Frequent Financial Reporting and Managerial Myopia

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Abstract: Using the transition of US firms from annual reporting to semi-annual reporting and then to quarterly reporting over the period 1950-1970, we provide evidence on the effects of increased reporting frequency on firms' investment decisions. Estimates from difference-in-differences specifications indicate that increased reporting frequency is associated with an economically large decline in investments. Additional analyses reveal that the decline in investments is most consistent with frequent financial reporting inducing myopic management behavior. Our evidence informs the recent controversial debate about eliminating quarterly reporting for US corporations.

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Frequent Financial Reporting and Managerial Myopia

1. Introduction

Corporate managers and practitioners often lament that frequent disclosure of financial reports (e.g., quarterly) causes investors and firms to become too focused on short term performance, resulting in myopic investment decisions.[1] For example, citing concerns about losing focus on its long term goals, Google (around its IPO in 2004) refused to provide quarterly guidance to analysts. Similarly, Unilever's CEO, Paul Polman, famously stopped the practice of issuing quarterly reports since 2009 and notes the following on the benefits of doing so:[2]

> "*Better decisions are being made. We don't have discussions about whether to postpone the launch of a brand by a month or two or not to invest capital, even if investing is the right thing to do, because of quarterly commitments*."

Regulators and lawmakers have also expressed reservations about quarterly reporting. Concerned about myopic behavior, in 2004 the EU parliament rejected a proposal to mandate quarterly reporting of financial statements and eventually adopted a transparency directive that only required interim narrative disclosures around first and third quarters. Furthermore, in an attempt to reduce short-term pressure on managers, in 2013 the EU eliminated even this interim reporting requirement altogether (Wagenhofer, 2014). Many have recommended that the US follow suit to remove the quarterly reporting requirement (Benot, 2015, *Wall Street Journal*). In support of these arguments, recent theoretical studies (Hermalin and Weisbach, 2012; Gigler et al., 2014; Edmans et al., 2016) suggest that greater disclosure can indeed cause managers to make myopic investment choices.

Despite these concerns, there are reasons to believe that increased public disclosure could mitigate corporate myopia and encourage investment in long term projects. For example, it could

[1] In their influential survey, Graham et al. (2005) note many CFOs deploring the culture of meeting quarterly targets and saying that it inhibits them from thinking about long-term growth; also, 78% note that they would be willing to sacrifice value in order to meet quarterly earnings target.
[2] See the commentary entitled "Business, society, and the future of capitalism" in Polman (2014).

improve firms' access to financing by reducing informational frictions between firms and capital providers, allowing the firm to invest in a larger set of positive NPV projects. Additionally, the increased transparency could improve monitoring and disciplining of corporate managers, mitigating agency problems, particularly, the reluctance on the part of managers to invest in long term projects. Thus, whether greater reporting frequency encourages or mitigates corporate myopia is ultimately an open empirical question that we explore in this study.

Empirically identifying the effect of reporting frequency on investment decisions is a challenging task. In the US, there is currently no cross-sectional variation in reporting frequency because the SEC regulation requires all publicly listed firms to report on a quarterly basis. In the international setting, while there is variation in reporting frequencies across countries it is difficult to separate the effect of reporting frequency from that of other features of countries' institutional and regulatory environments.

We consider a different setting that exploits the variation in US firms' reporting frequencies over an earlier time period 1950-1970. The SEC required annual reporting of financial statements in 1934, changed the required frequency to semi-annual reporting in 1955, and eventually changed to quarterly reporting in 1970. What is particularly helpful for our empirical identification is that many firms were forced to report at quarterly frequency even before the SEC mandate because of the more stringent reporting requirements imposed by some of the stock exchanges. For example, in 1929, the NYSE asked all firms to amend their listing agreement to commit to quarterly reporting.[3] The AMEX and the regional exchanges, however, were not supportive of quarterly reporting; these exchanges softened their stance only in 1962, requiring newly listed firms to report quarterly and pressuring already-listed firms do so. The staggered timing of the change in reporting frequency gives us a natural group of control firms to implement a difference-in differences (DiD) design in which we

[3] Butler et al. (2007) note that 90% of the active domestic firms on NYSE were complying with this requirement before the first SEC mandate in 1955.

compare the change in investments of treatment firms around a reporting frequency increase relative to the contemporaneous change in investments for the control firms with unchanged reporting frequency. This design mitigates concerns about the effect of unobserved common shocks or cross-sectional differences across firms.

Our DiD estimates suggest that firms significantly reduce investments in fixed assets following an increase in reporting frequency.[4] The decline is economically meaningful in that we observe a reduction of approximately 1.5% to 1.9% of total assets, which roughly corresponds to 15% to 21% of the standard deviation of investments in our sample. Moreover, the reduction in investments is persistent for at least 5 years, and is robust to use of several alternative matching procedures and sample selections.

Under the assumption that treatment and control firms share parallel trends in investments, absent changes in reporting frequency, the DiD estimates represent the causal effect of increased reporting frequency (Angrist and Pischke, 2009). In support of the parallel trends assumption, we find that changes in investment levels of treatment and control firms prior to the reporting frequency increases are indeed indistinguishable. Nonetheless, an important concern, as in any DiD setting, is whether the parallel trends would have continued in the post-treatment period absent any changes in reporting frequency. Such a violation of parallel trends assumption could occur if, for example, reporting frequency changes systematically coincide with declines in growth opportunities. Under such a scenario, investments for treatment and control firms would diverge even without the change in reporting frequency and the DiD estimate would be contaminated by the effect of concurrent changes in growth opportunities. We, however, think this is unlikely because the timing of the reporting frequency changes for our treatment firms is exogenously imposed by the SEC or the stock

[4] As we elaborate in Section 2, fixed asset investments map well into the theory underlying reporting frequency and myopic underinvestment. Moreover, both survey based evidence and large-sample archival research finds that managerial myopia can manifest in the form of underinvestment in fixed assets. We also considered using R&D, but data challenges preclude us from using this variable.

exchanges. Furthermore, we find that inclusion of controls for time-varying firm characteristics results in little change in the estimated effect of the reporting frequency increase, suggesting that the reporting frequency shocks are close to random at firm level. Our results are also robust to inclusion of state-year or even industry-year interactive fixed effects, each of which flexibly absorb the effect of any time-varying shocks at the industry or state level that could coincide with reporting frequency increases.

The investment decline is consistent with two possible, although not mutually exclusive, effects of reporting frequency. First, it could reflect myopic underinvestment by managers because of amplified capital market pressures induced by frequent reporting (myopia channel). Alternatively, it could represent a correction of previous excess investments by managers due to the discipline imposed by frequent reporting (disciplining channel). We conduct two sets of tests to assess the relative effects of the two channels.

