SEC regulation. Second, we examine whether fund managers' increased short-termism also manifests in their observable trading behavior. We find that fund managers who are forced by the SEC mandate to increase the reporting frequency exhibit (i) higher portfolio churn rates and shorter holding periods, (ii) increased sensitivity of mutual fund holdings to firm performance, and (iii) reduced portfolio allocation towards highly innovative firms that generate patents with higher citation counts. Together, these findings further support the interpretation that the decline in corporate innovation is a manifestation of increased short-term focus of fund managers.

Our paper furthers our understanding of drivers of corporate myopic behavior. While prior research suggests that the short-term focus of institutional money managers is a key driver of corporate myopic behavior, little work exists on what incentivizes money managers to focus on short-term performance in the first place. Our results highlight the role of mandated quarterly portfolio disclosures in shaping such incentives and lend support to the argument in Shleifer and Vishny (1990) that it is the money managers' career concerns and performance evaluation pressures that lie at the root of their short-term focus that in turn promotes corporate myopic behavior. More broadly, our findings enhance our understanding of the consequences of agency conflicts between money managers and fund investors on the real economy – a phenomenon that has become increasingly important to understand because of the astounding growth in delegated money management over the last three decades.[4]

Finally, this paper adds to our understanding of the consequences of increased mandated information disclosure. Much of the prior work in this area examines this issue in the context of agency problems between corporate managers and capital providers, and generally concludes that mandated disclosure benefits by resolving information asymmetries and constraining managerial misbehavior (e.g., Leuz and Verrecchia, 2000; Greenstone, Oyer, and Vissing-Jorgensen, 2006). A notable exception is Kraft, Vashishtha, and Venkatachalam (2017) who find that requiring corporates to provide accounting reports quarterly (as opposed to semi-annually or annually) causes managers to make inefficient myopic investment choices. We extend this literature by examining the role of increased mandated disclosure on the agency relation between funds' managers and investors and by highlighting how it can also create adverse real effects by distorting the incentives of career-concerned agents. The findings from our and Kraft et al. (2017) study echo the theme in Bond, Edmans, and Goldstein (2012) that greater informational efficiency does not necessarily translate into real efficiency.

## 2. The role of portfolio holdings disclosure in fund manager evaluation

Disclosures of portfolio holdings by institutional money managers was first mandated by the SEC under the Investment Company Act of 1940 to allow investors to better understand, monitor, and make informed asset allocation decisions. The SEC amended the portfolio holdings disclosure requirements in May 2004 to increase the frequency from semi-annual reporting to quarterly reporting.

[4] Fu and Huang (2016) note that the median institutional ownership of CRSP stocks has increased from less than 10% in 1980 to 60% by the end of 2012.

The regulation was triggered by concerns that semi-annual disclosures provided information with limited usefulness because these disclosures were stale and could be easily window dressed by fund managers.[5] The regulation was therefore expected to facilitate closer monitoring of fund managers by providing more timely and reliable information about stocks picks that was more representative of fund managers' actions during the quarter. As investors start relying more on portfolio disclosures because of their increased usefulness, we would expect fund managers to become more concerned about the adverse consequences of having stocks that might appear as losers to fund investors.

Our analysis rests on the premise that the provision of timelier portfolio disclosures would cause an economically significant change in fund managers' incentives. While ultimately this is an empirical question, *ex ante*, it is reasonable to expect the effect of the SEC regulation to be non-trivial. A large body of research shows that portfolio disclosures contain valuable information (incremental to fund returns) for evaluating fund managers and fund managers behave as if they are evaluated based on their specific stock picks and not just aggregate fund returns. This is because portfolio returns is a noisy proxy for fund managers' ability and a long time-series of returns is necessary to establish managers' ability with sufficient precision. Most funds typically do not have long time-series available.[6] A growing body of work has devised increasingly sophisticated ways to extract additional information from holdings data and shows that the informational gains from using holdings information can be substantial.[7] Furthermore, Agarwal, Gay, and Ling (2014) find that mutual fund flows respond to the incremental information in portfolio holdings over and above funds' reported returns, and research on window dressing of portfolio

[5] See https://www.sec.gov/rules/final/33-8393.htm for a detailed discussion of the rationale underlying the regulation.
[6] Kostovetsky and Warner (2015) find that the median survival period for managers is 2.4 years. Kothari and Warner (2001) find that pure returns-based approaches have little power to detect abnormal fund performance as large as 3%.
[7] For example, Wermers (2000) exploits holdings data to construct precise benchmarks and takes into account the effect of expenses. He finds that funds holds stocks that outperform the market by an economically large amount of 1.3% per year – a conclusion that stands in contrast to most prior work before his study that uses return-based approaches to conclude that funds are not good at picking stocks (e.g., Jensen, 1968; Gruber, 1996; and, Carhart, 1997). Similarly, Cohen, Coval, and Pástor (2005) show that funds' own holdings in conjunction with holdings of other funds can predict differences in future fund performance of 2.4% to 4.4% per year. See Wermers (2011) for a discussion of how portfolio disclosures add value and for a comprehensive discussion of the related literature.

holdings (e.g., Lakonishok et al., 1991) documents that fund managers behave as if they are evaluated on portfolio holdings.

## 3. Theoretical arguments

We first discuss how greater transparency about fund managers' portfolio choices can make them more short-term focused. We then discuss how increased short-termism of fund managers can in turn make corporate managers behave myopically to stifle innovation.

*3.1 How frequent portfolio disclosures can make fund managers myopic?*

Theoretical studies (Prat, 2005; Hermalin and Weisbach, 2012; Gigler et al., 2014; Edmans, Heinle, and Huang, 2016) suggest that the provision of more frequent/more precise disclosures that facilitate tighter monitoring of fund managers can motivate them to focus excessively on short-term performance. The broad intuition behind this result is best illustrated using the simple set-up in Hermalin and Weisbach (2012). Their model features a career-concerned agent with uncertain ability who can be fired if considered to be of low type by the principal. The principal learns about the agent's ability through publicly disclosed performance measures. An important role of the agent is to take actions (investment choices) that create value in the long run, but in the short run can expose the agent to the risk that short-term poor performance could be misconstrued as a sign of poor ability. Hermalin and Weisbach (2012) show that increasing the disclosure frequency or disclosure of more precise performance measures increases the reliance of the principal on these signals to learn about the agent's ability.[8] This, in turn, increases the agent's incentive to engage in myopic actions to boost short-term performance measures to favorably influence the principal's perception of her ability. Similar to Hermalin and Weisbach (2012), theoretical work by Gigler et al. (2014) and Edmans, Heinle, and Huang (2016) also shows that greater disclosure can amplify agent's incentive to myopically boost short-run performance at the cost of longer-run value.

[8] As explained in footnote 5 of Hermalin and Weisbach (2012), these results do not depend on the distinction between increased *quantity* (e.g., through more frequency) and *quality* of information.

A more direct evaluation of the detrimental effects of greater transparency through more frequent portfolio holdings disclosure is modeled in Prat (2005). Prat (2005) makes the distinction between transparency of actions (e.g., specific stock picks of fund managers) and transparency of output (e.g., aggregate fund returns). He shows that greater transparency about actions can incentivize agents to take actions (even if such actions are suboptimal) that conform to the behavior that is considered to indicate high ability by the principal. Such excessive conformism can make the fund manager myopic as she may focus more on being seen as taking actions that are considered to indicate high ability (such as not showing losing stocks in portfolio disclosures) and may ignore the potential of stocks that might "appear" as bad picks to fund investors based on poor quarterly performance.

In our specific setting of the 2004 SEC regulation, the above studies posit that the provision of more timely and reliable information about individual stock picks and their short-term quarterly performance following the SEC regulation can exacerbate fund managers' myopic tendencies. To elaborate, suppose that the fund manager has private information about a stock pick that she expects to generate great returns in the longer run, but can generate poor returns in the short run. Following the 2004 regulation in which fund investors have more timely/precise information about stock picks, fund manager would be hesitant to invest in this stock pick; this is because, fund investors assign greater weight to information from individual stock picks to draw inferences about fund manager's performance/ability. As such, the fund manager runs a greater risk that poor performance on this stock pick (as revealed in a quarterly portfolio disclosure) could be misconstrued as a sign of poor stock selection ability. In contrast, in a regime where fund investors have access to relatively stale information about fund manager's actions and have to primarily rely on aggregate fund performance to learn about the fund manager, the fund manager is more likely to get away with poor short-run performance on this stock pick without hurting perceptions about her ability. This is because even if the specific stock is performing poorly, the aggregate fund performance could still be at "acceptable" levels. Alternatively, even if the aggregate fund performance is poor, in the absence of sufficiently precise information about stock picks, fund investors may not be able to attribute poor fund performance to specific stock picks, in turn, reducing the pressure on fund managers to get rid of those stock picks. Overall, absence of timely/precise holdings disclosure shields the fund manager from the risk that she might be labelled as a bad type based on a premature evaluation of her longer-term bets over short-term, quarterly horizons.

*3.2 How fund manager short-termism can translate into corporate short-termism?*

In general, institutional money managers can affect corporate policies in two ways. They could directly intervene either through board representation or by putting pressure on the board behind the scene. Alternatively, they could affect corporate policies indirectly through the threat of exiting the firms resulting in a drop in share prices. Under this alternative mechanism, corporate managers who are sufficiently concerned about stock price yield to the preferences of short-term oriented institutional investors (even absent any direct intervention) to avoid their exit and the resultant decline in stock price.[9]

We consider threat of exit as the more plausible mechanism through which short-termism in fund managers translates into corporate myopia for two reasons. First, unlike other institutional investors such as hedge funds or pension funds, mutual funds are more known to affect firm policies indirectly by voting with their feet. Second, threat of exit is the implicit mechanism in almost all models of corporate myopia (e.g., Stein, 1989; Shleifer and Vishny, 1990; Edmans, 2009). In these models, corporate managers do not engage in myopic actions due to direct intervention of short-term investors in firm policies. Rather, they behave myopically because of the threat of decline in short-term stock price caused by the trading activity of investors who do not appreciate the long-run benefits of innovative investments and get misguided by poor current profits as a sign of poor future prospects.

Theory suggests that such a threat of decline in stock prices following investments in innovative, long-term projects increases as fund managers become more short-term oriented. This is because fund managers with shorter horizons will have less incentives to collect information about the long-term

[9] For evidence on how stock price concerns cause CEOs to cut investments, see Edmans, Fang, and Lewellen (2017).

prospects of a firm's R&D portfolio and might allocate more research effort towards predicting movements in quarterly earnings (Goldman and Slezak, 2003). Moreover, even if the fund managers are aware of the long-run prospects, they may not be willing to commit capital to stocks that they expect to appreciate in value only in the long run because of the increased motivation to show winning stocks over quarterly horizons. Therefore, reduced capital allocation by informed (but short-term focused) fund managers can perpetuate undervaluation over longer periods especially for firms that focus on long-term projects (Shleifer and Vishny, 1990). As a result, corporate managers who are sufficiently concerned about the decline in stock prices in the short run, become averse to making long-term oriented investments as fund managers become increasingly short-term focused.[10]

## 4. Sample construction, variable measurement, and empirical specification

To construct our sample, we focus on funds that increased their portfolio disclosure frequency following the SEC regulation in 2004. We begin with the sample in Agarwal et al. (2015) who follow a comprehensive approach to identify such funds by collecting portfolio disclosure dates from multiple data sources (SEC EDGAR, Morningstar, and Thomson Reuters S12).[11] We exclude index funds from the analysis as our hypothesis pertains to the effect of active investors. Our sample comprises of 1,459 actively managed mutual funds that were obligated to increase the disclosure frequency following the regulation change. These funds represent nearly 70% of the full sample of actively managed funds in

[10] An anecdote from Roberts (2014) illustrates the plausibility of the mechanism: *Once, during the late 1990s, executives with aerospace giant Lockheed Martin met with Wall Street stock analysts to show off the cutting-edge technologies the firm was preparing to invest in. No sooner was the presentation finished, recalls Norman Augustine, then CEO, than the analysts "literally ran out of the room . . . and sold our stock." Over the next four days Lockheed Martin's share price fell 11 percent. Stunned, Augustine phoned an analyst friend who had attended the presentation and asked him why the market had punished a technology company for investing in new technology. Augustine recounted the analyst's answer. "He said, 'First of all, it takes fifteen years for research to pay off, if it pays off at all. Second, your average shareholder owns your stock for eighteen months. Fifteen years from now, they'll probably own Boeing's stock, and they don't want you to have any good ideas. Furthermore, they don't want to pay for it.'*

[11] A key challenge in identifying funds affected by the regulation is that many funds voluntarily reported portfolio holdings at a frequency greater than that mandated by the SEC. Such voluntary disclosures can be made to multiple sources including SEC (on form N-30B2) and data vendors such as Morningstar and Thomson Reuters (formerly CDA/Spectrum). Agarwal et al. (2015) carefully separate voluntary quarterly reporters using disclosure dates from multiple data sources to identify funds that were forced to increase the disclosure frequency following the regulation.

Agarwal et al. (2015) and account for nearly 79% of the assets under management by active funds during the 5-year period prior to the regulatory shock. Next, we obtain the portfolio holdings data for these funds from Thomson Reuters S12 database to determine the stocks held by these funds. Our final sample consists of a maximum of 47,326 firm-year observations with data for required variables available for up to five years before and five years after the regulation (i.e., years 1999 to 2009).

*4.1 Measurement of innovation*

We measure a firm's innovation output using its patenting activity, which provides a superior measure of innovation compared to R&D expenditures. First, because patenting activity measures the output of the innovation process, it captures the combined effect of all innovation inputs including R&D, human capital, and other intangibles. In contrast, R&D represents the quantity of only one of the inputs in the innovation process and fails to differentiate between innovations of different quality. Second, R&D data in Compustat is missing for a large number (about 50%) of our sample firms. We use patent data collected by Kogan et al. (2012), which is available until year 2010.[12]

A firm can increase its innovation output by filing more patents (i.e., increase the quantity) or by filing patents that are more innovative and thus are likely to receive more citations (i.e., increase the quality). Our main measure is the citation-weighted patent count ($Citations_t$) of all patents applied by a firm in year $t$, which captures both the quality and quantity of innovation. We use a patent's application year instead of the grant year to better capture the actual timing of the innovation (Griliches, Pakes, and Hall, 1988). In additional analysis, we decompose $Citations_t$ into its two components and examine whether changes in innovation output are driven by changes in quantity (number of patent applications or $NumPat_t$) or changes in quality of innovation (citations per patent or $CiteperPat_t$). All patent measures are set to zero for firm-years with no patent filing activity in the patent database. Because patent and

[12] We thank the authors for making the data publicly available (https://iu.app.box.com/patents).

citation counts are highly skewed, we use the natural logarithm of one plus the innovation measure as the dependent variable in our empirical specifications.

Both patent volume and citations per patent measures are subject to truncation biases. There is a truncation bias in the number of patents toward the end of the sample because it takes an average of two years for a patent to be granted after the initial application. As a result, there may be some patent applications under review in later years in our sample. Citations per patent measure is truncated because patents granted in later years would mechanically have fewer years to collect citations. We follow the approaches suggested in Hall, Jaffe, and Trajtenberg (2001, 2005) to adjust for these truncation biases. We scale each patent with weights estimated from the empirical application-grant lag distribution (See Appendix for variable definitions). We adjust for the bias in citations per patent by dividing the citation count for each patent by the mean citation count for the grant-year cohort to which the patent belongs. We compute our main innovation measure (i.e., citation-weighted patent count) from truncation-adjusted patent counts and citations per patent. We also conduct a battery of additional robustness tests (available in the Internet Appendix) based on the approaches in Lerner and Seru (2015) and Dass, Nanda, and Xiao (2017) to ensure that our results are not driven by truncation biases. Without adjusting for truncation biases, our sample firms on average file 4.5 patents per year and each patent garners 6.25 future citations. Table 1 presents the descriptives for the final truncation bias adjusted variables used in our analyses. Evident from Table 1, after adjusting for truncation biases, the mean value for citation-weighted patent count (simple patent count) is 5.2 (4.6) and the mean value for citations per patent (after setting missing patent values to zero) is 0.31.

*4.2 Measurement of mutual fund ownership and control variables*

We obtain ownership data from the Thomson Reuters S12 database and compute mutual fund ownership as follows. First, for each firm-quarter observation, we compute the aggregate ownership of the 1,459 funds affected by the regulation as the sum of shares owned by these funds divided by the outstanding number of shares. If stock holdings are not available for a quarter, we use the holdings for the previous quarter; otherwise, we set the holdings to zero. We then define variable *MFOwn* for each firm-year as the average ownership of the mutual funds over the four quarters during the fiscal year.