First, we examine the operating performance around the reporting frequency increase. If the investment decline reflects a reduction in prior overinvestment, then firms should be able to produce prior levels of economic output using fewer resources, leading to greater future productivity. In contrast, because of forgone attractive investment opportunities, the myopia channel predicts lower growth and productivity. Consistent with the myopia channel, we find evidence of a decline in productivity (measured using asset turnover and ROA) and sales growth subsequent to the reporting frequency increase.

Second, we test a direct implication of the myopia theory by examining the effect of the lag with which benefits of investments flow into earnings. Theory suggests that frequent reporting is more likely to induce myopic underinvestment in industries in which capital investments generate value over longer horizons and can therefore appear as not being sufficiently profitable based on quarterly earnings performance for the first few quarters. Consistent with this prediction, we find that the investment decline following the reporting frequency increases is driven primarily by industries in which capital investments generate future earnings with a longer lag. Moreover, in placebo tests we find no evidence of decline in other investments (e.g., accounts receivables, inventories, cash, and marketable securities) that are likely to yield benefits over relatively shorter horizons.[5]

Collectively, the evidence suggests that a significant portion of the investment decline stemming from increased reporting frequency is due to managerial myopia. The magnitudes of the investment decline that we document are in line with other studies that find evidence of myopic underinvestment in other settings.[6]

Our paper makes three contributions to extant literature and practice. First, we contribute to the growing stream of research that examines the role of capital market features, governance, and ownership on managerial myopia. For example, Edmans et al. (2017) and Ladika and Sautner (2015) examine the role of equity incentives, Asker et al. (2015) and Bernstein (2015) examine the role of public ownership, He and Tian (2013) examine the role of financial analysts, Fang et al. (2014) examine the role of stock liquidity, Aghion et al. (2013) and Bushee (1998) examine the role of institutional investors, and Atanassov (2013) examines the roles of antitakeover laws. Our study highlights the role of an important policy choice variable – the frequency of financial reporting – in motivating myopic managerial behavior.

Second, we contribute to the work on economic consequences of mandated public information disclosure. Prior research suggests that increased transparency is beneficial through improved liquidity and reduced cost of capital (e.g., Leuz and Verrecchia, 2000; Fu et al., 2012). Our study provides evidence that increased mandated disclosure can also have adverse real effects in the form of myopic management behavior. Our work is complementary to two recent studies by Ernstberger et al. (2016) and Nallareddy et al. (2016) who provide evidence on the consequences of a

[5] Myopia theories also predict greater underinvestment when corporate managers care more about short term performance, perhaps because of career concerns, stock based compensation, takeover threat, or presence of impatient investors. We are unable to measure these incentives because of lack of data during our sample period.
[6] Asker et al. (2015) find that private firms out-invest public firms by about 3.7% to 6.8% of assets. They conclude that this underinvestment by public firms is attributable to managerial myopia caused by stock market listing.

different form of quarterly reporting requirement. Specifically, these studies examine the effect of quarterly disclosure of interim management statements (IMS) required under the 2004 transparency directive adopted by the EU. However, unlike the quarterly reporting requirement in our setting, their setting of IMS requires firms to provide only *narrative* disclosures on the financial position and performance of the firm rather than mandating disclosure of financial statement information.[7] The evidence from these studies suggests that while requiring disclosure of quarterly IMS results in increased real earnings management to meet/beat quarterly earnings benchmarks (Ernstberger et al., 2016), it does not result in a decrease in overall investment levels (Nallareddy et al., 2016).[8]

Our paper is also related to prior work on the relation between informational properties of earnings and investments (e.g., Biddle and Hilary, 2006; Biddle et al., 2009; Shroff et al., 2014; Balakrishnan et al., 2014). We add to this body of work by documenting that not only how earnings are measured, but also how frequently they are reported has significant real effects. Finally, our paper has implications for practice as the merits of quarterly reporting continue to be debated in the US and the rest of the world. The evidence in our paper supports the recent decision by both EU and UK to abandon the mandatory quarterly reporting requirement.

2. Theory and prior evidence

2.1. Theoretical link between reporting frequency and corporate myopia

Building upon early theoretical work (e.g., Stein, 1988, 1989) on managerial myopia, several recent studies (e.g., Hermalin and Weisbach, 2012; Gigler et al., 2014; Edmans et al., 2016) highlight that increasing the reporting frequency can create incentives for managers to make myopic

[7] Nallareddy et al. (2016) find that 94% of the mandatory adopters of IMS disclosure requirement in UK issued qualitative IMSs that do not include quarterly sales and earnings information.
[8] Note also that real earnings manipulation around specific benchmarks is quite distinct from the (potentially more costly) permanent forgoing of long-run investment opportunities that is predicted by theory (see section 2) and is part of the debate on the desirability of quarterly reporting. Our objective is to examine whether concerns about the latter phenomenon are borne out in the data.

investment decisions that boost short term profits at the expense of longer run firm value.[9] Stein (1989) shows that corporate myopia can manifest even in efficient capital markets with rational corporate managers and investors as long as two conditions are satisfied. First, corporate managers must exhibit some concern for short term stock prices when evaluating investments.[10] Second, there are information asymmetries between corporate managers and investors about investment expenditures; i.e., compared to corporate managers, investors are not fully able to distinguish expenditures that will yield long-term benefits from those that will not. As a result, investors may mistakenly attribute lower short-run earnings generated from investments that will yield benefits only in the long run to managerial misbehavior or poor business prospects, leading to lower stock prices in the short run. This makes corporate managers (who are sufficiently averse to undervaluation of their stock in the short run) reluctant to undertake investments in long-term oriented projects.

Gigler et al. (2014) extend Stein's (1989) work to show that increasing the reporting frequency can exacerbate incentives for myopic investment behavior. This occurs because increasing the reporting frequency produces shorter term earnings measures that fail to reflect the value of managerial actions that generate value only in the long run. This, in turn, engenders premature evaluation of managers that makes it unviable for them to engage in long-term investments. Thus, a more frequent reporting regime exacerbates the disincentives to invest in long term projects. Using a slightly different theoretical set-up, Hermalin and Weisbach (2012) and Edmans et al. (2016) also model a similar intuition.[11]

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[9] For more examples of theoretical models of myopic behavior, also see Narayanan (1985), Miller and Rock (1985), Shleifer and Vishny (1990), Bebchuk and Stole (1993), Von Thadden (1995), and Holmstrom (1999). Also, see Froot, Perold, and Stein (1992) for an intuitive explanation of the conditions that give rise to managerial myopia in equilibrium.

[10] Theoretical studies argue that this could be because lower prices in short run may expose the managers to a hostile takeover, lead to lower stock based compensation or corporate managers may be concerned about job termination following poor stock price performance.

[11] A natural question arises: if increased reporting frequency results in such myopic behavior why then do we observe managers voluntarily increasing the reporting frequency? Edmans et al. (2016) examine this issue by evaluating the investment effects in a voluntary disclosure regime in which managers may choose to provide less disclosure to avert myopic pressures. They find that such a commitment, however, is not credible and the

Capital investment projects, which commonly generate value over longer horizons, map well into the theory of reporting frequency and myopic underinvestment. Consider a capital investment made by a firm either to upgrade its manufacturing technology that improves the quality of its products or to penetrate a new market segment. It may take more than a quarter or two for such an investment to increase sales because customers may need some time to experience and appreciate the product quality or it may take some time to attract customer attention/loyalty in the new market segment. Thus, the benefits of such capital investments may not be immediate and hence, not reflected in near term earnings. As investors learn about the future prospects of such investments based on quarterly earnings reports, it is possible that after observing poor quarterly earnings reports for the first few quarters, investors may mistakenly interpret these poor results as a reflection of inferior investment choices made by the manager. This would result in a downward revision in investors' views about expected cash flows from these investments, resulting in a stock price decline. Above theoretical work shows that, in equilibrium, anticipating such a possibility, a manager who is sufficiently concerned about a short term decrease in stock price might not make such an investment to begin with.

Two aspects of the theory deserve clarification. First, in equilibrium, the investors are not fooled by managers' myopic behavior and, as a result, managers ultimately do not benefit from myopic actions. While investors do not directly observe the extent of underinvestment in long term projects, they understand managers' incentives to behave myopically and correctly conjecture that there will be underinvestment in long term projects. As a result, firm value reduces immediately following the reporting frequency increase as investors anticipate forgone investment opportunities. Despite being unable to fool the market, the manager is trapped into behaving myopically in

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equilibrium solution involves higher disclosure by managers and myopic under-investment in long run projects (See also Wagenhofer (2014) for a similar argument). Hermalin and Weisbach (2012) examine the political economy of disclosure regulation and argue that similar commitment problems can arise even in a regime with only mandated disclosures. They show that investors would lobby with the regulators to increase disclosure even if it is inefficient because of commitment problems.

equilibrium. The situation is akin to the prisoner's dilemma. Both managers and investors would be better off if managers did not behave myopically. However, such an equilibrium is not sustainable: even if investors conjecture no myopia, managers still have an incentive to fool them by behaving myopically.

The second issue that deserves elaboration is related to the cost of capital effects of increased disclosure frequency. Theory suggests that increased disclosure reduces cost of capital and Fu et al. (2012) find evidence consistent with reduced cost of capital following increased disclosure frequency. At first blush, it may appear that a decline in cost of capital would unequivocally motivate additional investments. In other words, more frequent reporting should unambiguously result in an *increase* in investments. However, it is important to realize that while increasing the reporting frequency may reduce the risk premium demanded by investors (reducing cost of capital) for a fixed distribution of cash flows, it also changes the way investors form expectations about future cash flows from newer investment expenditures. More specifically, as shown by prior theoretical studies of myopic behavior, increasing the reporting frequency can cause premature evaluation of long term investments by investors, leading them to underestimate expected cash flows from these projects following poor short term performance. If the manager is sufficiently averse to lower stock prices in the short run, he would be unwilling to make the investment despite the lower cost of capital. In fact, Edmans et al. (2016) explicitly model this trade-off and show that the cost of capital benefits of increased disclosure come at the price of forgone long term investments. Therefore, whether increased reporting frequency makes an investment more (or less) desirable to managers depends on the combined effect of reduced risk premium and changed investors' expectations about future cash flows. This point is also illustrated by the evidence in Asker et al. (2015) that firms significantly reduce investments after going public due to myopic pressures despite increased availability of cheaper capital from public investors.

2.2. Related Research

There are two streams of studies that provide evidence on the consequences of mandated reporting frequency. In the first stream, Butler et al. (2007) and Fu et al. (2012) use the same setting as our study to provide evidence on the informational benefits of increased reporting frequency in the form of earnings timeliness and cost of capital. While Butler et al. (2007) find little evidence that mandating quarterly reporting frequency leads to more timely incorporation of accounting information in stock prices, Fu et al. (2012) find evidence that it reduces the cost of capital. These studies do not, however, examine the investment (real) effects of reporting frequency.

A second set of studies (Ernstberger et al., 2016; Nallareddy et al., 2016) provides evidence on the consequences of a different notion of quarterly reporting by examining the effect of the introduction of quarterly interim management statements (IMS) required under the 2004 transparency directive adopted by the EU. Unlike the notion of quarterly reporting examined in our study that requires firms to file quarterly financial statements, the IMS requirement does not impose any requirement of disclosure of GAAP based financial statement information (e.g., quarterly net income, assets etc.), but merely requires qualitative disclosures. Nallareddy et al. (2016) report that 94% of the mandatory adopters of IMS disclosure requirement in the UK issued qualitative IMSs that do not include quarterly sales and earnings information.

Thus, the setting of IMS does not fully capture the forces associated with quarterly reporting. The difference is non-trivial and relates directly to the question of myopic underinvestment that we examine in our study. In fact, one of the key reasons why European Union rejected requiring quarterly disclosure of financial statements when drafting the Transparency Directive was the fear that it would lead to “short termism and undue pressure by analysts and fund managers” (Commission of the European Communities (2003)). This view of the EU regulators is supported by theory (discussed above) that highlights the role played by short-run performance measures (such as quarterly profits or sales) in forcing managers to forgo long-term investments. Given the above features of the EU setting, it is perhaps not surprising that Nallareddy et al. (2016), using a methodology similar to ours, do not find evidence of underinvestment in fixed assets surrounding the adoption of IMS.

Unlike Nallareddy et al. (2016), Ernstberger et al. (2016) focus on real earnings manipulation to meet quarterly earnings benchmarks and find evidence of increased real earnings management following adoption of IMS. While examining real earnings management is interesting in its own right, it is quite distinct from the potentially more costly (from a welfare perspective) systematic underinvestment in long-run growth opportunities that is predicted by theory and lies at the heart of the debate surrounding the desirability of quarterly reporting.[12] To the best of our knowledge, ours is the first study to provide evidence on the effects of mandated quarterly disclosure of financial statements on equilibrium investment levels.

3. Research setting and historical context

We use the staggered variations in the financial reporting frequency of US corporations over the years 1950-1970 as our research setting. Prior to the Securities Acts of 1933 and 1934, financial reporting requirements were largely governed by stock exchanges. As early as 1900, NYSE listing agreements began to require annual reporting of balance sheet and earnings information, and by 1910 annual reporting had become the norm (Shultz, 1936; NYSE, 1939). Agreements for semiannual reporting followed within the next ten years (e.g., the Cluett, Peabody Company in 1914). Beginning in 1923, the NYSE required all newly listed companies to publish quarterly financial statements and pressured already listed companies to do the same. In 1926, the NYSE asked all firms to amend their listing agreements to commit to quarterly reporting (NYSE, 1939). These efforts were reasonably successful and by the mid-1950s, 90% of the active domestic companies on NYSE were reporting quarterly (Taylor, 1963).