Following prior work, we control for several firm and industry characteristics that can affect a firm's innovation. Our control variables are firm size, $Log(MVE)_{it}$, measured as the natural logarithm of the market value of equity; return on assets, $ROA_{it}$, measured as operating income before depreciation scaled by lagged assets; lagged and contemporaneous growth opportunities, $Q_{it}$ and $Q_{it-1}$, measured as the ratio of market to book value of assets; lagged cash, $Cash_{it-1}$, measured as cash scaled by assets; lagged leverage ratio, $Leverage_{it-1}$, measured as the book value of debt scaled by assets; lagged capital stock, $CapStock_{it-1}$, measured as net PP&E scaled by assets; lagged financing constraints, $KZindex_{it-1}$, measured as the value of Kaplan and Zingales (1997) index; lagged firm age, $log(Age)_{it-1}$, measured as the natural logarithm of one plus the number of years a firm appears in the CRSP database; lagged illiquidity, $Illiquidity_{it-1}$, measured as the annual average of the daily Amihud (2002) illiquidity measure; Herfindahl-Hirschman index (HHI), $Hindex_{it-1}$, measured at the SIC four-digit level using annual sales. For any potential non-linearity in the relation between HHI and innovation (Aghion et al., 2005), we include squared $Hindex_{it-1}$. We report descriptives for all control variables in Table 1.

*4.3 Difference-in-differences specification*

We implement the DiD approach by comparing the change in innovation around the May 2004 regulation of firms with high fund ownership (treatment firms) to the change in the innovation of firms with low fund ownership (control firms) using the following specification:

$$Log(Citations)_{i,t} = \alpha_i + \beta_t + \gamma Treat_i \times Post_t + \Gamma X + \varepsilon_{i,t} \quad (1)$$

where $Log(Citations)_{it}$ is the natural logarithm of one plus firm *i*'s citation-weighted patent count; $Post_t$ is an indicator variable that equals one for fiscal years subsequent to the passage of the regulation in May 2004, and zero otherwise; $Treat_i$ is an indicator for firms with high ownership by funds affected by the regulation; *X* is a vector of time-varying control variables described in section 4.2; and, $\alpha_i$ and $\beta_t$ are firm and time fixed effects, respectively.[13] In our estimation, we include data for up to 5 years after and before the regulation. We drop observations for fiscal years that partially span periods both before and after the regulation.

*Treat* is coded as one for firms with above-median average ownership (measured over the five years prior to the regulation) by affected funds, and zero otherwise. Table 1 shows that the mean affected fund ownership (*MFOwn*) for all sample firms is about 5.2% (3.4%). Treatment firms have an average (median) ownership of about 8% (7%) by affected funds in the years prior to the regulation compared to a mean (median) ownership of 0.8% (0.5%) for control firms. In a survey of institutional investors, McCahery, Sautner, and Starks (2016) find two-thirds of the investors mentioning that even 2% ownership can induce sufficiently large threat of exit to induce corporate managers into changing real policies. Given these findings, the above approach for defining the treatment firms should provide enough power to detect differential effects of the regulation on treatment and control firms. We later explore the robustness of our results to alternative methods for identifying treatment and control firms.

The primary coefficient of interest in the above specification is the coefficient $\gamma$ on the interaction term, *Treat* x *Post*, which measures the average change in innovation of high fund ownership firms (first difference) relative to the average change in innovation of low fund ownership firms (second difference). Essentially, $\gamma$ captures the DiD estimate of the impact of regulation on firms' innovation. The efficacy of the DiD design in producing causal estimates depends on the assumption that, absent the regulatory shock, innovation activity of treatment and control firms exhibits parallel trends. We conduct several tests to assess the plausibility of this assumption.

## 5. Results

### *5.1. Main Results*

[13] Note that the main effects of *Post* and *Treat* are excluded in the equation because they are subsumed by time and firm fixed effects, respectively.

We begin by presenting visual evidence on the effect of the SEC regulation on innovation output. Figure 1 plots the innovation output for treatment and control firms in the years around the SEC regulation using our main variable (citation-weighted patent counts). The evidence is striking. First, during the 5 years prior to the SEC regulation, the innovation output of treatment and control firms moves in tandem, supporting the parallel trends assumption. Second, the innovation output of treatment firms exhibits a significant downward trend (relative to control firms) after the regulation.

Panel A of Table 2 provides formal evidence by presenting the OLS estimates of equation (1). We cluster standard errors by firm to allow for correlation between repeated observations from the same firm. In column (1), we present the estimates after including just the firm and year fixed effects and no control variables. The coefficient on the main variable of interest, the interaction term *Treat* × *Post*, is negative and significant at the 1% level (coefficient = −0.224). This suggests that firms with high ownership by funds affected by the SEC regulation experience a decline in innovation following the regulation. Adding several time-varying control variables in column (2) makes little difference to the significance of the innovation decline (coefficient = −0.215; $p$-value < 0.01).[14] The innovation decline is also economically meaningful: the coefficient estimates imply that the average firm files about 1.1 fewer citation-weighted patents per year after the regulation. The magnitude compares favorably to the effects of other economic forces documented in prior research (e.g., Amore, Schneider, and Zaldokas, 2013; Fang, Tian, and Tice, 2014; Mukherjee, Singh, and Zaldokas, 2017).

In untabulated analyses, we conduct two sets of robustness tests. First, we consider three alternative ways of identifying treatment firms using: (i) ownership in the year prior to the regulation as opposed to 5-year average, (ii) continuous measure of percentage ownership by affected funds, and (iii) an indicator for the presence of at least one large affected mutual fund blockholder with at least 2%

[14] In untabulated analyses, we find that our results are also robust to controlling for several measures of contemporaneous changes in liquidity. This mitigates the concern that our results may be driven by increases in liquidity caused by the SEC regulation (Fang, Tian, and Tice, 2014; Agarwal et al., 2015).

ownership.[15] Our inferences are unaffected with these alternative classifications. Second, to ensure that our results are not driven by the skewed nature of the patent data (for many firm-years, the patent measure is zero), we estimate quantile regressions (in which we model the 75$^{th}$, 80$^{th}$, 90$^{th}$, and 95$^{th}$ percentiles) and also estimate zero-inflated Poisson regressions. Again, our inferences continue to hold.

*5.2 Effect on quantity vs. quality of patents*

In columns (3) and (4) of Table 2, we decompose our main innovation measure into its components and examine whether the innovation output decline is the result of a decrease in the quantity of patents (*NumPat*) or quality of patents (*CiteperPat*). Recall that *CiteperPat* is set to zero for firm-year observations with no patents. Therefore, to ensure that our results for *CiteperPat* are not driven by reduction in quantity of patents, we augment the model for *CiteperPat* with an indicator variable for missing patent (*NoPatent*) and its interaction term with *Post*. In the presence of these two variables, the identification of *Treat* × *Post* is only informed by observations where there is a patent available to yield citations. Regression estimates reveal that, following the regulation, firms file both fewer patents and less impactful patents. Specifically, firms file 12.3% fewer patents (nearly 0.5 fewer patents for an average firm) and patents that receive 1.5% fewer citations.

*5.3 Assessing parallel trends and timing of innovation changes*

Next, we explore the timing of the changes in innovation around the SEC regulation to (i) test the parallel trends assumption underlying the DiD design and, (ii) to examine the persistence of the innovation decline. The parallel trend assumption states that conditional on covariates in the regression, treatment and control firms exhibit parallel movements in their innovation outcomes in the absence of the treatment shock. To test this assumption, we augment our specifications with two interaction terms for indicator variables for each of the two years prior to the regulation (*Pre(1)* and *Pre(2)*). Results reported in Table 3, column (1) indicate that neither interaction term, *Treat* × *Pre(1)* nor *Treat* × *Pre(2)*

[15] We use 2% cut-off based on McCahery, Sautner, and Starks (2016).

is statistically or economically significant, consistent with innovation output of high and low ownership firms following parallel trends prior to the regulation.

Even if the parallel trends assumption prior to the regulation period is satisfied, possibility exists that differences in characteristics of treatment and control firms cause the trends to diverge after the SEC regulation for reasons unrelated to disclosure requirements. For example, if active mutual funds tend to invest in value stocks and if value firms have declining innovation around the reform date (for reasons unrelated to mutual fund ownership), then our results could not be attributed to the SEC regulation.

To mitigate this concern, we assess the robustness of our results to including direct controls for heterogeneous trends that might result from observable differences in treatment and control firms. Specifically, in column (2), we augment the regression with interaction terms of initial firm characteristics (measured in the first year a firm appears in our sample) with year indicator variables. The inclusion of these terms flexibly absorbs any differences in trends in innovation output of treatment and control firms based on observable firm characteristics. Estimates in column (2) suggest that our DiD estimate of innovation decline continues to be economically and statistically significant (coefficient = –0.180; $p$-value < 0.01). In column (3), we include industry-year interactive fixed effects based on Fama and French (1997) 48-industry classification to absorb any heterogeneous trends that might result from differences in industry membership across treatment and control firms. Our results continue to hold (coefficient = –0.192; $p$-value < 0.01). Finally, estimates in column (4) show that our inferences are robust when we include interactive effects based on initial firm characteristics and industry membership in the same specification (coefficient = –0.158; $p$-value < 0.01). The above analyses provide comfort that our inferences are unlikely to be driven by differences in treatment and control firms that are predictive of future trends in innovation output.[16]

[16] In the Internet Appendix, we present the robustness of our results to estimation of DiD specifications on a matched sample of treatment and control firms. Because economically similar firms are more likely to satisfy parallel trends assumption, this analysis further mitigates concerns about our findings being driven by violation of this assumption.

In column (5), we explore the persistence of the decline in innovation following the SEC regulation. We do this by including a separate *Treat* × *Post* interaction term for each of the 5 years after the regulation. The results suggest that the innovation decline following the regulation is persistent and strengthens over time. The coefficient estimate of the innovation decline increase from an average of about 0.063 for the first 2 years to about 0.25 for the last 3 years, with the estimates being significant at less than 1% level in each of the 5 years. The slow increase in innovation decline is consistent with the idea that it might take some time for firms to adjust their R&D activities.[17]

*5.4 Additional analyses for testing causality*

We next exploit several alternative strategies to further aid a causal interpretation of our results.

*5.4.1 Placebo-type tests*

In this analysis, we attempt to control for any differential trends in the innovation output of treatment and control firms that are unrelated to disclosure requirements by exploiting ownership by four control groups of institutional investors whose frequency of portfolio disclosure was unaffected by the SEC regulation. By documenting that the innovation decline associated with high affected fund ownership is over and above any decline associated with ownership by other unaffected institutional investors, we can mitigate concerns that our results could be driven by any general downward trend in innovation output of high institutional ownership firms. A potential problem with these tests is that the SEC regulation could have spillover effects on the incentives of unaffected institutional investors. Froot, Scharfstein, and Stein (1992) show that increased short-termism by one class of investors can make it rational for other investors to also behave in a similar manner. The presence of such spillover effects

[17] Although the magnitudes are small, we observe decline in innovation output as early as in the first two years. Such quick change in innovation output is commonly observed in prior work (e.g., Fang, Tian, and Tice, 2014; Aghion, Reenen, and Zingales, 2013; Mukherjee, Singh, and Zaldokas, 2017). As Aghion, Reenen, and Zingales (2013) explain, this is to be expected because at any point in time firms with high R&D are likely to have several developed ideas waiting to be readily patented. Since patenting consumes money and effort, reduced focus on innovation can lead to quick reduction in patent output as firms may decide against pursuing patenting of these already developed ideas.

could attenuate or even eliminate the effect of interest in these tests. The estimates from these tests are therefore best viewed as lower bounds on the effect of the SEC regulation.

Our first control group comprises funds that voluntarily disclosed their portfolio holdings at quarterly frequency prior to the disclosure mandate (voluntary disclosers). A challenge with using this control group is that voluntary quarterly reporting is an endogenous choice and therefore voluntary reporters may differ from mandatory adopters (Ge and Zheng, 2006). Therefore, as a second control group, we consider a propensity-score-matched sample of mutual fund voluntary disclosers based on a logistic model in Agarwal et al. (2015), which is motivated by Ge and Zheng (2006). As our third and fourth control groups, we consider non-mutual fund institutions and hedge funds, respectively. These categories of institutional investors are subject to different disclosure requirements under Section 13(f) of the Securities and Exchange Act of 1934, and therefore, unaffected by the 2004 regulation.

To conduct this analysis, we augment our regression specifications with indicator variables for high ownership by control investor groups (*Unaffected*) and their interaction terms with *Post*. We use a common cut-off to identify high ownership by affected mutual funds and all the control investor groups. Specifically, we classify ownership by an investor group as high if it is more than the median level of ownership by affected mutual funds prior to the regulation. This approach allows us to compare the effect of ownership by affected mutual funds to similar levels of ownership by control groups. The coefficient on the interaction term *Unaffected* × *Post* allows us to gauge the effect of any other concurrent shocks that affect the innovation of firms with high institutional ownership. To the extent that factors other than the disclosure mandate influence the relation between mutual fund ownership and innovation output, the difference in coefficients of the interaction terms *Treat* × *Post* and *Unaffected* × *Post* is a better measure of the impact of the change in disclosure frequency.

Table 4 presents the results of this analysis. Across all the control groups (columns 1-4) the coefficient on the primary variable of interest, *Treat* × *Post* is negative and significant at the 1% level. For the control group of voluntary disclosers (column 1), the coefficient on the interaction term for the voluntary disclosers is statistically and economically insignificant. The results are similar for the propensity-score-matched sample of voluntary disclosers (column 2). For both non-mutual funds and hedge funds, we find that the interaction term for affected mutual fund ownership is much larger (about 2 to 2.3 times) than for either of these two control groups, with the differences being statistically significant (see columns 3 and 4). Collectively, this evidence further corroborates that our results are unlikely to be driven by any unobserved shocks coinciding with the SEC regulation.

*5.4.2 Instrumental variable analysis*

Next, we consider instrumental variable (IV) estimation. Following Aghion, Reenen, and Zingales (2013), we use a firm's addition to the Standard and Poor's (S&P) 500 index as an instrument for exogenous increase in institutional ownership. As noted in their study, because active institutional investors are typically benchmarked against S&P 500 index, they have an incentive to own S&P 500 firms. The exclusion restriction is likely to be satisfied because stocks are added to S&P 500 because they represent a sector well, and not because of their expected performance or innovation output.[18]

We use two-stage least squares (2SLS) for the IV estimation. To accommodate 2SLS in our DiD set-up, we estimate the following modified version of our DiD specification:

$$\Delta Log(Citations)_i = \alpha + \beta Treat_i + \Gamma Controls_i + \varepsilon_i, \qquad (2)$$

where $\Delta Log(Citations)_i$ is the difference between the mean of the logged innovation output in the post-regulation period and the mean in the pre-regulation period. Because the dependent variable in this specification already represents the first difference, coefficient $\beta$ in this specification carries a difference-in-differences interpretation. The above specification allows us to adopt the standard 2SLS estimation procedure with one endogenous regressor and one instrumental variable. $Controls_i$ includes all control

[18] Standard and Poor's states that inclusion of a firm in the S&P 500 index is not an opinion on its investment potential. http://www.standardandpoors.com/regulatory-affairs/indices/en/us.

variables used in our main analysis, measured in the year prior to the regulation; In addition, we also control for three prior-period stock returns and their squared and cubic terms.[19]

Table 5 presents the results of IV estimation using an indicator for whether a firm is part of the S&P 500 index in the year prior to the SEC regulation as the instrumental variable. Estimates in column (1) of the first stage indicate that S&P 500 inclusion is a strong predictor of high ownership by affected funds. Specifically, S&P 500 inclusion is associated with a 29% increase in the likelihood of a firm being classified as a treatment firm ($p$-value < 0.01). We also compute Cragg-Donald Wald F-statistic to conduct a formal test of whether the instrument is weak. Using the critical values for this test compiled by Stock and Yogo (2005), the hypothesis of weak instrument is rejected at the 1% level. Estimates in column (2) of stage 2 suggest that treatment firms exhibit a significantly larger decline in average citation-weighted patent counts following the SEC regulation (coefficient estimate = –0.494; $p$-value < 0.01). The economic magnitude of the innovation decline is larger than what we obtain from simple DiD estimates in Table 2. These results mitigate concerns that our main results are spuriously driven by the endogeneity of treatment assignment. It is worth noting that while IV estimates are more amenable to causal inference, they tend to estimate the local average treatment effects for a narrow set of units that actually respond to the instrument (Imbens and Angrist, 1994). These estimates therefore likely represent the upper bound of the effect of interest and are unlikely to represent the average effect for the larger sample, which we believe is better characterized by our main DiD estimates in Table 2.[20]

[19] Aghion, Reenen, and Zingales (2013) note that while inclusion in S&P 500 should be orthogonal to expected future performance/innovation output, to ensure stable index composition S&P avoids choosing firms at the risk of bankruptcy and prefers large firms with good past performance. They find that S&P firms exhibit good stock returns in the three years preceding their inclusion in the index and note that the possibility exists that these returns might contain information about potential *increases* in innovation. They address this issue by including past returns as a control variable. Since we find a *decline* in innovation, this issue is only likely to bias against finding our results. Nonetheless, to ensure our results are not driven by any differences in past performance of treatment and control firms, we control for stock returns for the last three years and their squared and cubic terms to control for any nonlinearities.