[12] For example, a corporate manager who expects to fall slightly short of a quarterly earnings benchmark could beat it by postponing the purchase of some laboratory equipment (beakers, microscopes, distillation equipment etc.) or postponing a training program for its scientists. This type of real manipulation is distinct from a permanent forgoing of R&D and capital expenditures because of concerns that stock price would be affected negatively in the next few quarters due to poor short-run reporting performance resulting from such investments.

Unlike the NYSE, neither the AMEX nor the regional exchanges supported quarterly reporting because of the concern that some firms, finding the regulation too burdensome, might choose to delist and move to the over-the-counter market. In 1962, the AMEX and the other exchanges softened their stances, requiring newly listed corporations to report quarterly and encouraging already-listed companies do so, following which many AMEX firms adopted quarterly reporting frequency.

The reporting requirements mandated by the SEC also lagged behind those of the NYSE. Using the powers granted by the Securities Acts, the SEC initially mandated annual reporting of financial statements in 1934 and semi-annual reporting in 1955. The SEC did not consider quarterly reporting until the end of the 1960s when the Wheat Commission proposed quarterly reporting. In September 1969, the SEC proposed that companies file quarterly reports on a new Form 10-Q, a proposal finally adopted in October 1970 and effective for quarters ending after December 31, 1970.[13]

Our research setting offers two key advantages for testing the effect of changes in reporting frequency on firms' investment behavior. First, the changes in reporting frequency occurs at different times allowing us to implement a DiD design. Specifically, the fact that several firms already report on a more frequent basis due to the exchange listing requirements or voluntarily gives us a natural set of control firms for our DiD design. Second, by focusing on a sample of treatment firms that were forced to change the reporting frequency (either because of the SEC mandate or exchange requirements), we can mitigate endogeneity concerns associated with firms' voluntary choice of reporting frequency.

Readers may raise some natural questions about this setting. Was myopia a big concern during this time period? How relevant are the findings from an earlier time period to the current debate given the significant changes in the institutional environment since that time period? The

[13] For a richer description of the historical context, we refer the reader to Taylor (1963) and Butler et al. (2007).

notion of excessive focus on the near term concerned classical economists as early as Jevons (1871), Marshall (1890), and Pigou (1920). Legendary investor Warren Buffett expressed his frustration that markets had become "increasingly short-term oriented" in a letter in 1969.[14] Anecdotal evidence suggests that discussions about myopic focus on quarterly horizons date back as early as 1930s. For example, John Maynard Keynes (1936) notes:

> *"For most of [the professional investors and speculators] are, in fact, largely concerned, not with making superior long-term forecasts of the probable yield of an investment over its whole life, but with foreseeing changes in the conventional basis of valuation a short time ahead of the general public. They are concerned, not with what an investment is really worth to a man who buys it 'for keeps,' but with what the market will value it at, under the influence of mass psychology,* ***three months*** *or a year hence."* (emphasis added)

There is also academic evidence on myopia from this time period.[15] Although there have been significant changes in the institutional environment since that time period, to the extent these changes have only further increased the investors' and corporate managers' focus on short term stock prices, our estimates may represent the lower bound of myopic underinvestment triggered by mandated quarterly reporting in recent time periods.

4. Research Design, Sample and Measurement of Variables

4.1 Research Design

We test the myopia hypothesis by examining the changes in the level of capital investments around reporting frequency increases. Holding growth opportunities constant, if after a reporting frequency increase managers systematically avoid capital investments that produce value over longer horizons, then we would expect to see a decline in aggregate capital investments following reporting

[14] Buffett writes "..it seems to me that: ..a swelling interest in investment performance has created an increasingly short-term oriented and (in my opinion) more speculative market." See the letter dated May 29, 1969 that Warren Buffett wrote to the limited partners of Buffett Partnership: http://www.rbcpa.com/WEB_letters/1969.05.29.pdf.

[15] Early studies examined managerial myopia by analyzing internal hurdle rates used by managers based on the idea that excessive focus on the short term should manifest in the form of higher hurdles rates for discounting future payoffs. King (1972) and Sumner (1974) find that the internal discount rates implied by corporates' investment decisions may be as high as 25%. Similarly, Nield (1964) found that investors usually expected full pay-back on an investment within 3 to 5 years, even though the average life of plant and equipment as per census estimates was often 10 times that.

frequency shocks. This approach is similar to that used in the finance literature that examines myopic underinvestment in other settings (e.g., Asker et al., 2015; Bena et al., 2016; Edmans et al., 2017).[16]

We estimate the following DiD specification on a matched sample of treatment and control firms:

$$INVESTMENT_{i,s,t} = \alpha_i + \gamma_{s,t} + \beta_1\, AFTER_{i,t} + \beta_2 TREAT_i * AFTER_{i,t} + \lambda Z_{i,t} + \varepsilon_{i,s,t} \qquad (1)$$

where $INVESTMENT_{i,s,t}$ is the amount of net additional investments for firm *i*, headquartered in state *s*, during the year *t*; $TREAT_i$ is an indicator variable for treatment firms; $AFTER_{i,t}$ is an indicator variable that equals 1 for periods after the treatment year and 0 for periods prior to the treatment year. For each matched treatment and control firm, we include data for up to five years before and after the treatment year, i.e., t = (-5,+5). We exclude the treatment year (t=0) from our analyses. $Z_{i,t}$ represents a vector of time-varying control variables and α_i represents firm fixed effects. Finally, the equation also includes headquarter state and year interacted fixed effects, $\gamma_{s,t}$, to flexibly absorb the confounding effect of any contemporaneous changes in local business conditions (or growth opportunities) or any secular trends in investments coinciding with reporting frequency increases.[17]

Our main coefficient of interest in equation (1) is β_2, the coefficient on the interaction term $TREAT_i * AFTER_{i,t}$ which measures the change in investments for treatment firms around reporting frequency increases (first difference) relative to contemporaneous changes in investments of control firms (second difference). Under the assumption that treatment and control firms share parallel trends in investments absent changes in reporting frequency, β_2 captures the causal effect of reporting

[16] In addition to examining changes in investment *levels* around the reporting frequency shocks, we also considered examining changes in the *sensitivity* of investment levels to changes in growth opportunities as another test of myopia hypothesis. The idea is that myopic managers would become less responsive to expansions in long run growth opportunities following increases in reporting frequency. The challenge with this test, however, is that theory shows that (see Section 2) investors rationally anticipate managers' myopic behavior and do not incorporate in market values long-term growth opportunities that are expected to be forgone following reporting frequency shocks. As a result changes in measures of growth opportunities (e.g., Tobin's Q) would not be expected to reflect changes in long run growth opportunities after reporting frequency increases, rendering an examination of the sensitivity of investments to long run growth opportunities moot.