[20] In the Internet Appendix, we explore the possibility that our results could be due to increased short-term pressure on corporate managers resulting from an increase in retail ownership around the SEC regulation. Inconsistent with the alternative explanation, we find that, if anything, treatment firms experience a slight decline in retail ownership relative to control firms after the SEC regulation.

## 6. Cross-sectional tests

We conduct three cross-sectional tests to further corroborate that the innovation decline following the disclosure shock reflects the effect of increased myopic pressures.

*6.1 Role of CEO incentives*

As discussed in section 3, corporate managers are expected to respond to increased short-term orientation of affected mutual funds because of the threat of exit by these institutional investors, which may result in a stock price decline. We therefore expect the innovation decline to be greater for corporate managers whose welfare is more closely tied to stock price performance. We use three different measures to capture the sensitivity of the corporate manager's welfare to changes in current stock prices. The first measure is a proxy for the sensitivity of CEO wealth to stock prices (*Delta*) calculated as the increase in value of the CEO's stock- and option-based portfolio for a 1% increase in stock price, using the methodology in Core and Guay (2002). We obtain data on CEO identities and stock and option holdings from the Equilar database.[21] We then divide our sample firms into two groups based on an indicator variable for whether the *Delta* measure is above median in the year prior to the SEC regulation (*HighDelta*). We then allow the effect of the SEC regulation to vary across the two groups by estimating a modified version of our main specification augmented with *HighDelta* and its interaction terms with *Treat*, *Post*, and *Treat* × *Post*. Because we require the firms to be present in the Equilar database for the year prior to the SEC regulation, our sample size for this analysis is reduced considerably. Table 6 presents the results of this analysis. Consistent with our hypothesis, estimates in column (1) indicate that the innovation decline is much larger (nearly twice) for firms with above-median values of CEO *Delta* prior to the regulation.

For the second measure, we exploit the industry-level variation in the sensitivity of CEO turnover to stock price performance. We expect the CEOs to be more concerned about short-term stock prices in

[21] We use Equilar database instead of ExecuComp to maximize our sample size. While Equilar collects data on executive compensation for Russell 3,000 companies, ExecuComp provides this data for only S&P 1500 firms.

industries in which they are more likely to experience a job loss following poor stock price performance. We measure the turnover sensitivity at the industry level over the 5-year period prior to the regulation. Following Parrino (1997), we carefully identify the cases of forced turnover of CEOs.[22]

We follow the approach in Jenter and Kanaan (2015) to estimate the forced turnover sensitivity to firm-specific performance (see Appendix for the details of the estimation methodology). To obtain reasonably precise estimates, we require at least five incidences of forced CEO dismissals (in the estimation window of 5 years prior to the regulation) in an industry. Because of this data requirement, we are able to estimate turnover sensitivities for only 13 industries when we use the Fama-French 48-industry classification. Therefore, we also replicate this analysis using the relatively coarser Fama-French 10-industry classification where we are able to obtain turnover sensitivities for 9 out of the 10 industries and can use most of our sample observations in the final regression. Table 6, columns (2) and (3), presents the results of this analysis using 48- and 10-industry classification, respectively. Consistent with our hypothesis, estimates in both columns suggest that the innovation decline is much larger (nearly 60 to 78% higher) in industries that exhibit above-median CEO turnover sensitivities.

Finally, we use CEO tenure to proxy for CEO's concerns about short-term stock price performance. We expect inexperienced corporate managers (for whom there is more uncertainty in the labor market about their ability) to have greater incentives to show high performance in the short run to create a favorable perception about their ability (e.g., Narayanan, 1985). We measure CEO tenure in the year prior to the SEC regulation. Consistent with our prediction, estimates in Table 6, column (4) indicate that the innovation decline in about 46% larger for firms with less experienced CEOs. Collectively, the above tests provide evidence that the innovation decline is stronger for CEOs who are more concerned about declines in stock prices in the short run and therefore are more sensitive to the exit threat posed by mutual fund managers.

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[22] We cannot use Equilar data to calculate turnover sensitivities because we have access to this data only for the one year before regulation. We require the time-series of CEO identities over the 5-year period before regulation for this analysis.

*6.2 Effect of fund manager age*

Compared to experienced fund managers with established reputations, we expect younger fund managers without an established track record to be more concerned about demonstrating their ability in the near term (Chevalier and Ellison, 1999). Thus, we expect the increase in short-term pressures following the regulation to be greater for firms that have greater ownership by funds managed by younger fund managers. To test this hypothesis, we divide the managers of affected mutual funds into young and old fund managers based on the median age cut-off and create two separate *Treat* indicators based on ownership by younger and older fund managers and their interaction terms with *Post*. We obtain data on fund manager age from Morningstar. Because manager birth dates are often missing in Morningstar, following the approach in Chevalier and Ellison (1999), when birth data is missing we infer fund managers' age using college graduation dates by assuming that the manager was 21 years old at graduation. As predicted, results in Table 7 indicate that the innovation decline is much greater (nearly three times) for firms with high ownership by mutual funds managed by younger managers.

*6.3 Variation in the nature of innovation*

Finally, we exploit the cross-sectional variation in the nature of innovation. We expect the decline to be steeper for innovations that are more likely to generate radical business opportunities whose profit potential may take a long time to realize. We base this analysis on the distinction between explorative and exploitative innovations that has been widely examined by prior work (March, 1991; Henderson, 1993; Levinthal and March, 1993; Sørensen and Stuart, 2000; Chava et al., 2013). Exploitative innovations build upon the firms' existing body of technological knowhow and hence, represent incremental improvements and refinements in existing technologies. In contrast, explorative innovation involves developing new technologies outside of firms' existing scope and are based on learning-by-experimentation (e.g., Henderson, 1993; Levinthal and March, 1993), which often result in radical technological advances. In contrast to exploitative innovations, explorative innovations are more likely to result in path-breaking products whose business potential may take a long time to realize. We therefore expect explorative innovations to be more vulnerable to myopic pressures and expect a greater decline for them following the disclosure shock.

Following prior work (e.g., Chava et al., 2013), we define exploitative patents as those that include at least one citation to a prior patent assigned to the same assignee (i.e., at least one self-citation). Intuitively, patents that exhibit self-citations are likely to be building upon firms' prior knowhow. Conversely, explorative patents are those that do not exhibit any self-citations. As in prior work, we find that nearly 60% of the patents in our sample are explorative and 40% are exploitative.

In Table 8, we separately model the counts for explorative and exploitative patents. DiD estimates in columns (1) and (2) show that explorative patent counts (estimate = –0.160; $p$-value < 0.01) decline at nearly twice the rate of exploitative patents (estimate = –0.078; $p$-value < 0.01). To statistically compare the effects on explorative and exploitative patents, we also jointly estimate the two models using maximum likelihood estimation. Results in columns (3) and (4) show that the estimated decline in explorative patents continues to be nearly twice that for exploitative patents, with the difference being statistically significant at the 1% level. Collectively, the evidence suggests that the decline is steeper for innovations that are more likely to generate value over longer horizons and consequently are more vulnerable to myopic pressures.

**7. Evidence from trading behavior of fund investors and fund managers**

In this section, we attempt to further corroborate our economic story by exploring the changes in trading behavior of fund investors and fund managers.

*7.1 Sensitivity of mutual fund flows to portfolio holdings information*

In theory, the SEC regulation makes fund managers more short-term focused because increasing the timeliness and reliability of portfolio holdings information increases fund investors' reliance on the information contained in individual stock picks to learn about fund manager's ability. This intensifies manager's concerns that poor short-term performance (as revealed in a quarterly portfolio disclosure) on a long-term stock pick could be mistaken for poor ability and lead to outflows. We estimate the following

DiD specification to examine if fund flows indeed become more sensitive to the presence of losing/winning stocks in the portfolio following the regulation:

$$\begin{aligned} Flow_{i,t+1} = \alpha_i + \beta_t + \gamma_0 \%Losers_{it} + \gamma_1 TreatFund_i \times \%Losers_{it} + \gamma_2 \%Losers_{it} \times Post_t \\ + \gamma_3 TreatFund_i \times \%Losers_{it} \times Post_t + \gamma_4 \%Winners_{it} + \gamma_5 TreatFund_i \\ \times \%Winners_{it} + \gamma_6 \%Winners_{it} \times Post_t + \gamma_7 TreatFund_i \times \%Winners_{it} \times Post_t \\ + \gamma_8 TreatFund_i \times Post_t + \Lambda Z + \varepsilon_{i,t+1} \end{aligned} \quad (3)$$

where *Flow* is the quarterly flows for a fund, measured as the percentage change in fund's assets after adjusting for fund returns, *Post* is an indicator variable for periods after the SEC regulation, *TreatFund* is an indicator variable that equals one for funds that increased their disclosure frequency following the regulation, and zero for funds unaffected by the regulation. *%Losers (%Winners*) is the percentage of assets of a fund invested in stocks that might appear as losers (winners) to fund investors. Building upon the approach in Lakonishok et al. (1991), we define losing (winning) stocks as those that fall in the bottom (top) quintile of return performance within the group of firms with similar size and book-to-market ratios during the reporting period *t*. Specifically, we first divide stocks into quintiles based on size, and then further each size quintile into five groups based on book-to-market ratio. We identify losing stocks as the bottom 20% based on return performance in each size and book-to-market group. The equation also includes, fund and time fixed effects ($\alpha_i$ and $\beta_t$) as well as a vector of control variables, *Z* (for variable definitions, see Appendix).[23] Main effects of *TreatFund* and *Post* are excluded due to presence of fund and time fixed effects. The key coefficients of interest are $\gamma_3$ and $\gamma_7$, coefficients on the triple interaction terms, which capture the DiD estimate of the effect of the regulation on the sensitivity of fund flows to the presence of losing (winning) stocks in a fund's portfolio.

[23] An important control variable included is the fund's risk-adjusted performance (alpha). Controlling for fund performance (whose availability is unaffected by the regulation) allows us to focus on the incremental information content of portfolio holdings. We allow for the well-documented convexity in the flow-performance relation (e.g., Chevalier and Ellison, 1997; Sirri and Tufano, 1998). Following the approach in Sirri and Tufano (1998), the coefficient on fund alpha varies across the bottom quintile, the middle three quintiles, and the top quintile through the use of three variables (*AlphaBot*, *AlphaMid*, and A*lphaTop*) that equal alpha if the observation falls under the corresponding portion of the alpha distribution, and zero otherwise. We also include the interactions of these three variables with *Post* dummy to allow for any changes in the flow-performance relation around the SEC regulation.

We estimate the specification on a fund-quarter panel of 54,360 observations. For funds that report semi-annually in the pre-regulation period, holdings information for some quarters would not be available. If the holdings data for a quarter are not available, we impute the *%Losers* and *%Winners* variables for that quarter based on the most recent holdings disclosure prior to that quarter. This approach mimics the decision making of fund investors who can only draw upon stale holdings information in such quarters while making trading decisions.

An important consideration in this analysis is the timing of measurement of fund flows. The SEC regulation allows funds to disclose holdings for the end of the quarter $t$ with a maximum delay of up to 60 days. If we measure *Flow* only for the 3-months subsequent to quarter $t$, we would capture only one month of flows after the holdings information actually becomes available to fund investors for funds that utilize 60-day delay option. Measuring the fund flows for the 3-month period after the maximum allowable delay of 60-days is also problematic because many funds do not use the maximum delay of 60 days and report their holdings much earlier, which would preclude us from capturing the flows for the months immediately following the holdings disclosure for funds that report early. To address the above issue, we measure the flows over the two quarters following the quarter for which winner and loser variables are measured, which allows us to capture flows for periods after the disclosure of holdings for all cases of disclosure delays.[24] We cluster standard errors at fund level, which adjusts for arbitrary forms of correlations between observations for the same fund that might result from overlapping windows for flow measurement. In a robustness check discussed in the Internet Appendix, we report results using the more conservative approach in which we measure the flow over the 3-month period after the maximum allowable delay of 60-days and find our inferences to be robust.

[24] We also considered using the actual filing dates to more accurately measure the flows after the holdings are observable by the fund investors. Since we can find the actual filing dates only for 17% of the observations, this approach results in a significant attrition in sample size and a substantial loss in statistical power for our analysis.

Table 9 presents the results of estimating equation (3). We focus our discussion based on the results in column (1) in which we control for fund alpha based on a 6-factor model; results are similar from other models. First, it can be seen that the coefficient on *%Losers* is negative and significant (estimate = –0.244; $p$-value < 0.01), suggesting that fund investors respond negatively to the presence of losing stocks for the quarterly reporting control funds in the pre-period. Second, the coefficient on *TreatFund* × *%Losers,* which captures the incremental sensitivity for treatment funds in the pre-period, is positive and significant (estimate = 0.207; $p$-value < 0.05). This suggests that semi-annual reporters exhibit significantly lower sensitivity to presence of losing stocks in the pre-period. Finally, and most important, the DiD estimate (coefficient on the triple interaction term) is negative and significant (estimate = –0.278; $p$-value < 0.05), suggesting that the SEC regulation significantly increased the penalty for presence of losing stocks. This result is remarkably robust to extensive sensitivity checks we report in the Internet Appendix. We, however, do not find any evidence of changes in the sensitivity of flows to the presence of winning stocks.[25] The above evidence lends support to our argument that, all else equal, fund managers would be more worried about showing losing stocks in their quarterly portfolio holdings after the regulation.

*7.2 Investment horizon of mutual funds*

A fund manager more focused on showing stock price appreciation in the near term would have less incentive to research and identify undervalued stocks with good long-term potential, causing the fund manager to focus on investment strategies with shorter holding periods. We estimate various versions of the following DiD specification to test this prediction:

$$Log(Horizon)_{j,t} = \alpha_j + \gamma_t + \beta\ TreatFund_j \times Post_t + \Lambda Z + \varepsilon_{j,t} \qquad (4)$$

[25] The asymmetry in the effect of the regulation on *%Losers* and *%Winners* is consistent with evidence in Lakonishok et al. (1991), who find that fund managers window dress by selling losers. Since window dressing distorts fund's true exposure to losers, we expect the effects of the provision for timely information to manifest mainly for *%Losers*.

where $Horizon_{j,t}$ is a measure of turnover of fund $j$ in year $t$; and, $Z$ is a vector of time-varying fund-characteristics (described in the Appendix) that are known to affect fund turnover. The equation also includes fund fixed effects ($\alpha_j$) and time fixed effects ($\gamma_t$). The coefficient of interest is $\beta$, which represents the DiD estimate of the effect of SEC regulation on mutual funds' trading horizon.

Our main measure of investor duration (*Dur_CP*) is based on Cremers and Pareek (2014), who show that this measure outperforms other commonly used measures of trading by short-term institutions. For robustness, we also consider three other turnover measures that capture the inverse of holding horizon. The first two measures represent the annual average of the quarterly churn rates computed using the approaches in Gaspar, Massa, and Matos (2005) and Yan and Zhang (2009), respectively (*Churn_GMM* and *Churn_YZ*). The main difference between the two approaches is that while Gaspar, Massa, and Matos (2005) use the sum of aggregate purchases and sales, Yan and Zhang (2009) use the minimum of the aggregate purchases and sales. Yan and Zhang (2009) note that the advantage of their approach is that it minimizes the impact of fund flows on turnover. Finally, we also use an annual turnover measure provided by CRSP Mutual Fund database (*Churn_CRSP*) that accounts for intra-quarter trading not captured in the turnover measures imputed from holdings. We annualize the first three measures available at quarterly frequency by taking the average over four quarters.

Table 10 presents the results from estimating equation (4). Regardless of the measure used, we find significant decrease (increase) in mutual funds' holding horizon (turnover) following the regulation. The decline in investment horizon is also economically significant. For example, estimates in column (2) show that there is an average decline of nearly 6% in the duration measure in the years 3-5 after the SEC regulation. Taken together, these results further support the interpretation that the decline in innovation is due to fund managers' increased focus on the short-run results.

*7.3 Other changes in mutual fund trading behavior*

In the Internet Appendix, we provide evidence on changes in two other dimensions of mutual fund trading behavior that is consistent with increased short-term focus. First, using return on assets and stock returns as measures of investee firm performance, we show that affected mutual fund ownership is significantly more sensitive to short-run corporate performance after the SEC regulation. Second, we explore whether funds respond to the regulation by tilting their portfolios to less innovative firms. A priori it is not clear that we would expect such a shift in portfolio holdings in equilibrium following the regulation. On the one hand, the increased short-term focus would incentivize fund managers to assign greater portfolio weight to firms with less innovative and shorter-term-oriented corporate investment policies. On the other hand, given that corporate managers respond to this short-term focus of fund managers by reducing their investment in innovative projects, it is not obvious that fund managers would need to move away from these stocks in equilibrium. We find that fund managers affected by the SEC regulation exhibit reduced portfolio allocation towards highly innovative firms that generate patents with higher citation counts. These findings further indicate that the decline in corporate innovation reflects increased short-term focus of fund managers.

**8. Conclusions**

Pressure from institutional money managers to generate profits in the short run is often blamed for corporate myopia. Theoretical research suggests that money managers' short-term focus stems from their career concerns and greater fund transparency can amplify these concerns. Using a difference-in-differences design around a regulatory shock that increased transparency about fund managers' portfolio choices, we provide evidence consistent with more frequent portfolio disclosures intensifying myopic corporate behavior in investee firms by increasing the short-term focus of money managers.

Our results highlight the role of mandated quarterly portfolio disclosures in shaping short-term incentives and lend support to the argument in Shleifer and Vishny (1990) that it is the money managers' own career concerns and performance evaluation pressures that lie at the root of their short-term focus that promotes corporate myopic behavior. These findings portray a dark side of the consequences of agency relationship between money managers and fund investors on the real economy – a phenomenon that has become increasingly important to understand because of the astounding growth in delegated money management over the last three decades. Our paper also adds to our understanding of the consequences of increased mandated information disclosure by establishing how it can create adverse real effects by distorting the incentives of career-concerned agents.

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**Appendix**

This appendix includes the description of the main variables used in the analysis.

| Variable name | Description |
|---|---|
| | Panel A: Innovation measures |
| *NumPat*$_{it}$ | Number of patents applied for in year $t$ that were eventually granted. To adjust for the truncation bias, we follow Hall, Jaffe, and Trajtenberg (2001, 2005) and scale each patent using weight factors constructed using the empirical application-grant lag distribution of patents granted during the 10 year period from 1990 to 1999. Specifically, for each patent during this period, we calculate the numbers of years between the application date and the grant date. We then define $W_y$ as the proportion of patents that were granted in $y$ years. Finally, we calculate the truncation adjusted patent count for year $t$ as $(Raw\ patent\ count)/\sum_{y=0}^{2010-t} W_y$. |
| *CiteperPat*$_{it}$ | Number of citations per patent for patents applied for in year $t$ that were eventually granted. Following Hall, Jaffe, and Trajtenberg (2001, 2005), we adjust for this truncation bias by dividing the citation count for each patent by the mean citation count for the grant year cohort to which the patent belongs. |
| *Citations*$_{it}$ | Citation weighted patent count of patents applied in year $t$ that were eventually granted. This measure is constructed using truncation bias adjusted patent counts and citations per patent as described above. |
| *NoPatent*$_{it}$ | Indicator variable that is set to 1 if the patent variable is missing, 0 otherwise. |
| *NumExplore*$_{it}$ | Number of patents (truncation bias adjusted) with no self-citations (e.g., Chava et al., 2013) applied for in year $t$. |
| *NumExploit*$_{it}$ | Number of patents (truncation bias adjusted) with at least one self-citation (e.g., Chava et al., 2013) applied for in year $t$. |
| | Panel B: Firm Characteristics |
| *MFOwn*$_{it}$ | Thomson Reuters S12 stock ownership of actively managed U.S. equity funds whose number of mandatory portfolio disclosures increased due to the May 2004 regulation change. It is measured as the average ownership over the 5-year period prior to the regulation. |
| *Treat* | Indicator variable coded as one for firms with above-median average ownership (measured over the five years prior to the regulation) by affected funds, and zero otherwise. |
| *Post* | An indicator variable that equals one for fiscal years subsequent to the passage of the SEC regulation in May 2004, and zero otherwise. |
| *MVE*$_{it}$ | Market value of equity in $ million at the end of fiscal year $t$. |
| *ROA*$_{it}$ | Return on assets for year $t$ measured as operating income before depreciation for year $t$ scaled by lagged total assets |
| *Q*$_{it}$ | Market to book ratio at the end of year $t$ measured as (market value of equity + book value of assets – book value of common equity)/(book value of assets). |
| *Cash*$_{it-1}$ | Cash scaled by total assets at the end of year $t-1$. |
| *Leverage*$_{it-1}$ | Book leverage at the end of year $t-1$ measured as (long term debt + debt in current liabilities)/(total assets). |
| *CapStock*$_{it-1}$ | Capital stock at the end of year $t-1$ measured as net property, plant, and equipment scaled by total assets. |
| *KZindex*$_{it-1}$ | Kaplan-Zingales (1997) index measured at the end of year $t-1$ calculated as –1.002×(net income + depreciation and amortization expense)/lagged PP&E + |

| | |
|---|---|
| | 0.2826389×(Total assets–book value of common equity–deferred tax _balance sheet + market cap of common equity)/total assets + 3.139193× Total debt/total assets – 39.3678×total dividend/lagged PP&E – 1.314759× cash and equivalent/lagged PP&E. |
| *Age$_{it-1}$* | Number of years a firm appears in the CRSP database as of the end of year *t-1*. |
| *Illiquidity$_{it-1}$* | Average during year *t-1* of the daily Amihud (2002) illiquidity measure calculated as the absolute value of daily return divided by daily dollar trading volume. |
| *Hindex$_{it-1}$* | Herfindahl-Hirschman index at the end of year *t-1* measured at the SIC four-digit level using market shares based on annual sales. |

Panel C: CEO measures

| | |
|---|---|
| *CEODelta* | Sensitivity of CEO wealth to stock prices measured as the increase in value of the CEO's stock- and option-based portfolio for a 1% increase in stock price calculated using the methodology in Core and Guay (2002). |
| *CEOTurnSens* | Forced turnover sensitivity to firm-specific performance computed following the approach in Jenter and Kanaan (2015). Specifically, we first decompose each firm's annual stock return into its industry- and firm-specific component using the following regression: $r_{i,t-1} = \beta_0 + \beta_1 r_{ind\ peers,t-1} + \mu_{i,t-1}$, where $r_{i,t-1}$ represents firm *i*'s annual stock return and $r_{ind\ peers,t-1}$ is the equal-weighted return of the industry peers (firm *i* is excluded from the peer group). We use the residual $\mu_{i,t-1}$as the measure of firm-specific stock return performance and the predicted value of the return,$r_{i,t-1}^{pred} = \widehat{\beta_0} + \widehat{\beta_1} r_{ind\ peers,t-1}$, as the portion of return driven by industry conditions. Finally, we obtain our measure of turnover sensitivity to firm-specific performance as the coefficient $\gamma_2$ from the following linear probability model: $CEO\ Forced\ Dismissal_{i,t} = \gamma_0 + \gamma_1 r_{i,t-1}^{pred} + \gamma_2 \mu_{i,t-1} + \varepsilon_{i,t}$. We estimate the above equation at the industry level to obtain $\gamma_2$ as an industry-level measure of CEO turnover sensitivity. We estimate this equation over the five-year period in our sample prior to the SEC regulation. To obtain reasonably precise estimates, we require at least 5 incidences of forced CEO dismissals in an industry. |
| *CEOTenure* | Number of years the executive worked as a CEO for a firm. |

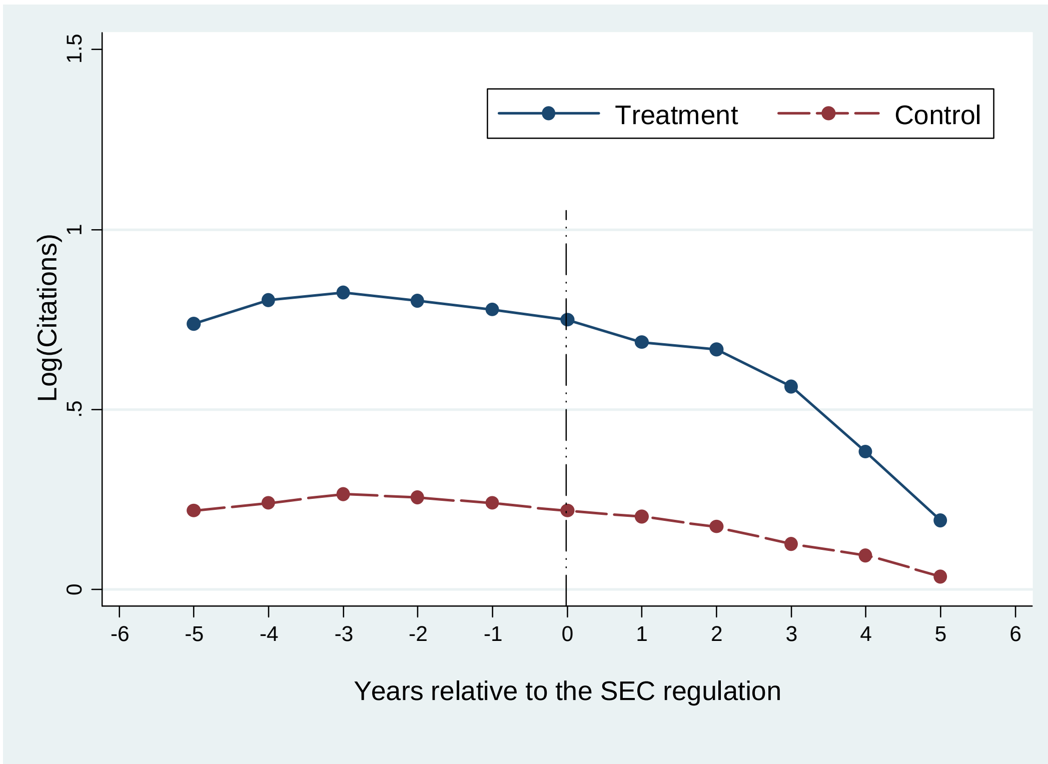
Panel D: Fund-level measures

| | |
|---|---|
| *TreatFund* | Indicator variable that equals one for mutual funds that increased their disclosure frequency following the regulation, and zero otherwise. |
| *%Losers* (*%Winners*) | *%Losers* (*%Winners*) is the percentage of assets of a mutual fund invested in losing (winning) stocks, i.e., stocks that fall in the bottom (top) quintile of return performance within the group of firms with similar size and book-to-market ratios during the reporting period *t*. We first divide stocks into quintiles based on size, and then further each size quintile into five groups based on book-to-market ratio. We then identify losing (winning) stocks as the bottom (top) 20% based on return performance in each size and book-to-market group. |
| *Alpha* | Risk-adjusted fund performance measured using either CAPM, Fama-French (1993) 3-factor, Carhart 4-factor, Fama-French (2015) 5-factor or a 6–factor model that |

| | |
|---|---|
| | augments Fama-French 5–factor model with the liquidity factor from Pástor and Stambaugh (2003). |
| *AlphaBot* (*AlphaMid*) {*AlphaTop*} | *Alpha* if the observation for a period falls under the bottom quintile (middle three quintiles) {top quintile} of the *Alpha* distribution for that period, and zero otherwise. |
| *Fund Size* | Assets under management in the fund at the end of the fiscal quarter. |
| *Load* | An indicator variable defined as 1 if fund has any front-end or back-end load, and 0 otherwise. |
| *Expense ratio* | Fund's annual expense ratio reported at fund's fiscal year end. |
| *Turnover* | Minimum of the total purchases and sales by a fund in a quarter divided by beginning-of-the-quarter assets. |
| *Style distinctiveness* | One minus the R-square from four-factor model following Sun, Wang, and Zheng (2012) and Amihud and Goyenko (2013). |
| *Active share* | Active share measure as defined in Cremers and Petajisto (2009). |
| *Quarterly flows* | Percentage change in the assets under management of a fund after adjusting for fund returns during the quarter. |
| *Churn_GMM$_{jt}$* | Churn rate of fund *j* in quarter *t* is $CR_{j,t} = \dfrac{\sum_{i\in K}\left\|N_{i,j,t}P_{i,t} - N_{i,j,t-1}P_{i,t-1} - N_{i,j,t-1}\Delta P_{i,t}\right\|}{\sum_{i\in K}\dfrac{N_{i,j,t}P_{i,t} + N_{i,j,t-1}P_{i,t-1}}{2}}$ where *K* is the number of firms held in the portfolio of fund *j*, $P_{i,t}$ and $N_{i,j,t}$ are the price and number of shares of firm *i* held by fund *j* in quarter *t*. |
| *Churn_YZ$_{jt}$* | Churn rate of fund *j* in quarter *t* is $CR_{j,t} = \dfrac{\min(CR\_buy_{j,t}, CR\_sell_{j,t})}{\sum_{i\in K}\dfrac{N_{i,j,t}P_{i,t} + N_{i,j,t-1}P_{i,t-1}}{2}}$ where *K* is the number of firms held in the portfolio of fund *i*, $P_{i,t}$ and $N_{i,j,t}$ are the price and number of shares of firm *i* held by fund *j* in quarter *t*.<br>$CR\_buy_{j,t} = \sum_{i\in K}\left\|N_{i,j,t}P_{i,t} - N_{i,j,t-1}P_{i,t-1} - N_{i,j,t-1}\Delta P_{i,t}\right\|$ where $N_{i,j,t} > N_{i,j,t-1}$<br>$CR\_sell_{j,t} = \sum_{i\in K}\left\|N_{i,j,t}P_{i,t} - N_{i,j,t-1}P_{i,t-1} - N_{i,j,t-1}\Delta P_{i,t}\right\|$ where $N_{i,j,t} < N_{i,j,t-1}$ |
| *Dur_CP$_{jt}$* | Fund duration is computed by averaging the duration of stock *i* in fund *j* in quarter *t*, using the market value of the stock holdings in each fund's portfolio as weights. The duration for each stock *i* in fund *j* is computed as: $Duration_{i,j,T-1} = \sum_{t=T-W}^{T-1}\left(\dfrac{(T-t-1)\alpha_{i,j,t}}{H_{i,j}+B_{i,j}}\right) + \dfrac{(W-1)H_{i,j}}{H_{i,j}+B_{i,j}}$ where $B_{i,j}$ is the total percentage of shares of stock *i* bought by fund *j* between quarters *t*=*T*–*W* and *t*=*T*–1, $H_{i,j}$ is the total percentage of shares of stock *i* held by fund *j* at quarter *t*=*T*–*W*, and $\alpha_{i,j,t}$ is the percentage of total shares outstanding of stock *i* bought or sold by fund *j* between *t*–1 and *t*, where $\alpha_{i,j,t}$>0 for buys and <0 for sells. |
| *Churn_CRSP$_{jt}$* | Annual fund turnover computed as the minimum of buys and sells divided by the fund's assets as reported in the CRSP mutual fund database. |

**Figure 1: Citations-weighted patent counts (in logs) for treatment and control firms during the pre- and post- SEC regulation periods**

The following figure plots the logarithm of the citations-weighted patent counts for the treatment firms (those with above-median ownership by funds prior to the regulation) and control firms (those with below-median ownership by funds prior to the regulation) for a five-year period before and after the regulation.



## Table 1: Descriptive Statistics

This table presents descriptive statistics of the main variables used for estimating equation (1). For variable descriptions, see the Appendix.