[17] Note that state-year interactive fixed effects are more general and subsume simple year effects. Therefore, neither state nor year fixed effects are separately included in the equation. Similarly, the main effect of *TREAT* is omitted from the specification because its effect is subsumed by firm fixed effects.

frequency on investments (Angrist and Pischke, 2009). In our analysis, we verify this assumption for the pre-treatment period. Even if the parallel trends assumption is not violated in the pre-treatment period, an important question, as with any DiD analysis, is whether the parallel trends would have continued in the post-treatment period in the absence of a change in reporting frequency. Such a violation of parallel trends assumption could occur if, for example, reporting frequency increases systematically coincide with changes in growth opportunities. In this case, investments for treatment and control firms would diverge even without the change in reporting frequency and the DiD estimate would be contaminated by the effects of concurrent changes in growth opportunities.

There are two features of our research setting that help address this concern. First, because our analysis focuses on cases where the timing of the reporting frequency increase was exogenously imposed on firms either by the SEC or the stock exchanges, it is unlikely that the timing systematically coincides with changes in firm level growth opportunities or other firm characteristics. Second, the presence of multiple shocks to reporting frequency regimes staggered over time further mitigates this concern. For any unobserved shock to explain our finding, it would need to systematically coincide with three different shocks to reporting frequency (two mandated by the SEC and one by the AMEX) that are separated by many years during our sample period. Nonetheless to ensure the robustness of our findings, our specification includes several variables to control for growth shocks and other changes in firm characteristics; we also explore the sensitivity of our findings to using alternative methods to control for growth opportunities (refer section 5.3).

4.2 Data on reporting frequency and description of matching approach

To construct our sample, we draw from the data on the financial reporting frequency collected by Butler et al. (2007) from *Moody's Industrial News Reports* (published semiweekly).[18]

[18] Butler et al. (2007) collect this data for all NYSE and AMEX firms appearing on the monthly CRSP database in any year from 1950 to 1973. They eliminate industries that the SEC typically excludes from certain disclosure requirements (i.e., utilities, financial service, insurance, real-estate firms, and railroads and other transportation

From this sample, we derive a final sample containing 937 treatment firms matched with an equal number of control firms. Our "treatment" firm-years consists of observations where a firm increased its reporting frequency either voluntarily or involuntarily during the treatment year, but not during the two year period prior to the treatment year. Most of our analysis, however, is based on firms that changed their reporting frequency involuntarily. We consider a firm to have involuntarily increased its reporting frequency if the increase occurred either because of the two SEC mandates in years 1955 and 1970 or because of the strong pressure by the AMEX to report on a quarterly basis around 1962. More specifically, a firm is considered to have involuntarily increased its reporting frequency if the firm (i) increased the frequency to semiannual reporting starting in 1955; or (ii) increased the frequency to quarterly reporting after 1967;[19] or (iii) is listed on the AMEX and increased its frequency to quarterly reporting starting one year before and up to two years after 1962, the year in which the AMEX started urging existing firms and requiring newly listed firms to switch to quarterly reporting. Our sample of involuntary adopters consists of 545 "treatment" firm years.

Table 1, Panel A provides the frequency distribution of treatment firms across different reporting frequency changes for both the full sample and the involuntary adopters sample. Notice that the full sample includes voluntary and involuntary changes in reporting frequency, including firms changing their reporting frequency to three times during the year.

For each treatment firm-year we identify a matched "control" firm that did not change reporting frequency in the same year (i.e., during the treatment year) in which the treatment firm changed its reporting frequency. We also require that control firms did not change the reporting frequency two years before and two years after the treatment year. We use propensity score matching

companies), leaving a sample of 3,702 firms to collect data on reporting frequency. For more details on the data sources and composition of the original sample, see Butler et al. (2007).

[19] Although the SEC mandated quarterly reporting in 1970, we follow the approach suggested in Butler et al. (2007) and Fu et al. (2012) and include firms that switched in the three years before 1970 because SEC discussions and proposals preceded the issuance of final mandate (Butler et al., 2007). This approach allows us to identify involuntary adopters that increased reporting frequency in anticipation of the final mandate. In subsequent analysis (Table 5, Panel B) we find that our results are not sensitive if we use a more stringent time frame of involuntary adopters.

to identify the set of control firms. Specifically, we estimate a propensity score model for each year to identify a control firm for each treatment firm in that year. We employ nearest neighbor matching and drop observations with propensity scores outside the common support to ensure high match quality (Smith and Todd, 2005). In the matched sample for involuntary adopters of higher reporting frequency (used for main analysis), our control group comprise firms that were reporting at a quarterly frequency even prior to the mandated reporting frequency increases.[20]

Following the approach suggested in Asker et al. (2015), for our baseline specifications, we follow a parsimonious matching approach based on firm size (logarithm of total assets) and industry (using Fama-French 10 industry classification) to maximize the number of treatment firms that get retained in our sample.[21] While matching on the relatively broad Fama-French 10 industry classification minimizes sample attrition, one may be concerned that this raises the possibility that our results are driven by industry differences across treatment and control firms. As explained later, our specifications control for other economic differences in treatment and control firms through firm-fixed effects and a variety of time varying controls. Moreover, we document later that our results are robust to the inclusion of industry-year interactive effects, which fully account for the effect of any time-varying industry differences across firms (see Gormley and Matsa, 2014). In the Online Appendix, we explore the sensitivity of our findings to a variety of matching approaches and find that our results are robust if we match on Fama-French 48 industry classification and several additional firm characteristics beyond size and industry membership.

Figure 1 presents the size distribution of our full sample of 937 matched pairs of treatment and control firms. It can be seen that the distribution for treatment and control firms is very similar.

[20] In our full sample that also includes voluntary adopters, there are 5 control firms that are not reporting on a quarterly basis. Our results on full sample are qualitatively unchanged if we drop these 5 control firms.

[21] Asker et al. (2015) point to the problems associated with overmatching when considering many variables in the propensity score matching (see also Heckman et al. (1999) for a discussion of this point). The issue is that while one can make matched firms arbitrarily similar on many dimensions, such a matching procedure can result in firms in the final sample that are even less representative of their respective groups. Moreover, the reduced sample size decreases statistical power.

A t-test of differences in the mean level of total assets across treatment and control firms in the treatment year does not reject the null hypothesis of equal means (t-statistic = -0.44, result not tabulated). Table 1, Panel B presents the industry distribution of treatment and control firms. A visual inspection reveals that the industry distribution of treatment and control firms is also similar. A chi-square test (not tabled) of the difference in proportions across industries between the treatment and control sample is not statistically significant. Thus, our matching procedure yields satisfactory match quality.

4.3 Measurement of investments

We use two measures to capture firms' investments in fixed assets. Our first measure, *CAPEX*, is defined as the amount of capital expenditures scaled by beginning of year total assets. Our second measure is defined as the change in net fixed assets scaled by beginning of year total assets (*CHPPE*).[22] Unlike capital expenditures, *CHPPE* captures growth in investments not only through direct capital expenditures but also through fixed assets purchased through mergers and acquisitions and those acquired through long term leases recorded under the capital lease accounting treatment. In addition, this measure incorporates a firm's divestments in the form of a sale or disposal of fixed assets.