(N = 47,326)

| | Mean | StDev | P10 | P50 | P90 |
|---|---|---|---|---|---|
| *MFOwn* | 0.052 | 0.055 | 0.000 | 0.034 | 0.134 |
| *Treat* | 0.475 | 0.499 | 0.000 | 0.000 | 1.000 |
| *Citations* | 5.206 | 21.332 | 0.000 | 0.000 | 6.717 |
| *NumPat* | 4.618 | 18.323 | 0.000 | 0.000 | 6.314 |
| *Citeperpatent* | 0.307 | 1.053 | 0.000 | 0.000 | 1.031 |
| *MVE* | 3,085.594 | 9,196.970 | 18.097 | 280.972 | 6,436.893 |
| *ROA* | 0.055 | 0.235 | –0.152 | 0.088 | 0.251 |
| *Q* | 1.898 | 1.693 | 0.885 | 1.312 | 3.459 |
| *Cash* | 0.187 | 0.224 | 0.010 | 0.086 | 0.547 |
| *Leverage* | 0.212 | 0.207 | 0.000 | 0.168 | 0.493 |
| *CapStock* | 0.237 | 0.237 | 0.015 | 0.151 | 0.628 |
| *KZindex* | –12.870 | 41.299 | –28.309 | –1.736 | 1.950 |
| *Age* | 15.875 | 14.751 | 3.255 | 11.008 | 34.942 |
| *Illiquidity* | 2.064 | 7.055 | 0.001 | 0.043 | 4.185 |
| *Hindex* | 0.219 | 0.187 | 0.054 | 0.169 | 0.459 |

## Table 2: Mandatory portfolio disclosure and corporate innovation

This table presents ordinary least square estimates of equation (1) relating corporate innovation to mutual fund ownership. Measures of innovation include: (i) citation-weighted patent counts (*Citations*), (ii) simple patent counts (*NumPat*), and (iii) citations per patent (*CiteperPat*).*Treat* is an indicator for above-median average ownership by mutual funds affected by the SEC regulation in the periods prior to the regulation. *Post* is an indicator for periods after the SEC regulation. For variable descriptions, see the Appendix. Standard errors, reported in parentheses, are based on clustering at the firm level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

| | $Log(Citations_{it})$ | $Log(Citations_{it})$ | $Log(NumPat_{it})$ | $Log(CiteperPat_{it})$ |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $Treat_i \times Post_t$ | –0.224*** | –0.215*** | –0.123*** | –0.015** |
| | (0.021) | (0.021) | (0.017) | (0.006) |
| $Log(MVE)_{it}$ | | 0.033*** | 0.044*** | –0.004 |
| | | (0.007) | (0.006) | (0.003) |
| $ROA_{it}$ | | –0.093*** | –0.058** | –0.009 |
| | | (0.031) | (0.024) | (0.014) |
| $Q_{it}$ | | 0.006 | –0.011*** | 0.004** |
| | | (0.004) | (0.003) | (0.002) |
| $Q_{it-1}$ | | 0.020*** | 0.011*** | 0.001 |
| | | (0.003) | (0.002) | (0.001) |
| $Cash_{it-1}$ | | 0.151*** | 0.093** | 0.041** |
| | | (0.047) | (0.037) | (0.018) |
| $Leverage_{it-1}$ | | –0.028 | –0.037 | 0.007 |
| | | (0.041) | (0.032) | (0.015) |
| $CapitalStock_{it-1}$ | | 0.287*** | 0.078 | 0.005 |
| | | (0.064) | (0.054) | (0.021) |
| $KZindex_{it-1}$ | | –0.000 | –0.000 | –0.000 |
| | | (0.000) | (0.000) | (0.000) |
| $Log(Age)_{it-1}$ | | –0.051* | 0.017 | –0.037*** |
| | | (0.027) | (0.023) | (0.008) |
| $Illiquidity_{it-1}$ | | 0.002*** | 0.000 | 0.001*** |
| | | (0.000) | (0.000) | (0.000) |
| $Hindex_{it-1}$ | | 0.542*** | 0.149 | –0.076 |
| | | (0.183) | (0.164) | (0.056) |
| $(Hindex_{it-1})^2$ | | –0.460** | –0.101 | 0.064 |
| | | (0.191) | (0.176) | (0.054) |
| *NoPatent* | | | | –0.634*** |
| | | | | (0.012) |
| *NoPatent* × *Post* | | | | 0.207*** |
| | | | | (0.012) |
| Firm fixed effects | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes |
| Observations | 47,326 | 47,326 | 47,326 | 47,326 |
| R-squared | 0.818 | 0.820 | 0.882 | 0.737 |

## Table 3: Assessing parallel trends and the persistence of innovation changes

This table presents evidence on the timing of the effects of the May 2004 SEC regulation on patenting activity by presenting ordinary least square estimates of a modified version of equation (1) with firms' citation-weighted patent count as the dependent variable. *Treat* is an indicator for above-median average ownership by mutual funds affected by the SEC regulation in the periods prior to the regulation. *Pre(1)* and *Pre(2)* are indicator variables for observations that fall during one and two years prior to the SEC regulation. *Post(1)* through *Post(5)* is an indicator variable for observations that fall during one through five years after the SEC regulation. Control represents control variables in equation (1). Industry represents Fama-French 48 industry indicator variable. For other variable descriptions, see the Appendix. Standard errors, reported in parentheses, are based on clustering at the firm level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

| | $Log(Citations_{it})$ (1) | $Log(Citations_{it})$ (2) | $Log(Citations_{it})$ (3) | $Log(Citations_{it})$ (4) | $Log(Citations_{it})$ (5) |
|---|---|---|---|---|---|
| $Treat_i \times Pre2$ | 0.004 | 0.007 | 0.005 | 0.003 | 0.003 |
| | (0.015) | (0.017) | (0.016) | (0.017) | (0.017) |
| $Treat_i \times Pre1$ | 0.006 | 0.002 | 0.001 | –0.002 | –0.002 |
| | (0.018) | (0.019) | (0.019) | (0.020) | (0.020) |
| $Treat_i \times Post$ | –0.227*** | –0.180*** | –0.192*** | –0.158*** | |
| | (0.023) | (0.023) | (0.023) | (0.023) | |
| $Treat_i \times Post1$ | | | | | –0.068*** |
| | | | | | (0.023) |
| $Treat_i \times Post2$ | | | | | –0.058** |
| | | | | | (0.025) |
| $Treat_i \times Post3$ | | | | | –0.147*** |
| | | | | | (0.028) |
| $Treat_i \times Post4$ | | | | | –0.276*** |
| | | | | | (0.031) |
| $Treat_i \times Post5$ | | | | | –0.330*** |
| | | | | | (0.037) |
| $Controls_{t=1}$×year | No | Yes | No | Yes | Yes |
| Industry×year | No | No | Yes | Yes | Yes |
| Firm fixed effects | Yes | Yes | Yes | Yes | Yes |
| Firm-level controls | Yes | Yes | Yes | Yes | Yes |
| Observations | 47,326 | 47,326 | 47,326 | 47,326 | 47,326 |
| R-squared | 0.815 | 0.830 | 0.835 | 0.842 | 0.842 |

## Table 4: Effects of ownership by funds unaffected by the SEC regulation

This table presents evidence from placebo-type analysis in which we compare the effect of the ownership by mutual funds affected by SEC regulation to the effect of ownership by investor groups whose portfolio disclosure frequency was unaffected by the regulation. We consider four placebo investor groups: (i) all mutual funds that voluntarily reported at quarterly frequency prior to the regulation, (ii) propensity score (PS) matched group of voluntary quarterly reporters that are similar to the affected mutual funds on observable fund characteristics, (iii) all non-mutual fund investors (Non-MFs), and (iv) hedge funds. *Unaffected* is an indicator variable for above-median ownership by the respective unaffected investor groups. All estimates are obtained from modified versions of equation (1) (augmented with unaffected fund ownership variables and their interaction terms with *Post*) with citation-weighted patent counts as the dependent variable. For variable descriptions, see the Appendix. Standard errors, reported in parentheses, are based on clustering at the firm level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

| | Voluntary quarterly reporters | | | |
|---|---|---|---|---|
| | Full Sample (1) | PS matched Sample (2) | Non-MFs (3) | Hedge Funds (4) |
| $Treat_i \times Post_t$ | –0.230*** | –0.225*** | –0.185*** | –0.166*** |
| | (0.025) | (0.024) | (0.023) | (0.024) |
| $Unaffected_i \times Post_t$ | 0.033 | 0.027 | –0.079*** | –0.085*** |
| | (0.030) | (0.031) | (0.022) | (0.023) |
| **Difference** | **–0.263*****| **–0.251*****| **–0.107***** | **–0.081**** |
| | **(0.049)** | **(0.048)** | **(0.038)** | **(0.041)** |

## Table 5: Instrumental variable analysis using S&P500 membership as the instrument

This table presents two-stage least square estimates of equation (2) relating corporate innovation to mutual fund ownership, using an indicator for S&P500 membership in the year prior to the SEC regulation as an instrumental variable for *Treat*. *Treat* is an indicator for above-median average ownership by mutual funds affected by the SEC regulation in the periods prior to the regulation. The dependent variable *ΔLog(Citations)* represents the difference between mean logged citation-weighted patent count in the periods after and periods before the 2004 SEC regulation. For other variable descriptions, see the Appendix. Standard errors, reported in parentheses, are based on clustering at the firm level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

| | First Stage | Second Stage |
|---|---|---|
| | $Treat_i$ | $\Delta Log(Citations)_i$ |
| $Treat_i$ | | –0.494*** |
| | | (0.135) |
| $S\&P\ 500_i$ | 0.293*** | |
| | (0.028) | |
| *Test of weak instrument* | | |
| Cragg-Donald Wald F-statistic | 109.092 | |
| (*p*-value) | (0.000) | |
| Firm-level controls | YES | YES |
| Observations | 3,468 | 3,468 |
| R-squared | 0.429 | 0.199 |

## Table 6: Effect of CEO incentives

This table presents evidence on how the corporate innovation decline following the SEC regulation varies by CEO incentives. *CEODelta* represents the increase in value of the CEO's stock and option based portfolio for a 1% increase in stock price calculated using the methodology in Core and Guay (2002). *CEOTurnSens* is the sensitivity of CEO turnover to firm's stock price performance. This variable is measured at both the Fama-French 48 and Fama-French 10 industry level. *CEOTenure* is the number of years that the executive worked as a CEO at the firm. Dependent variable is firm-level citation-weighted patent counts. For all other variable descriptions, see the Appendix. Standard errors, reported in parentheses, are based on clustering at the firm level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

| | *CEODelta* | *CEOTurnSens* | | *CEOTenure* |
|---|---|---|---|---|
| | | Fama-French 48 | Fama-French 10 | |
| | (1) | (2) | (3) | (4) |
| *Treat* × *Post* (*Below median group*) | –0.144*** | –0.167*** | –0.178*** | –0.270*** |
| | (0.031) | (0.035) | (0.022) | (0.0368) |
| *Treat* × *Post* (*Above median group*) | –0.279*** | –0.268*** | –0.318*** | –0.186*** |
| | (0.0368) | (0.0458) | (0.0385) | (0.030) |
| *Test of differences:* | | | | |
| *Diff* (*Above median – Below median*) | –0.135*** | –0.101* | –0.140*** | 0.085* |
| | (0.048) | (0.058) | (0.044) | (0.047) |
| Firm fixed effects | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes |
| Firm-level controls | Yes | Yes | Yes | Yes |
| Observations | 29,455 | 25,496 | 46,071 | 28,518 |
| R-squared | 0.808 | 0.812 | 0.818 | 0.809 |

## Table 7: Effect of fund manager age

This table presents evidence on the effect of age of the fund manager on the innovation decline following the SEC regulation. *High age Treat* (*Low age Treat*) is an indicator variable for above-median ownership by mutual funds affected by the SEC regulation that are run by older (younger) fund managers. Fund managers with above (below) median age in the year prior to the SEC regulation are classified as old (young). Dependent variable is the firm-level citation-weighted patent count. For variable descriptions, see the Appendix. Standard errors, reported in parentheses, are based on clustering at the firm level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

| | $Log(Citations_{it})$ |
|---|---|
| $High\ age\ Treat_i \times Post_t$ | –0.066*** |
| | (0.026) |
| $Low\ age\ Treat_i \times Post_t$ | –0.187*** |
| | (0.026) |
| Difference | 0.121*** |
| | (0.046) |
| Firm fixed effects | Yes |
| Year fixed effects | Yes |
| Firm-level controls | Yes |
| Observations | 47,326 |
| R-squared | 0.820 |

## Table 8: Explorative versus exploitative innovation

This table models the effect of the SEC regulation on explorative and exploitative patents separately. *Log(NumExplore$_{it}$)* (*Log(NumExploit$_{it}$)*) represents natural logarithm of one plus the count of explorative (exploitative) patents filed by firm *i* in year *t*. *Treat* is an indicator for above-median average ownership by mutual funds affected by the SEC regulation in the periods prior to the regulation. *Post* is an indicator for periods after the SEC regulation. Columns (1) and (2) present results from independent estimations of the models for explorative and exploitative patents, while columns (3) and (4) present results from joint estimation using maximum likelihood. For variable descriptions, see the Appendix. Standard errors, reported in parentheses, are based on clustering at the firm level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

| | Independent estimation | | Joint Estimation using Maximum Likelihood | |
|---|---|---|---|---|
| | *Log(NumExplore$_{it}$)* | *Log(NumExploit$_{it}$)* | *Log(NumExplore$_{it}$)* | *Log(NumExploit$_{it}$)* |
| | (1) | (2) | (3) | (4) |
| *Treat$_i$ × Post$_t$* | –0.160*** | –0.078*** | –0.138*** | –0.074*** |
| | (0.016) | (0.014) | (0.014) | (0.012) |
| *Test of difference in estimates* | | | | |
| Diff (Col (3) – Col (4)) | | | –0.064*** | |
| | | | (0.011) | |
| Firm fixed effects | Yes | Yes | Yes | |
| Year fixed effects | Yes | Yes | Yes | |
| Firm-level controls | Yes | Yes | Yes | |
| Observations | 47,326 | 47,326 | 47,326 | |
| R-squared | 0.835 | 0.858 | | |
| Log Likelihood | | | -56,021.650 | |

## Table 9: Sensitivity of fund flows to information in portfolio holdings

This table presents evidence on the effect of the SEC regulation on the sensitivity of fund flows to presence of losing and winning stocks in portfolio holdings by presenting ordinary least square estimates of equation (3). Dependent variable is the mutual fund flows measured over the two quarters subsequent to the quarter end *t* for which *%Losers* and *%Winners* are measured. *TreatFund* is an indicator variable that equals one for mutual funds that increased their disclosure frequency following the regulation, and zero for mutual funds unaffected by the regulation. *%Losers (%Winners)* is the percentage of assets of a mutual fund invested at the end of quarter *t* in losing (winning) stocks as defined in Section 7 of the paper. For other variable descriptions, see the Appendix. Standard errors, reported in parentheses, are based on clustering at the fund level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| <u>*Triple interactions capturing DiD Estimates:*</u> | | | | | | |
| *TreatFund × %Losers × Post* | –0.278** | | –0.268** | | –0.279** | |
| | (0.125) | | (0.125) | | (0.125) | |
| *TreatFund × %Losers × Post(1-2)* | | –0.261** | | –0.251* | | –0.262** |
| | | (0.131) | | (0.130) | | (0.131) |
| *TreatFund × %Losers × Post(3-5)* | | –0.263* | | –0.251* | | –0.262* |
| | | (0.135) | | (0.134) | | (0.135) |
| *TreatFund × %Winners × Post* | –0.100 | | –0.105 | | –0.105 | |
| | (0.098) | | (0.098) | | (0.098) | |
| *TreatFund × %Winners × Post(1-2)* | | –0.147 | | –0.152 | | –0.152 |
| | | (0.106) | | (0.105) | | (0.105) |
| *TreatFund × %Winners × Post(3-5)* | | –0.061 | | –0.067 | | –0.066 |
| | | (0.103) | | (0.103) | | (0.102) |
| <u>*Other terms in the model:*</u> | | | | | | |
| *%Losers* | –0.244*** | –0.245*** | –0.215*** | –0.216*** | –0.232*** | –0.233*** |
| | (0.078) | (0.078) | (0.078) | (0.078) | (0.078) | (0.078) |
| *TreatFund × %Losers* | 0.207** | 0.208** | 0.201** | 0.202** | 0.212** | 0.213** |
| | (0.097) | (0.097) | (0.097) | (0.097) | (0.097) | (0.097) |
| *%Losers × Post* | 0.129 | | 0.127 | | 0.114 | |
| | (0.108) | | (0.109) | | (0.109) | |
| *%Losers × Post(1-2)* | | 0.082 | | 0.087 | | 0.062 |
| | | (0.115) | | (0.115) | | (0.115) |
| *%Losers × Post(3-5)* | | 0.162 | | 0.155 | | 0.154 |
| | | (0.116) | | (0.116) | | (0.116) |
| *% Winners* | 0.137** | 0.137** | 0.120** | 0.121** | 0.120** | 0.120** |
| | (0.061) | (0.061) | (0.061) | (0.061) | (0.061) | (0.061) |
| *TreatFund× % Winners* | 0.029 | 0.029 | 0.036 | 0.036 | 0.032 | 0.032 |
| | (0.076) | (0.076) | (0.076) | (0.076) | (0.075) | (0.075) |
| *%Winners × Post* | –0.070 | | –0.077 | | –0.066 | |
| | (0.084) | | (0.083) | | (0.083) | |
| *%Winners × Post(1-2)* | | –0.040 | | –0.034 | | –0.036 |
| | | (0.090) | | (0.090) | | (0.090) |
| *%Winners × Post(3-5)* | | –0.093 | | –0.112 | | –0.089 |
| | | (0.088) | | (0.088) | | (0.088) |
| *TreatFund × Post* | 0.029 | 0.027 | 0.028 | 0.027 | 0.031 | 0.029 |
| | (0.034) | (0.034) | (0.034) | (0.034) | (0.034) | (0.034) |
| <u>*Control Variables:*</u> | | | | | | |

| | | | | | | |
|---|---|---|---|---|---|---|
| *AlphaBot* | 0.359*** | 0.359*** | 0.519*** | 0.518*** | 0.336*** | 0.336*** |
| | (0.113) | (0.113) | (0.110) | (0.110) | (0.088) | (0.088) |
| *AlphaBot × Post* | 0.312* | 0.317* | 0.325** | 0.332** | 0.141 | 0.150 |
| | (0.163) | (0.163) | (0.160) | (0.160) | (0.136) | (0.136) |
| *AlphaMid* | 0.690*** | 0.690*** | 0.859*** | 0.859*** | 0.688*** | 0.688*** |
| | (0.161) | (0.161) | (0.153) | (0.153) | (0.164) | (0.164) |
| *AlphaMid × Post* | 0.181 | 0.180 | 0.311 | 0.312 | 0.156 | 0.161 |
| | (0.237) | (0.237) | (0.238) | (0.238) | (0.234) | (0.234) |
| *AlphaTop* | 1.663*** | 1.661*** | 1.626*** | 1.625*** | 1.511*** | 1.510*** |
| | (0.144) | (0.144) | (0.137) | (0.137) | (0.146) | (0.146) |
| *AlphaTop × Post* | –1.194*** | –1.198*** | –0.955*** | –0.952*** | –0.978*** | –0.981*** |
| | (0.201) | (0.201) | (0.192) | (0.193) | (0.199) | (0.199) |
| *Fund Size* | –0.119*** | –0.119*** | –0.121*** | –0.121*** | –0.119*** | –0.119*** |
| | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) |
| *Load* | 0.019 | 0.019 | 0.019 | 0.019 | 0.019 | 0.020 |
| | (0.015) | (0.015) | (0.015) | (0.015) | (0.015) | (0.015) |
| *Expense ratio* | 0.247 | 0.252 | 0.280 | 0.287 | 0.074 | 0.077 |
| | (2.180) | (2.180) | (2.173) | (2.174) | (2.179) | (2.179) |
| | | | | | | |
| Alpha Measure used | 6–Factor | 6–Factor | 5–Factor | 5–Factor | 4–Factor | 4–Factor |
| Fund fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Time fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 54,360 | 54,360 | 54,360 | 54,360 | 54,360 | 54,360 |
| R-squared | 0.283 | 0.283 | 0.285 | 0.285 | 0.284 | 0.284 |

## Table 10: Effect of mandatory portfolio disclosure frequency on fund horizon

This table presents evidence on the effects of the May 2004 SEC regulation on the turnover of affected mutual funds. *Dur_CP* is a measure of investor duration developed by Cremers and Pareek (2014). *Churn_GMM* and *Churn_YZ* represent measures of fund-level turnover computed using the approaches specified in Gaspar, Massa, and Matos (2005) and Yan and Zhang (2009), respectively. *Churn_CRSP* is a measure of annual fund turnover provided by the CRSP mutual fund database. *TreatFund* is an indicator variable that equals one for mutual funds that increased their disclosure frequency following the regulation, and zero for mutual funds unaffected by the regulation. *Post(1-2)* is an indicator variable for observations that fall within two years after the SEC regulation. *Post(3-5)* is an indicator variable for observations from 3 to 5 years after the regulation. Standard errors, reported in parentheses, are based on clustering at the fund level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

| | $Log(Dur\_CP)_{jt}$ | | $Log(Churn\_GMM)_{jt}$ | | $Log(Churn\_YZ)_{jt}$ | | $Log(Churn\_CRSP)_{jt}$ | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| $TreatFund_j \times Post_t$ | –0.043* | | 0.020*** | | 0.008*** | | 0.033** | |
| | (0.023) | | (0.007) | | (0.002) | | (0.016) | |
| $TreatFund_j \times Post(1\text{-}2)$ | | –0.027 | | 0.018*** | | 0.008*** | | 0.031** |
| | | (0.022) | | (0.007) | | (0.002) | | (0.015) |
| $TreatFund_j \times Post(3\text{-}5)$ | | –0.058** | | 0.022*** | | 0.009*** | | 0.035* |
| | | (0.026) | | (0.008) | | (0.002) | | (0.019) |
| *Style distinctiveness*$_{jt}$ | –0.207*** | –0.380*** | 0.117*** | 0.106*** | 0.016** | –0.047*** | 0.200*** | 0.060*** |
| | (0.072) | (0.023) | (0.025) | (0.008) | (0.008) | (0.003) | (0.058) | (0.014) |
| *Active share*$_{jt}$ | –0.407*** | 0.075 | 0.118*** | –0.040 | –0.001 | –0.000 | 0.238*** | –0.002 |
| | (0.077) | (0.095) | (0.026) | (0.038) | (0.008) | (0.013) | (0.063) | (0.071) |
| *Load*$_{jt}$ | 0.032 | –0.207*** | –0.015** | 0.117*** | –0.006*** | 0.016** | –0.056*** | 0.200*** |
| | (0.021) | (0.072) | (0.007) | (0.025) | (0.002) | (0.008) | (0.016) | (0.058) |
| *Fund flows*$_{jt}$ | –0.380*** | –0.407*** | 0.106*** | 0.118*** | –0.047*** | –0.001 | 0.060*** | 0.238*** |
| | (0.023) | (0.077) | (0.008) | (0.026) | (0.003) | (0.008) | (0.014) | (0.063) |
| *Alpha (5 factor)*$_{jt}$ | 0.077 | 0.040*** | –0.040 | –0.014*** | –0.000 | –0.004*** | –0.002 | –0.026*** |
| | (0.095) | (0.007) | (0.038) | (0.002) | (0.013) | (0.001) | (0.071) | (0.005) |
| *Fund size*$_{jt}$ | 0.040*** | 0.031 | –0.014*** | –0.015** | –0.004*** | –0.006*** | –0.026*** | –0.056*** |
| | (0.007) | (0.021) | (0.002) | (0.007) | (0.001) | (0.002) | (0.005) | (0.016) |
| *Expense ratio*$_{jt}$ | –1.410 | –1.419 | –0.083 | –0.083 | 0.093 | 0.093 | 5.399** | 5.401** |
| | (3.253) | (3.256) | (1.078) | (1.079) | (0.312) | (0.312) | (2.358) | (2.359) |
| Fund and Year Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 13,393 | 13,393 | 13,668 | 13,668 | 13,668 | 13,668 | 13,489 | 13,489 |
| R-squared | 0.810 | 0.810 | 0.663 | 0.663 | 0.593 | 0.593 | 0.827 | 0.827 |

**Internet Appendix for**

**"Mutual Fund Transparency and Corporate Myopia"**

In this appendix, we provide several robustness tests and describe additional analyses that document mutual fund trading behavior consistent with increased short-term focus following the regulation. For each of the sections below we pinpoint the relevant section in the paper so that it is easier for the reader to connect the paper with this appendix.

## 1. Robustness to truncation bias (Refer section 4.1 in the paper)

Although we adjust the innovation measures for truncation bias using the approaches suggested in prior work (Hall, Jaffe, and Trajtenberg, 2001, 2005), recent research (Lerner and Seru, 2015; Dass, Nanda, and Xiao, 2017) suggests that these approaches may not be adequate. Therefore, we conduct a series of additional tests to ensure that truncation biases do not drive our findings. First, following the recommendation in Dass, Nanda, and Xiao (2017), we drop data for the last three years before the end date of the patent data. While this test provides estimates that are less likely to be affected by truncation bias, a disadvantage is that it will underestimate the effect of regulation had the innovation output declined gradually. We, therefore, view estimates from these tests as the lower bounds of the effect of regulation. Table IA1, Panel B, column (1) presents the results for our main measure of innovation (citation-weighted patent count). As expected, the DiD estimate is lower (coefficient estimate = −0.112), but it is still both economically and statistically significant at 1% level.

Second, we construct an alternative measure of citation-weighted patent counts in which we only consider citations that occur only in a three-year window following the date a patent is granted (*Citations3Yr*). *Citations3yr* is identical to our main measure *Citations* in all respects except the constrained citation accumulation window of 3 years. Bernstein (2015) notes that citations are concentrated in first few years and there is considerable serial correlation in citation rates. By imposing an identical window for accumulation of citations both pre- and post-regulatory shock for both treatment and control firms, this approach mitigates concerns about results being driven by truncation biases. This approach is recommended in Lerner and Seru (2015) and has been used by Lerner, Sorensen, and Strömberg (2011), Bernstein (2015), and Bernstein, Giroud, and Townsend (2016). Because we need a three-year window to accumulate citations, the sample for this analysis ends 3 years before the end of patent database in 2010. Estimates in Panel B, column (2) presents the results for this analysis. The DiD estimate of innovation decline continues to be economically large (coefficient = –0.236) and statistically significant ($p$-value < 0.01).

In our next set of tests, we construct a time-varying firm-level measure of the expected truncation bias (*BiasinCites*) to address this concern. To construct *BiasinCites*, we combine patent data from NBER patent database that ends in year 2006 with the patent data from Kogan et al. (2012) that ends in 2010. Under the assumption that the Kogan et al. (2012) data provides almost bias-free patent data for years till 2005 (i.e., for years that are at least five years away from the end year), we can take the difference between citation counts using Kogan et al. (2012) data and NBER data for years 2005 and before to obtain a firm-level measure of bias. For example, to measure the expected bias for a firm in year 2008 (i.e., 2 years away from the truncation year of 2010), we take the difference between citations for patents applied by that firm in year 2004 (i.e., 2 years from truncation in NBER database) using Kogan et al. (2012) data and NBER data.

We first exploit this measure by estimating the DiD specification on a entropy balanced sample of treatment and control firms on truncation bias 1 to 5 years away from the database ending date. Table IA1, Panel A, shows that the mean bias measures for both treatment and control firms are nearly identical after entropy balancing. Because both treatment and control firms are similarly susceptible to truncation biases, the DiD estimates on this sample are unlikely to be affected by truncation biases. Estimates in Panel B, column (3) suggest that innovation decline is economically and statistically significant (coefficient = –0.178, $p$-value < 0.01).

Finally, we include *BiasinCites* and its squared term (to allow for any potential nonlinearities) as control variables in the regression estimation on the full sample. Estimates in Column (4) show that our estimates of innovation decline are similar to that obtained before (coefficient = –0.182, $p$-value < 0.01).

**2. Block Ownership as the treatment variable (Refer section 5.1 in the paper)**

In this section, we explore an alternative approach for identifying treatment firms based on ownership by one or more blockholders, who might have greater ability to directly intervene in investee firms' operational decisions. Specifically, we use an indicator variable for the presence of at least one large *individual* blockholder mutual fund affected by the regulation in the year prior to the regulation. While it is likely that large blockholders have greater incentives and ability to directly intervene in investee firms' operations, as we explained in the paper, we consider threat of exit as the more plausible mechanism through which short-term oriented fund managers affect corporate innovation. Under this mechanism, corporate managers decrease corporate innovation because of the fear of decline in stock prices resulting from the trading activity of fund managers who do not appreciate the long-run benefits of innovative investments. Edmans (2009) shows that such a threat of uninformed trading is lower when the firm is owned by large *individual* blockholders who, all else equal, have greater incentives to collect information about firms' prospects relative to investors with smaller holdings. He argues that collective uninformed trading by atomistic shareholders is more likely to induce myopic pressures on corporate managers relative to trading by large blockholders. As a result, our estimates of innovation decline using blockholder ownership could be muted.

We use 2% ownership threshold to classify a mutual fund as a blockholder. In a survey of institutional investors, McCahery, Sautner, and Starks (2016) find two-thirds of the institutional investors mentioning that 2% ownership levels can be sufficient to induce changes in corporate policies. In our sample, 39% of the firms have at least one blockholder prior to the regulation based on this definition. Table IA2, columns (1)–(3) present our main results using the blockholder indicator variable and it is evident that our inferences are robust, albeit muted compared to our results reported in Table 2. This is not surprising given that these blockholders have incentives to research the firm and become more informed thereby reducing the extent of short-termism. Estimates in columns (4)–(6) show that our inferences are also robust if we use a 1% ownership threshold to identify blockholders.

### 3. Measuring treatment firms using mutual fund ownership that includes ownership by funds that voluntarily reported portfolio holdings on a quarterly basis (Refer section 5.1 in the paper)

In this section, we use an alternative way to measure the treatment variable. Specifically, we identify treatment firms using combined ownership both by mutual funds that were forced to report on a quarterly basis following the regulation and funds that voluntarily reported on a quarterly basis prior to the regulation. The purpose of this analysis is to mitigate biases that might result from mandatory and voluntary funds choosing to own systematically different types of firms. While this approach mitigates biases originating from the endogeneity of the disclosure frequency choice, a downside is that it introduces measurement error in our classification of treatment and control firms. For example, a firm that has little mandatory fund ownership but very high voluntary ownership can get classified as a treatment firm. However, we would not expect such a firm to be affected by the SEC regulation. Nevertheless, we repeat the empirical analyses in the paper using a treatment indicator for above-median combined ownership by both mandatory and voluntary mutual funds on an average basis in the 5 years prior to the regulation. Tables IA3 through IA8 present the results from repeating all of our analyses presented in the paper using this alternative classification scheme for treatment and control firms.[1] Evident from theses tables, the tenor of our conclusions that are based on a treatment variable using only mandatory fund ownership is unaltered. However, our estimates of innovation decline across specifications are systematically lower when we use the combined ownership variable instead of just mandatory fund ownership, consistent with our expectation that classification errors would bias our estimates downward.

[1]It is worth noting that in the placebo tests using this alternative definition of treatment indicator, we cannot use mutual funds that voluntarily reported at a quarterly frequency before the SEC regulation as a placebo investor group (unlike Table 4 of the paper) because the treatment indicator is measured using combined ownership by mandatory and voluntary funds.

## 4. Matching estimators (Refer section 5.3 in the paper)

In this section, we examine the robustness of our results to difference-in-differences (DiD) matching estimators. Prior work shows that DiD estimation on matched samples (especially matching on pre-treatment outcomes) can be effective in removing bias caused by selection on unobservables. DiD matching estimators were proposed by Heckman, Ichimura, Smith, and Todd (1998); and Heckman, Ichimura, and Todd (1997) as an extension of matching robust to selection on unobservables. Intuitively, matching on pre-treatment outcome variables allows one to implicitly match on any unobservables that affect the outcome variables; thereby, reducing bias caused by unobservables. In their application of DiD matching estimators for evaluating the effectiveness of job training programs, Heckman, Ichimnura, Smith, and Todd (1998); and Heckman, Ichimura, and Todd (1997) find that adding past wages to matching variables significantly reduces the bias in matching estimators relative to an experimental benchmark. Based on the above work, we use DiD matching estimators (with matching on pre-treatment innovation output and other firm characteristics) to further examine if our inferences are driven by any unobserved differences in treatment and control firms would violate the parallel trends assumption.

Because inferences from this analysis depend crucially on the quality of the matching achieved, we rely on recent advances in matching technique instead of using the conventional propensity score matching method. Specifically, we estimate our DiD specifications on an entropy balanced sample of treatment and control firms (Hainmueller, 2012; Hainmueller and Xu, 2013). Entropy balancing is essentially a reweighting technique that represents a generalization of propensity score matching to achieve significantly improved covariate balance across treatment and control samples. Under the conventional propensity score matching approach, each control unit is assigned a weight of either zero or one (i.e., the unit is either retained or discarded). Instead of using this restrictive weighting scheme, entropy balancing assigns a continuous set of weights to control units to create a set of control counterfactuals that much more closely resemble treatment units. Moreover, this approach prevents loss of information and drop in sample size because most observations get assigned appropriate non-zero weights instead of being discarded. As discussed below, we exploit this benefit of entropy balancing to do extensive matching on innovation output in pre-periods, which would not be possible with conventional propensity score matching because of significant reductions in sample size.

Table IA9 presents the results of this analysis. We follow two matching approaches. In the first approach, we do the entropy balancing on control variables, industry membership (Fama and French (1997) 48-industry classification), and the citation-weighted patent counts in the year prior to the regulation. Panel A presents the covariate balance across the treatment and control groups after reweighting based on entropy balancing procedure. Consistent with expectations, the treatment and control means are virtually identical across all variables. Column (1) in Panel B presents the DiD estimates on the entropy balanced sample. The DiD estimate of the innovation decline continues to be economically and statistically significant (coefficient = –0.111; $p$-value < 0.01).

We next exploit the ability of entropy balancing to achieve high covariate balance without significant sample attrition by augmenting the list of matching variables to also include innovation output in all of the 5 years prior to the regulation. This approach ensures that treatment and control firms exhibit similar levels of innovation output and trends in all years prior to the innovation, allowing to also better control for any unobservable differences that are predictive of trends in innovation output. Again, Panel A shows that we obtain near perfect covariate balance across treatment and control firms across all variables; and, in column (2) of Panel B, we continue to find an economically and statistically significant estimate of innovation decline (coefficient = –0.117; $p$-value < 0.01).

**5. Ruling out retail ownership as a confounding variable (Refer section 5.4.2 in the paper)**

In this section, we attempt to rule out one possible confounding variable: changes in retail investor ownership. That is, we explore the possibility that, after the SEC regulation, treatment firms experience a decline in affected mutual fund ownership and an increase in retail investor ownership. Just as retail investors can cause fund managers to become short-term oriented, they could also cause corporate managers to become short-term oriented. To test this, we examine changes in percentage retail investor ownership (*RetailOwn*) and affected mutual fund ownership (*MFOwn*) by estimating a modified version of equation (1) with *RetailOwn* and *MFOwn* at the end of year *t* as the dependent variables. We measure retail ownership as one minus the total institutional ownership. Inconsistent with the alternative explanation, estimates in Table IA10 suggest that, if anything, treatment firms experience a slight decline in retail ownership (about 1.1%) and an increase in affected mutual fund ownership (about 0.5%) relative to control firms.