As we explain in Section 2, fixed asset investment choices map well into the theory underlying reporting frequency and myopic underinvestment. Moreover, both survey based and archival research also suggest that managerial myopia can manifest in the form of underinvestment in fixed assets. In their influential survey, Graham et al. (2005) report that corporate executives admit to cutting capital expenditures and avoiding equipment maintenance in order to meet short term earnings targets. Asker et al. (2015), Edmans et al. (2017), and Ladika and Sautner (2015) find large

[22] Our results are robust if we use change in gross fixed assets instead of net fixed assets as the dependent variable. A conceptual limitation of using gross fixed assets is that it overstates the amount of assets by not taking depreciation into account.

sample archival evidence that managerial myopia can indeed manifest in the form of reduced capital expenditures.

Ideally we would like to conduct our analyses only on long term oriented capital investments, but we do not have firm-level data on the timing of the pay-offs from capital investment projects. However, in subsequent analysis we document that the investment decline is primarily driven by industries in which capital investments tend to generate earnings with a longer lag.

We also considered other investment measures used in prior work such as R&D and advertising expenses. However, these measures are very thinly populated in Compustat or unavailable during our sample period. For example, R&D is missing for over 80% of our sample observations. Recent research finds that firms with missing R&D exhibit innovation activity that is similar to firms that disclose R&D and advice against setting missing R&D to zero (Koh and Reeb, 2015). Moreover, during our sample period, accounting rules allowed firms the flexibility to record a portion of R&D expenditures as an asset on the balance sheet, further introducing noise in the R&D variable.

4.4 Control variables

Our choice of control variables is motivated by recent studies that model firm-level investments such as Campello and Graham (2013) and Asker et al. (2015). First, we control for investment opportunities (*INVESTOPP*). Campello and Graham (2013) recommend using predicted values from regressions of Tobin's Q on variables that contain information about firms' marginal product of capital (see also Asker et al., 2015). Specifically, for every Fama-French 48 industry, we estimate regressions of Tobin's Q (calculated as market value of assets divided by book value of assets) on sales growth, return on assets, book leverage, net income, and year fixed effects. *INVESTOPP* is computed for each firm-year as the predicted value from these regressions. The advantage of this approach is that it allows us to more precisely capture market's valuation of a firm's growth opportunities based on the firm's capital productivity and reduces any measurement error in Tobin's Q due to noise/misvaluation of stock prices.[23]

Next, we control for firm size measured as the natural logarithm of total assets (*LOG(ASSETS)*) and profitability measured as operating income before depreciation and amortization scaled by total assets (*EBITDA*). We also control for beginning of year cash scaled by assets (*CASH*) and beginning of year long term debt scaled by assets (*LEVERAGE*) because firms with more cash and lower leverage can more easily exploit improvements in investment opportunities. Finally, firm fixed effects in the specifications control for the effect of any time-invariant firm characteristics and state-year interactive fixed effects absorb the confounding effects of any changes in local business conditions or any secular trends in investments coinciding with reporting frequency increases. All control variables are measured using information obtained from Compustat and CRSP databases.

5. Results

5.1 Descriptive Statistics

Table 1, Panel C presents descriptive statistics for both the full sample and the sample of involuntary adopters that were forced to increase the reporting frequency. The full sample constitutes 10,115 (12,217) firm-year observations representing 937 matched pairs of treatment and control firms for which *CAPEX* (*CHPPE*) and other financial information are available to estimate equation (1). The mean (median) value of total assets for the sample firms is about \$88 million (\$25 million). The mean (median) firm experiences an increase of 4.7% (2.1%) in net fixed assets and reports capital expenditures as a percentage of assets of 8.6% (6.2%). The higher proportion of capital expenditures relative to the increase in fixed assets is consistent with significant disposals of fixed assets during this time period, but could also reflect the effect of depreciation expense which reduces net fixed assets even without disposals. The sample of involuntary adopters is relatively smaller with

[23] Our inferences are robust if we use Tobin's Q as a measure of growth opportunities.

5,791 observations for the *CAPEX* sample (6,902 for the *CHPPE* sample) representing 545 matched pairs of treatment and control observations. However, the distribution of firm characteristics is similar to that presented for the full sample.

5.2 Main findings

We first present graphical evidence by plotting differences between the investment levels of treatment and control firms around the reporting frequency increases. If higher reporting frequency induces myopic underinvestment, in the pre-shock periods (i.e., periods prior to the reporting frequency increase) we would expect to see higher investment rates for treatment firms relative to control firms who are already reporting at a quarterly frequency. We would also expect to see the investment rates for treatment firms converging closer to those for control firms in the post-shock periods. Figure 2 plots the size and industry-adjusted mean investment rates for treatment firms (involuntary adopters) relative to control firms for the following three periods around the reporting frequency shocks: (i) the pre-shock period, (ii) two years subsequent to the shock, and (ii) years 3 through 5 after the shock.[24] Panel A presents the plot for the full sample (i.e., firms that either voluntarily or involuntarily adopted a frequency increase) and Panel B presents the plot for the matched sample of involuntary adopters. Consistent with expectations, it can be seen in both panels that the treatment firms indeed exhibit higher levels of *CAPEX* and *CHPPE* prior to the shocks and these investment levels approach closer to those for control firms subsequent to the reporting shocks.

Table 2 presents evidence from the regression estimates of our DiD specification (Equation (1)). We first present results in Columns (1)-(4) for the entire sample of voluntary and involuntary adopters. Columns (1) and (3) present the estimates of equation (1) without controls with *CAPEX* and

[24] The size and industry adjusted investment rates are obtained as the coefficient on the *TREAT* dummy obtained from the following cross-sectional regressions estimated separately for each of the three time-periods: $INVESTMENT_{i,s,t} = \alpha + \beta_1 TREAT_i + \beta_2 LOG(ASSETS)_{i,t} + \Gamma IND_i + \gamma_{s,t} + \varepsilon_{i,s,t}$, where *IND* represents industry dummies (Fama-French 48 industry level) and other variables are as defined before. Our approach for visually depicting the Difference-in-differences approach in essence is similar to that used in prior studies (e.g., Autor, Donohue, and Schwab (2006) and Acharya, Baghai, and Subramanian (2014)).

CHPPE as the dependent variables. The coefficient on the interaction term *TREAT*AFTER* is negative and statistically significant at better than 1% level, suggesting that, relative to control firms, treatment firms decrease their investment levels following a reporting frequency increase. Coefficient estimates suggest that treatment firms experience a decline of 1.2% in *CAPEX* (1.3% in *CHPPE*) following an increase in reporting frequency. Estimates in Columns (2) and (4) suggest that the inclusion of control variables makes little difference to these results and the DiD estimates continue to be statistically significant (at less than 1% level) and exhibit similar magnitudes (decline of 1.2% in *CAPEX* and 1.4% in *CHPPE*) to those reported in columns (1) and (3).

Table 2, Columns (5) – (8) present the main results for the sample of involuntary adopters. DiD estimates from specifications without control variables (Columns (5) and (7)) reveal that following a reporting frequency increase, treatment firms exhibit an average decline of 1.9% in *CAPEX* (significant at 1% level) and of 1.5% in *CHPPE* (significant at 5% level). The decline in investments is also economically significant and corresponds to 21% (15%) of the standard deviation in *CAPEX* (*CHPPE*).

Estimates in Columns (6) and (8) indicate that the inclusion of control variables does not meaningfully alter either the statistical significance or the magnitudes of the investment decline (decline of 1.8% in *CAPEX* and 1.5% in *CHPPE*). Little change in coefficient magnitudes when including the control variables supports our earlier conjecture that reporting frequency shocks are close to random at the firm level in the sample of involuntary adopters and are not systematically coinciding with changes in firm characteristics (see Roberts and Whited (2012), who suggest this test to assess the randomness of treatment assignment). In the remainder of the paper, we limit our attention to the sample of involuntary adopters, which allows for better identification of the causal effect of the reporting frequency increase.[25]

[25] Our inferences are unchanged if we conduct all of our subsequent analyses using the full sample.

To establish causality, throughout the paper we focus on the DiD estimate captured by the coefficient on *TREAT*AFTER*. The nature of our treatment and control samples, however, also leads to predictions about cross-sectional differences in investment levels of treatment and control firms, which is what we explore next. Because the control firms are already reporting at a quarterly frequency, under the myopia hypothesis we expect the investment levels of control firms to be lower than that of treatment firms before the shocks and to be similar after the shocks. That is, we would expect the coefficient on *TREAT* (difference in investment levels pre-shock) to be positive and we would expect the sum of the coefficients on *TREAT* and *TREAT*AFTER* (difference in investment levels post-shock) to be indistinguishable from zero.

The specifications considered in Table 2 do not allow us to test these cross-sectional predictions as the coefficient on *TREAT* is subsumed by firm fixed effects (see footnote 17). Therefore, for this analysis we use a modified version of equation (1) in which we replace firm fixed effects with firm level random effects.[26] Table 3 reports the results from the modified specifications using random firm effects. Column (1) (Column (2)) presents the results for *CAPEX* (*CHPPE*) as the investment measure. Estimates in Column (1) indicate that coefficient on *TREAT* is 0.014 (t-stat = 1.934), suggesting that the investment rates for treatment firms are higher by 1.4 % in the periods prior to the reporting increase. Coefficient on *TREAT*AFTER* is -0.016 (t-stat = -2.895), suggesting that following the reporting frequency increase, treatment firms exhibit a decline in investment rate of 1.6% relative to control firms. This estimate is very similar to that reported in Table 2 where we include firm fixed effects. Finally, we report the significance levels for the sum of the coefficients on *TREAT* and *TREAT*AFTER*, which captures the cross-sectional difference in investment rates after

[26] Unlike fixed effects model which estimates a unique intercept for each firm without imposing any distributional assumptions, the random effects model assumes that the firm-specific intercepts are drawn from a normal distribution. The random effects model makes more restrictive assumptions, but consumes fewer degrees of freedom, making it feasible to estimate the effect of time-invariant variables such as *TREAT*. Statistical literature recommends the use of random effects model when it is necessary to estimate the effect of time-invariant variables (e.g., Greene, 2003; Morgan, 2013). See Lesmond (2005) and Demirgüç-Kunt and Maksimovic (2002) as examples of studies that resort to random effects model to study the effect of time-invariant variables.

the reporting frequency increase. We find that the sum of the coefficients is economically and statistically indistinguishable from zero. The inferences are similar when we use the *CHPPE* variable. Overall, the results suggest that the treatment firms have greater investment levels before the reporting frequency change and converge to those of control firms following the reporting frequency change.

In Table 4, we explore the timing of the changes in investments surrounding reporting frequency increases to test the parallel trend assumption underlying our DiD estimation and to also examine the persistence of the investment declines. The parallel trend assumption states that conditional on covariates in the regression, treatment and control firms exhibit parallel movements in their investments in the absence of the treatment shock. Several studies (e.g., Angrist and Pischke, 2009; Lechner, 2011) recommend testing the parallel trends assumption by using pre-treatment time period indicator variables to examine whether treatment and control firms exhibit any differential changes in investments prior to the treatment year. To accomplish this, we augment equation (1) with an indicator variable *BEFORE(-1)* and an interaction term *TREAT*BEFORE(-1)*, where *BEFORE(-1)* is coded as one for the one year period prior to the reporting frequency increase and zero otherwise. Estimates in columns (1) and (2) with *CAPEX* and *CHPPE* as dependent variables indicate that the coefficient estimates on the interaction term, *TREAT*BEFORE(-1)*, are statistically and economically indistinguishable from zero. This suggests that changes in investments for treatment and control firms are not statistically different one year prior to the reporting frequency increase. The coefficients on the main variable of interest, *TREAT*AFTER*, continue to be negative and with comparable magnitudes as before. In columns (3) and (4), we present similar specifications using an indicator variable that is lagged by one additional year (*BEFORE(-2)*). Inferences are similar: coefficient on *TREAT*BEFORE(-2)* is insignificant whereas the coefficient on *TREAT*AFTER* continues to be negative and significant. These findings suggest that treatment and control firms follow parallel trends in investments for the two years prior to the reporting frequency increase, and these trends diverge only after the reporting frequency increase.

Next, we present evidence on the persistence of the investment decline for the treatment firms. If the investment decline reflects a shift to a new equilibrium with lower investment levels following the shift in reporting frequency regime, then investment decline should not be temporary and should persist over time. To evaluate the persistence, we create two indicator variables: *AFTER(+1,+2)* and *AFTER(+3,+5)*. *AFTER(+1,+2)* equals one for the first two years subsequent to the reporting frequency increase and zero otherwise; *AFTER(+3,+5)* equals one for year 3 and beyond following the reporting frequency increase and zero otherwise. We estimate equation (1) after replacing the variables *AFTER* and *TREAT*AFTER* with the above two indicator variables and their corresponding interaction terms with *TREAT*. Estimates of the modified specification are presented in columns (5) and (6) of Table 4 for *CAPEX* and *CHPPE*, respectively. In both columns, the coefficients on both interaction terms, *TREAT*AFTER(+1,+2)* and *TREAT*AFTER(+3,+5)*, are negative and statistically significant. Moreover, the coefficients on both interaction terms are of comparable magnitudes regardless of the dependent variable. Together, these findings indicate that the decline in investment following a reporting frequency increase is not short-lived, but persists over time.