## 6. Some robustness tests for the analysis of fund flow sensitivity to losing stocks (Refer section 7.1 in the paper)

In this section, we present some robustness tests for the analysis of the sensitivity of mutual fund flows to the presence of losing stocks. We first present the robustness of the main results to the use of an alternative window for measuring fund flows. Specifically, Table IA11 presents the estimates from measuring mutual fund flows over the 3-month period after the maximum allowable delay of 60 days for the disclosure of portfolio holdings for quarter *t*. The DiD estimate of the sensitivity of fund flows to the disclosure of losing stocks continues to be negative and significant across all models. Second, in Table IA12 we present the robustness of the results to using two alternative models for computing fund alpha: Fama-French 3-factor model and CAPM. Again, the results are very similar.

## 7. Sensitivity of fund ownership to firm performance (Refer section 7.3 in the paper)

In this section we examine the effect of the SEC regulation on the sensitivity of mutual fund ownership to short-term corporate performance. If more frequent disclosures prompt mutual funds to become more impatient in the wake of poor short-run corporate performance, this should manifest in increased sensitivity of mutual fund ownership to firm performance. We test this using the following specification:

$$MFOwn_{it+1} = \alpha_i + \gamma_{t+1} + \beta_1\, Performance_{it} + \beta_2\, Performance_{it} \times Post_{it} + \varepsilon_{it+1} \quad \text{(IE1)}$$

where $Performance_{it}$ is either firm $i$'s stock return or return on assets for year t. $\alpha_i$ and $\gamma_{t+1}$ are firm and time fixed effects. The coefficient of interest is $\beta_2$, which provides an estimate of the change in the sensitivity of mutual fund ownership to firm performance for funds affected by the regulation. Table IA13 presents the results of this analysis. Regardless of the performance measure used, estimates using the full sample reported in columns (1) – (2) (for stock return) and columns (5) – (6) (for ROA) suggest that the coefficient on the interaction term is positive and significant at the 1% level, indicating that mutual fund ownership becomes more sensitive to firm performance following the regulation. To ensure that this result is not driven entirely by control firms with little mutual fund ownership, Table IA13 also reports results of estimating equation (IE1) only for the subsample of treatment firms. We continue to find increased sensitivity of mutual fund ownership to investee firm performance. These results support the interpretation that the increased impatience of mutual fund owners is a reason for corporate managers to reduce innovation activities following the SEC mandate.

**8. Changes in the nature of portfolio holdings (Refer section 7.3 in the paper)**

In this section, we explore whether mutual funds also change the nature of their portfolio holdings in response to the regulation by tilting their portfolios to less innovative firms. A priori it is not clear that we would expect such a shift in portfolio holdings in equilibrium following the regulation. On the one hand, the increased short-term focus would incentivize fund managers to assign greater portfolio weight to firms with less innovative and shorter-term-oriented corporate investment policies. On the other hand, given that corporate managers respond to this short-term focus of fund managers by reducing their investment in innovative projects, it is not obvious that fund managers would need to move away from these stocks in equilibrium.

For this analysis, we first create three fund-level measures that capture the extent of investment by the funds in innovative firms by value-weighting the innovation measures of all investee firms in each mutual fund portfolio. For example, let $Citations_{i,PreReg}$ denote the citation-weighted patent count for investee firm $i$ measured using the patents filed over the five-year period prior to the SEC regulation. We then weight each stock by the proportion of its market value in a given fund's portfolio for each period. That is, the fund-level innovation measure for fund *j* for year *t* is calculated as follows:

$$FundCitations_{jt} = \sum_{i=1}^{N} w_{ijt} Citations_{i,\mathrm{PreReg}} \tag{IE2}$$

where $w_{ijt}$ is the value weight of fund *j* in stock *i* at the end of year *t*. We create similar fund-level measures based on patent counts and citations per patent. Because the level of innovation of the investee firms is held constant based on the innovation output in years prior to the regulation, this measure changes over time only because of changes in the portfolio weights on investee firms with different level of innovation activity. We then estimate a DiD specification similar to equation (IE2) in the paper with the fund-level innovation measure as the dependent variable.

Table IA14 presents the results of this analysis. Estimates in columns (1) – (2) suggest no evidence of a significant change in the nature of portfolio holdings when we measure innovation using citation-weighted patent counts. When we, however, consider the components of citation-weighted patent counts separately (i.e., simple patent counts and citations per patent) in columns (3) – (6), we find evidence of funds tilting their portfolio holdings toward firms with fewer citations per patent following the regulation. This evidence on systematic reduction in portfolio weights on firms that work on more impactful innovation characterized by higher citations per patent further highlights the increased aversion of fund managers toward corporate investment in innovative projects following the SEC regulation.

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## Table IA1: Controlling for Truncation Bias in Citations Data

This table presents evidence on the robustness of the findings presented in Table 2 of the paper to four potential solutions to the truncation bias in the measurement of citation-weighted patent counts. See section 1 of this appendix for a description of the approaches. Standard errors, reported in parentheses, are based on clustering at the firm level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

*Panel A: Covariate balance on the bias matched estimation sample used in column (3) of Panel B*

| | Treatment Mean | Control Mean |
|---|---|---|
| Years from truncation | *BiasinCites* | *BiasinCites* |
| 1 | 17.34 | 17.39 |
| 2 | 15.97 | 16.01 |
| 3 | 10.9 | 10.92 |
| 4 | 7.778 | 7.787 |
| 5 | 5.281 | 5.28 |

*Panel B: Estimation Results*

| | Sample restricted to 2007<br>(1) | Citations in first three years only<br>(2) | Estimation on Bias matched sample<br>(3) | Control for Bias in Citations<br>(4) |
|---|---|---|---|---|
| | $Log(Citations_{it})$ | $Log(Citations3Yr_{it})$ | $Log(Citations_{it})$ | $Log(Citations_{it})$ |
| $Post_t \times Treat_i$ | –0.112*** | –0.236*** | –0.178*** | –0.182*** |
| | (0.019) | (0.019) | (0.031) | (0.019) |
| $Log(BiasinCites)_{it-1}$ | | | | –0.355*** |
| | | | | (0.016) |
| $(Log(BiasinCites))^2_{it-1}$ | | | | 0.060*** |
| | | | | (0.003) |
| Firm fixed effects | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes |
| Firm level controls | Yes | Yes | Yes | Yes |
| N | 40,589 | 40,589 | 40,335 | 47,326 |
| R-squared | 0.873 | 0.786 | 0.823 | 0.829 |

## Table IA2: Results with block ownership

This table presents ordinary least square estimates of equation (1) in the paper using blockholder ownership instead of average mutual fund ownership. Blockholder ownership (*Block*) is as indicator variable for treatment firms with at least one mutual fund owner affected by the regulation with 2% (or 1%) ownership in the year prior to the SEC regulation, and zero otherwise. For variable descriptions, see the Appendix in the paper. Standard errors, reported in parentheses, are clustered at the firm level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

| | *Block threshold=2%* | | | *Block threshold=1%* | | |
|---|---|---|---|---|---|---|
| | *Log(Citations$_{it}$)* | *Log(NumPat$_{it}$)* | *Log(CiteperPat$_{it}$)* | *Log(Citations$_{it}$)* | *Log(NumPat$_{it}$)* | *Log(CiteperPat$_{it}$)* |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $Post_t \times Block_i$ | –0.121*** | –0.079*** | –0.015** | –0.142*** | –0.096*** | –0.012* |
| | (0.022) | (0.019) | (0.007) | (0.021) | (0.018) | (0.006) |
| Firm fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm level controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 47,326 | 47,326 | 47,326 | 47,326 | 47,326 | 47,326 |
| R-squared | 0.819 | 0.871 | 0.737 | 0.819 | 0.871 | 0.737 |

**Table IA3: Mandatory portfolio disclosure and corporate innovation**

This table presents ordinary least square estimates of equation (1) in the paper relating corporate innovation to mutual fund ownership. Measures of innovation include: (i) citation-weighted patent counts (*Citations*), (ii) simple patent counts (*NumPat*), and (iii) citations per patent (*CiteperPat*). *Treat* is an indicator for above median combined average ownership both by mutual funds affected and unaffected by the SEC regulation in the periods prior to the regulation. *Post* is an indicator for periods after the SEC regulation. For variable descriptions, see the Appendix. Standard errors, reported in parentheses, are based on clustering at the firm level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

| | $Log(Citations_{it})$ | $Log(Citations_{it})$ | $Log(NumPat_{it})$ | $Log(CiteperPat_{it})$ |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $Post_t \times Treat_i$ | –0.211*** | –0.202*** | –0.084*** | –0.014** |
| | (0.021) | (0.020) | (0.018) | (0.007) |
| $Log(MVE)_{it}$ | | 0.034*** | 0.043*** | –0.003 |
| | | (0.007) | (0.006) | (0.003) |
| $ROA_{it}$ | | –0.091*** | –0.058** | –0.005 |
| | | (0.031) | (0.024) | (0.013) |
| $Q_{it}$ | | 0.006 | –0.011*** | 0.004** |
| | | (0.004) | (0.003) | (0.002) |
| $Q_{it-1}$ | | 0.020*** | –0.002 | 0.006 |
| | | (0.003) | (0.040) | (0.018) |
| $Cash_{it-1}$ | | 0.153*** | 0.347 | –0.226 |
| | | (0.047) | (0.221) | (0.182) |
| $Leverage_{it-1}$ | | –0.027 | 0.524* | –0.292 |
| | | (0.041) | (0.311) | (0.241) |
| $CapitalStock_{it-1}$ | | 0.289*** | –0.225 | –0.080 |
| | | (0.064) | (0.242) | (0.110) |
| $KZindex_{it-1}$ | | –0.000 | –0.000 | 0.000 |
| | | (0.000) | (0.001) | (0.000) |
| $Log(Age)_{it-1}$ | | –0.046* | –0.105*** | –0.004 |
| | | (0.027) | (0.034) | (0.012) |
| $Illiquidity_{it-1}$ | | 0.002*** | 0.009** | –0.003* |
| | | (0.000) | (0.004) | (0.002) |
| $Hindex_{it-1}$ | | 0.553*** | –0.154 | 0.229 |
| | | (0.183) | (0.422) | (0.183) |
| $(Hindex_{it-1})^2$ | | –0.463** | 0.028 | –0.079 |
| | | (0.191) | (0.165) | (0.054) |
| *NoPatent* | | | | –0.643*** |
| | | | | (0.012) |
| *Post *NoPatent* | | | | 0.231*** |
| | | | | (0.013) |
| Firm fixed effects | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes |
| Observations | 47,326 | 47,326 | 47,326 | 47,326 |
| R-squared | 0.817 | 0.819 | 0.888 | 0.743 |

## Table IA4: Assessing parallel trends and the persistence of innovation changes

This table presents evidence on the timing of the effects of the May 2004 SEC regulation on patenting activity by presenting ordinary least square estimates of a modified version of equation (1) with firms' citation-weighted patent count as the dependent variable. *Treat* is an indicator for above-median combined average ownership both by mutual funds affected and unaffected by the SEC regulation in the periods prior to the regulation. *Pre(1)* and *Pre(2)* are indicator variables for observations that fall during one and two years prior to the SEC regulation. *Post(1)* through *Post(5)* are indicator variables for observations that fall during one through five years after the SEC regulation. Control represents control variables in equation (1). Industry represents Fama-French 48 industry indicator variable. For other variable descriptions, see the Appendix of the paper. Standard errors, reported in parentheses, are based on clustering at the firm level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

| | $Log(Citations_{it})$ | $Log(Citations_{it})$ | $Log(Citations_{it})$ | $Log(Citations_{it})$ | $Log(Citations_{it})$ |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| $Pre2 \times Treat_i$ | 0.004 | 0.004 | 0.003 | –0.001 | –0.000 |
| | (0.015) | (0.017) | (0.016) | (0.017) | (0.017) |
| $Pre1 \times Treat_i$ | 0.008 | 0.003 | 0.003 | –0.001 | –0.001 |
| | (0.018) | (0.019) | (0.019) | (0.020) | (0.020) |
| $Post_t \times Treat_i$ | –0.214*** | –0.170*** | –0.179*** | –0.148*** | |
| | (0.023) | (0.023) | (0.023) | (0.023) | |
| $Post1 \times Treat_i$ | | | | | –0.064*** |
| | | | | | (0.023) |
| $Post2 \times Treat_i$ | | | | | –0.054** |
| | | | | | (0.025) |
| $Post3 \times Treat_i$ | | | | | –0.136*** |
| | | | | | (0.028) |
| $Post4 \times Treat_i$ | | | | | –0.261*** |
| | | | | | (0.032) |
| $Post5 \times Treat_i$ | | | | | –0.313*** |
| | | | | | (0.038) |
| $Controls_{t=1}$×year dummies | No | Yes | No | Yes | Yes |
| Industry × year dummies | No | No | Yes | Yes | Yes |
| Firm fixed effects | Yes | Yes | Yes | Yes | Yes |
| Firm-level controls | Yes | Yes | Yes | Yes | Yes |
| Observations | 47,326 | 47,326 | 47,326 | 47,326 | 47,326 |
| R-squared | 0.815 | 0.830 | 0.835 | 0.841 | 0.842 |

## Table IA5: Effects of ownership by funds unaffected by the SEC regulation

This table presents evidence from placebo-type analysis in which we compare the effect of the ownership by mutual funds affected by SEC regulation to the effect of ownership by investor groups whose portfolio disclosure frequency was unaffected by the regulation. We consider two investor groups: (i) all non-mutual fund investors (Non-MFs) and (ii) hedge funds. *Treat* is an indicator for above-median combined average ownership both by mutual funds affected and unaffected by the SEC regulation in the periods prior to the regulation. *Unaffected* is an indicator variable for above-median ownership by the respective unaffected investor groups. We cannot use mutual funds that voluntarily reported at a quarterly frequency before the SEC regulation as a placebo investor group in this analysis (unlike Table 4 of the paper) because the treatment indicator is measured using combined ownership by mandatory and voluntary funds. All estimates are obtained from modified versions of equation (1) in the paper (augmented with unaffected fund ownership variables and their interaction terms with *Post*) with citation-weighted patent counts as the dependent variable. For variable descriptions, see the Appendix of the paper. Standard errors, reported in parentheses, are based on clustering at the firm level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

| | Non-MFs<br>(1) | Hedge Funds<br>(2) |
|---|---|---|
| $Treat_i \times Post_t$ | –0.169*** | –0.169*** |
| | (0.023) | (0.026) |
| $Unaffected_i \times Post_t$ | –0.082*** | –0.059** |
| | (0.023) | (0.026) |
| **Difference** | **–0.0868**** | **–0.109**** |
| | **(0.040)** | **(0.047)** |

## Table IA6: Instrumental variable analysis using S&P500 membership as the instrument

This table presents two stage least square estimates of equation (2) of the paper relating corporate innovation to mutual fund ownership, using an indicator for S&P500 membership in the year prior to the SEC regulation as an instrumental variable for *Treat*. *Treat* is an indicator for above-median combined average ownership both by mutual funds affected and unaffected by the SEC regulation in the periods prior to the regulation. The dependent variable *ΔLog(Citations)* represents the difference between mean logged citation-weighted patent count in the periods after and periods before the 2004 SEC regulation. For other variable descriptions, see the Appendix of the paper. Standard errors, reported in parentheses, are based on clustering at the firm level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

| | First Stage | Second Stage |
|---|---|---|
| | $Treat_i$ | $\Delta Log(Citations)_i$ |
| $Treat_i$ | | –0.484*** |
| | | (0.132) |
| $S\&P\ 500_i$ | 0.299*** | |
| | (0.028) | |
| *Test of weak instrument* | | |
| Cragg-Donald Wald F-statistic | 117.807 | |
| (*p*-value) | (0.000) | |
| Firm-level controls | YES | YES |
| Observations | 3,468 | 3,468 |
| R-squared | 0.450 | 0.203 |

## Table IA7: Effect of CEO incentives

This table presents evidence on how the corporate innovation decline following the SEC regulation varies by CEO incentives. *CEODelta* represents the increase in value of the CEO's stock and option based portfolio for a 1% increase in stock price calculated using the methodology in Core and Guay (2002). *CEOTurnSens* is the sensitivity of CEO turnover to firm's stock price performance. This variable is measured at both the Fama-French 48 and Fama-French 10 industry level. *CEOTenure* is the number of years that the executive worked as a CEO at the firm. Dependent variable is firm-level citation-weighted patent counts. *Treat* is an indicator for above-median combined average ownership both by mutual funds affected and unaffected by the SEC regulation in the periods prior to the regulation. For all other variable descriptions, see the Appendix of the paper. Standard errors, reported in parentheses, are based on clustering at the firm level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

| | CEO Delta | CEO turnover sensitivity | | CEO Tenure |
|---|---|---|---|---|
| | | Fama-French 48 | Fama-French 10 | |
| | (1) | (2) | (3) | (4) |
| *Treat* × *Post* (*Below median group*) | –0.149*** | –0.133*** | –0.174*** | –0.253*** |
| | (0.030) | (0.035) | (0.021) | (0.037) |
| *Treat* × *Post* (*Above median group*) | –0.242*** | –0.269*** | –0.295*** | –0.179*** |
| | (0.037) | (0.046) | (0.039) | (0.029) |
| *Test of differences:* | | | | |
| *Diff* (*Above median – Below median*) | –0.093* | –0.136** | –0.121*** | –0.073 |
| | (0.048) | (0.058) | (0.044) | (0.048) |
| Firm fixed effects | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes |
| Firm-level controls | Yes | Yes | Yes | Yes |
| Observations | 29,455 | 25,496 | 46,071 | 28,518 |
| R-squared | 0.808 | 0.812 | 0.818 | 0.809 |

## Table IA8: Effect of fund manager age

This table presents evidence on the effect of age of the fund manager on the innovation decline following the SEC regulation. *High age Treat* (*Low age Treat*) is an indicator variable for above-median combined average ownership both by mutual funds affected and unaffected by the SEC regulation that are run by older (younger) fund managers. Fund managers with above- (below-) median age in the year prior to the SEC regulation are classified as old (young). Dependent variable is the firm-level citation-weighted patent count. For variable descriptions, see the Appendix of the paper. Standard errors, reported in parentheses, are based on clustering at the firm level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

| | $Log(Citations_{it})$ |
|---|---|
| $High\ age\ Treat_i \times Post_t$ | –0.050* |
| | (0.027) |
| $Low\ age\ Treat_i \times Post_t$ | –0.183*** |
| | (0.026) |
| Difference | 0.133*** |
| | (0.048) |
| Firm fixed effects | Yes |
| Year fixed effects | Yes |
| Firm-level controls | Yes |
| Observations | 47,326 |
| R-squared | 0.820 |

## Table IA9: Matched sample analysis

This table presents evidence using a matched sample of treatment and control firms. Firms with above- (below-) median average ownership by mutual funds affected by the SEC regulation in the periods prior to the regulation are considered as treatment (control) firms. The matched sample of treatment and control firms is created using entropy balanced matching approach following Hainmueller (2012). Under matching approach 1, firms are matched on all control variables and citation-weighted patent counts in the year prior to the regulation. Under matching approach 2, the list of matching variables is augmented to also include citation-weighted patent counts in all years prior to the regulation. Panel A provides the covariate balance while Panel B provides the OLS estimation. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

*Panel A: Covariate balance*

| | Matching approach 1 | | Matching approach 2 | |
|---|---|---|---|---|
| | Treatment Mean | Control Mean | Treatment Mean | Control Mean |
| $Citations_{T-1}$ | 10.63 | 10.62 | 11.22 | 11.21 |
| $Citations_{T-2}$ | | | 11.52 | 11.51 |
| $Citations_{T-3}$ | | | 11.32 | 11.32 |
| $Citations_{T-4}$ | | | 10.62 | 10.61 |
| $Citations_{T-5}$ | | | 10.74 | 10.74 |
| $Log(MVE)_{T-1}$ | 6.818 | 6.815 | 6.861 | 6.86 |
| $ROA_{T-1}$ | 0.109 | 0.1089 | 0.1111 | 0.1111 |
| $Q_{T-1}$ | 1.998 | 1.998 | 1.922 | 1.922 |
| $Cash_{T-1}$ | 0.1995 | 0.1995 | 0.1867 | 0.1867 |
| $Leverage_{T-1}$ | 0.2071 | 0.2071 | 0.2128 | 0.2128 |
| $CapStock_{T-1}$ | 0.2478 | 0.2478 | 0.2537 | 0.2536 |
| $KZindex_{T-1}$ | –7.799 | –7.797 | –7.179 | –7.178 |
| $Age_{T-1}$ | 18.25 | 18.24 | 20.04 | 20.03 |
| $Illiquidity_{T-1}$ | 0.3782 | 0.3854 | 0.3962 | 0.4 |
| $Hindex_{T-1}$ | 0.2341 | 0.2342 | 0.238 | 0.2381 |

**Table IA9 (continued)**

*Panel B: Estimation results*

| | Matching Approach 1 | Matching Approach 2 |
|---|---|---|
| | *Log(Citations$_{it}$)* | *Log(Citations$_{it}$)* |
| | (1) | (2) |
| $Post_t \times Treat_i$ | –0.111*** | –0.117*** |
| | (0.033) | (0.037) |
| Firm fixed effects | Yes | Yes |
| Year fixed effects | Yes | Yes |
| Firm level controls | Yes | Yes |
| Observations | 40,131 | 36,109 |
| R-squared | 0.846 | 0.849 |

## Table IA10: Changes in mutual fund ownership and retail ownership surrounding the SEC regulation

This table presents difference-in-differences estimates of changes in mutual fund ownership and retail ownership following the 2004 SEC regulation for treatment firms relative to control firms. The dependent variable, *MFOwn* represents the percentage of stock owned by mutual funds affected by the 2004 SEC regulation. *RetailOwn* represents retail ownership measured as 1 minus the total institutional ownership. *Treat* is an indicator for above-median average ownership by mutual funds affected by the SEC regulation in the periods prior to the regulation. *Post* is an indicator for periods after the SEC regulation. Standard errors, reported in parentheses, are based on clustering at the firm level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

| | $RetailOwn_{it}$ | $MFOwn_{it}$ |
|---|---|---|
| | (1) | (2) |
| $Treat_i \times Post_t$ | –0.011*** | 0.005*** |
| | (0.004) | (0.001) |
| Firm fixed effects | Yes | Yes |
| Year fixed effects | Yes | Yes |
| Observations | 46,271 | 46,350 |
| R-squared | 0.883 | 0.814 |

## Table IA11: Sensitivity of fund flows to losing stocks in portfolio holdings: Robustness to use of an alternative window for measurement of fund flows

This table presents evidence on the effect of the SEC regulation on the sensitivity of fund flows to presence of losing and winning stocks in portfolio holdings by presenting ordinary least square estimates of equation (3) in the paper. Dependent variable is the mutual fund flows measured for the 3-month period after the maximum allowable delay of 60 days within which holdings for quarter *t* are required to be disclosed. *TreatFund* is an indicator variable that equals one for mutual funds that increased their disclosure frequency following the regulation, and zero for mutual funds unaffected by the regulation. *%Losers (%Winners*) is the percentage of assets of a mutual fund invested at the end of quarter *t* in losing (winning) stocks as defined in section 7 of the paper. For other variable descriptions, see the Appendix of the paper. Standard errors, reported in parentheses, are based on clustering at the fund level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

| | (1) | (2) | (3) |
|---|---|---|---|
| <u>*Triple interactions capturing DiD Estimates:*</u> | | | |
| *TreatFund × %Losers × Post* | –0.111** | –0.106** | –0.110** |
| | (0.051) | (0.051) | (0.051) |
| *TreatFund × %Winners × Post* | –0.028 | –0.030 | –0.030 |
| | (0.041) | (0.041) | (0.041) |
| <u>*Other terms in the model:*</u> | | | |
| *%Losers* | –0.125*** | –0.113*** | –0.122*** |
| | (0.033) | (0.033) | (0.033) |
| *TreatFund × %Losers* | 0.106*** | 0.103*** | 0.108*** |
| | (0.040) | (0.040) | (0.040) |
| *%Losers × Post* | 0.054 | 0.058 | 0.051 |
| | (0.044) | (0.044) | (0.044) |
| *% Winners* | 0.053** | 0.046* | 0.048* |
| | (0.027) | (0.027) | (0.027) |
| *TreatFund× % Winners* | 0.017 | 0.019 | 0.018 |
| | (0.033) | (0.033) | (0.033) |
| *%Winners × Post* | –0.046 | –0.051 | –0.047 |
| | (0.035) | (0.035) | (0.035) |
| *TreatFund × Post* | 0.012 | 0.011 | 0.012 |
| | (0.014) | (0.014) | (0.014) |
| <u>*Control Variables:*</u> | | | |
| *AlphaBot* | 0.095 | 0.163*** | 0.091 |
| | (0.064) | (0.063) | (0.061) |
| *AlphaBot × Post* | 0.201** | 0.252*** | 0.161* |
| | (0.089) | (0.086) | (0.084) |
| *AlphaMid* | 0.248*** | 0.368*** | 0.269*** |
| | (0.078) | (0.074) | (0.080) |
| *AlphaMid × Post* | 0.248** | 0.201* | 0.183 |
| | (0.118) | (0.119) | (0.122) |
| *AlphaTop* | 0.641*** | 0.636*** | 0.541*** |
| | (0.066) | (0.062) | (0.071) |
| *AlphaTop × Post* | –0.423*** | –0.300*** | –0.307*** |
| | (0.088) | (0.085) | (0.091) |

| | | | |
|---|---|---|---|
| *Fund Size* | –0.045*** | –0.045*** | –0.044*** |
| | (0.002) | (0.002) | (0.002) |
| *Load* | 0.007 | 0.007 | 0.008 |
| | (0.006) | (0.006) | (0.006) |
| *Expense ratio* | 0.427 | 0.445 | 0.362 |
| | (0.902) | (0.898) | (0.905) |
| | | | |
| *%Losers + TreatFund × %Losers* | –0.019 | –0.010 | –0.014 |
| | (0.025) | (0.025) | (0.025) |
| Alpha Measure used | 6–Factor | 5–Factor | 4–Factor |
| Fund fixed effects | Yes | Yes | Yes |
| Time fixed effects | Yes | Yes | Yes |
| Observations | 54,547 | 54,547 | 54,547 |
| R-squared | 0.209 | 0.211 | 0.209 |

## Table IA12: Sensitivity of fund flows to losing stocks in portfolio holdings: Robustness to use of fund alpha based on Fama–French 3–factor model and CAPM

This table presents evidence on the effect of the SEC regulation on the sensitivity of fund flows to presence of losing and winning stocks in portfolio holdings by presenting ordinary least square estimates of equation (3) in the paper. Dependent variable is the mutual fund flows measured over the two quarters subsequent to the quarter end *t* for which *%Losers* and *%Winners* are measured. *TreatFund* is an indicator variable that equals one for mutual funds that increased their disclosure frequency following the regulation, and zero for mutual funds unaffected by the regulation. *%Losers (%Winners*) is the percentage of assets of a mutual fund invested at the end of quarter *t* in losing (winning) stocks as defined in section 7 of the paper. For other variable descriptions, see the Appendix of the paper. Standard errors, reported in parentheses, are based on clustering at the fund level. Statistical significance (two–sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

| | (1) | (2) |
|---|---|---|
| *Triple interactions capturing DiD Estimates:* | | |
| *TreatFund × %Losers × Post* | –0.252** | –0.248** |
| | (0.125) | (0.125) |
| *TreatFund × %Winners × Post* | –0.118 | –0.104 |
| | (0.098) | (0.098) |
| *Other terms in the model:* | | |
| *%Losers* | –0.142* | –0.112 |
| | (0.079) | (0.078) |
| *TreatFund × %Losers* | 0.189** | 0.185* |
| | (0.096) | (0.096) |
| *%Losers × Post* | 0.052 | 0.079 |
| | (0.110) | (0.111) |
| *% Winners* | 0.079 | 0.003 |
| | (0.061) | (0.060) |
| *TreatFund× % Winners* | 0.052 | 0.040 |
| | (0.075) | (0.075) |
| *%Winners × Post* | –0.067 | –0.038 |
| | (0.084) | (0.084) |
| *TreatFund × Post* | 0.030 | 0.027 |
| | (0.034) | (0.034) |
| *Control Variables:* | | |
| *AlphaBot* | 0.580*** | 0.383*** |
| | (0.094) | (0.096) |
| *AlphaBot × Post* | 0.192 | 0.374*** |
| | (0.139) | (0.145) |
| *AlphaMid* | 0.876*** | 1.012*** |
| | (0.154) | (0.117) |
| *AlphaMid × Post* | –0.051 | –0.348** |
| | (0.233) | (0.172) |
| *AlphaTop* | 1.754*** | 1.486*** |
| | (0.154) | (0.117) |

| | | |
|---|---|---|
| | – | – |
| *AlphaTop × Post* | 1.064*** | 0.630*** |
| | (0.205) | (0.158) |
| | – | – |
| *Fund Size* | 0.120*** | 0.118*** |
| | (0.006) | (0.006) |
| *Load* | 0.018 | 0.019 |
| | (0.015) | (0.015) |
| *Expense ratio* | 0.011 | –0.171 |
| | (2.173) | (2.187) |
| | | |
| *%Losers + TreatFund × %Losers* | 0.047 | 0.073 |
| | (0.064) | (0.064) |
| Alpha Measure used | 3–Factor | CAPM |
| Fund fixed effects | Yes | Yes |
| Time fixed effects | Yes | Yes |
| Observations | 54,360 | 54,360 |
| R–squared | 0.286 | 0.287 |

## Table IA13: Sensitivity of mutual fund ownership to firm performance

This table presents estimates of equation (IE1) to examine the changes in the sensitivity of mutual fund ownership to firm performance following the 2004 SEC regulation. The dependent variable, *MFOwn*, represents the percentage of stock owned by mutual funds affected by the regulation. Firm performance is measured using either lagged annual stock return or return on assets. *Post(1-2)* is an indicator variable for observations that fall within two years after the SEC regulation. *Post(3-5)* is an indicator variable for observations from 3 to 5 years after the regulation. Standard errors, reported in parentheses, are based on clustering at the firm level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

| Performance Measure | Stock Return | | | | Return on Assets | | | |
|---|---|---|---|---|---|---|---|---|
| | Full Sample | | Treatment Firms | | Full Sample | | Treatment Firms | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *Performance$_{it}$* | 0.004*** | 0.004*** | 0.007*** | 0.007*** | 0.004*** | 0.004*** | 0.010*** | 0.010*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.001) | (0.001) | (0.003) | (0.003) |
| *Performance$_{it}$ × Post* | 0.007*** | | 0.009*** | | 0.032*** | | 0.059*** | |
| | (0.001) | | (0.001) | | (0.002) | | (0.006) | |
| *Performance$_{it}$× Post(1-2)* | | 0.004*** | | 0.005*** | | 0.030*** | | 0.056*** |
| | | (0.001) | | (0.002) | | (0.002) | | (0.006) |
| *Performance$_{it}$× Post(3-5)* | | 0.010*** | | 0.013*** | | 0.034*** | | 0.062*** |
| | | (0.001) | | (0.002) | | (0.003) | | (0.007) |
| Firm Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 44,294 | 44,294 | 21,796 | 21,796 | 46,349 | 46,349 | 22,489 | 22,489 |
| R-squared | 0.824 | 0.824 | 0.698 | 0.699 | 0.818 | 0.818 | 0.687 | 0.687 |

## Table IA14: Do funds move to less innovative firms?

This table examines whether mutual funds shift their portfolio holdings toward less innovative firms by estimating the changes in fund-level measures of innovation created by value-weighting innovation measures of all investee firms in the mutual fund's portfolio, where the innovation activity of the investee firms is measured over the five-year period prior to the regulation. These fund-level innovation measures are labeled as *FundCitations*, *FundNumPat*, and *FundCiteperPat* and are obtained by value-weighting the citation counts, patent counts, and citations per patent of the investee firms, respectively. *TreatFund* is an indicator variable that equals one for mutual funds that increased their disclosure frequency following the regulation, and zero for mutual funds unaffected by the regulation. *Post(1-2)* is an indicator variable for observations that fall within two years after the SEC regulation. *Post (3-5)* is an indicator variable for observations from 3 to 5 years after the regulation. For variable descriptions, see the Appendix of the paper. Standard errors, reported in parentheses, are based on clustering at the fund level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

| | $Log(FundCitations)_{jt}$ | | $Log(FundNumPat)_{jt}$ | | $Log(FundCiteperPat)_{jt}$ | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $TreatFund_j \times Post_t$ | –0.026 | | –0.017 | | –0.012* | |
| | (0.038) | | (0.035) | | (0.006) | |
| $TreatFund_j \times Post(1\text{-}2)$ | | –0.015 | | –0.005 | | –0.009 |
| | | (0.036) | | (0.034) | | (0.006) |
| $TreatFund_j \times Post(3\text{-}5)$ | | –0.034 | | –0.026 | | –0.014** |
| | | (0.043) | | (0.040) | | (0.007) |
| Fund Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 17,527 | 17,527 | 17,527 | 17,527 | 17,527 | 17,527 |
| R-squared | 0.926 | 0.926 | 0.932 | 0.932 | 0.854 | 0.854 |