5.3 Robustness tests

In this section, we conduct several robustness tests to assess the sensitivity of our findings to (i) some key research design choices (Table 5) and (ii) some additional ways of controlling for changes in firms' growth opportunities (Table 6). In Table 5 Panel A, we document that our findings are not sensitive to the choice of matching procedure. First, we document that our results are robust if we alter our baseline matching approach by using the finer Fama-French 48 industry membership instead of the Fama-French 10 industry classification. As can be seen in columns (1) and (2), the estimated investment decline continues to be both statistically and economically significant (decline of 1.3% in *CAPEX* and 1.7% in *CHPPE*). Next, we alter our baseline matching approach by augmenting the list of matching variables to also include EBITDA, Leverage, Cash, growth opportunities, and pre-treatment levels of *CAPEX* and *CHPPE*.[27] Again, our results are robust: the estimated decline in *CAPEX* is 1.8% ($p < 0.01$) and in *CHPPE* is 1.5% ($p < 0.05$).

In Panel B of Table 5, we explore two alternative definitions of classifying the treatment firms of involuntary adopters. In the first alternative, we restrict the treatment sample to firms that increased the reporting frequency only because of the two SEC mandates in 1955 and 1970. That is, we exclude treatment firms that increased their reporting frequency around 1962 because of changed listing requirements and increased pressure from the AMEX to report on a quarterly basis. In the second (arguably even more stringent) alternative, we consider only firms that changed their reporting frequency in the years after the SEC mandate. That is, we exclude early adopters that changed reporting frequency during the three years prior to 1970 in anticipation of the SEC mandate. Results indicate that our inferences are unaltered. Despite the reduction in sample size, the DiD estimates of the investment decline continue to be statistically significant and economically quite large with estimates varying from 1.7% to 2.4%.

In the next set of analyses, we explore two alternative approaches to control for any concurrent changes in growth opportunities coinciding with reporting frequency increases. First, we replace state-year interactive fixed effects by industry-year interactive fixed effects to examine whether any industry level growth shocks coinciding with reporting frequency increases could explain our findings.[28] Estimates in Table 6, Panel A indicate that the decline in investments remains statistically and economically significant even after including industry-year interactive fixed effects.

[27] Covariate balance presented in the Online Appendix reveals that there are no statistically significant differences between treatment and control firms in the matched sample across all matching variables including the pre-treatment levels of investments.
[28] We do not include state-year interactive and industry-year interactive simultaneously because McKinnish (2008) and Gormley and Matsa (2014) note that estimates from models with too many fixed effects (leaving little remaining variation to estimate the effect of interest) are notoriously susceptible to attenuation bias. Regardless, in untabulated analysis we find that our inferences are robust if we include both state-year and industry-year fixed effects in the same specification.

Decline in *CAPEX* (*CHPPE*) is 1.8% (1.5%) when we use the Fama-French 10 industry classification. Results are robust to using a finer industry classification at the Fama-French 48 industry classification level (see columns 2 and 4).[29] Second, we examine whether changes in firms' lifecycles can explain our results. If firms increase reporting frequency when they reach maturity stage and experience declining growth opportunities, then lifecycle differences could drive our research findings. Although controls for investment opportunities should ideally capture changes in growth opportunities that occur with lifecycle changes, we augment the empirical specifications with two proxies that capture life cycle effects: (i) firm age (*AGE*) and (ii) retained earnings scaled by total assets (*RE*). DeAngelo et al. (2006) note that firms with low *RE* tend to be growth firms whereas firms with high *RE* tend to be mature. To allow for any potential nonlinearities in the relation between lifecycle and investments, we also include quadratic terms of *AGE* and *RE*. Again, our results are robust. Results presented in Table 6, Panel B indicate that controlling for lifecycle effects has little impact on the statistical and economic significance of the decline in investments.

6. What causes the decline in investments?

The analyses thus far offer compelling evidence that, on average, firms experience a decline in investments following an increase in reporting frequency. The investment decline is consistent with two possible, although not mutually exclusive, effects of reporting frequency. It could either reflect myopic underinvestment by managers because of amplified capital market pressures induced by frequent reporting (myopia channel) or represent a correction of previous excess investments by managers due to the discipline imposed by frequent reporting (disciplining channel). We conduct two sets of tests to assess the relative effects of the two channels.

6.1 Future productivity and growth

[29] Note that the number of observations is slightly higher when we replace state-year with industry-year interactive fixed effects because data on the headquarter state is not available for some firms during this time period.

We first examine the implications of the decline in investments for future productivity and growth. The disciplining channel predicts improved productivity following reporting frequency increases. That is, if the investment decline following a reporting frequency increase represents correction of prior overinvestment, then firms should be able to generate prior levels of economic output by deploying fewer resources. This should unambiguously result in productivity improvements. The prediction for growth is however ambiguous. Mechanically, reduction in investments would result in lower growth. However, if prior overinvestment resulted in pecuniary managerial consumption that did not impact revenues in prior years, we would expect no change in growth.

Under the myopia channel, as the reporting frequency is increased from annual to quarterly frequency, managers forgo attractive investment opportunities that create value in the longer run (year or longer) but may appear as poor investment choices based on the earnings performance over the next few quarters. This should result in reduced productivity and growth over horizons of a year or longer.

We use two measures that capture economic output produced per unit of resources consumed: (i) asset turnover measured as sales scaled by lagged assets (*ASSETTURN*), and (ii) return on assets measured as net income scaled by lagged assets (*ROA*). Both of these measures capture the aggregate efficiency of deployment of total assets. We measure firm growth using annual sales growth (*SALESGROWTH*).

We first present graphical evidence by plotting differences in the performance measures for treatment and control firms around the reporting shocks in Figure 3. The patterns appear consistent with the myopia hypothesis: Prior to the reporting shock, treatment firms on average exhibit higher level of asset turnover, ROA, and sales growth than control firms who are already reporting at a quarterly frequency, but this performance differential starts declining following the reporting frequency change.

We next estimate the following DiD specification to more formally examine the effect of reporting frequency on operating performance:

$$PERFORMANCE_{i,s,t} = \alpha_i + \gamma_{s,t} + \beta_1\, AFTER(+1,+2)_{i,t} + \beta_2\, AFTER(+3,+5)_{i,t} + \beta_3 TREAT_i * AFTER(+1,+2)_{i,t} + \beta_4 TREAT_i * AFTER(+3,+5)_{i,t} + \varepsilon_{i,s,t} \qquad (2)$$

where *PERFORMANCE* represents *ASSETTURN*, *ROA*, or *SALESGROWTH*. The coefficients of interest are β_3 and β_4, which capture the DiD estimate of the effect of reporting frequency increase on a firm's productivity and growth in the first two years and the subsequent three years, respectively. We examine the two time periods separately because the effects may be gradual.

Table 7, Columns (1)-(3) present the results of estimating equation (2). Estimates in column (1) suggest that firms experience a significant deterioration in asset turnover following reporting frequency increases. Specifically, the coefficient on *TREAT*AFTER(+1,+2)* is negative (coefficient = -0.079), but it is not significant at conventional levels. However, the decline in asset turnover over the subsequent three years (*TREAT*AFTER(+3,+5)*) is economically large (coefficient = -0.118) and statistically significant at the 10% level. Estimates in column (2) reveal that there is little change in *ROA* during the first two years (coefficient = -0.004), but it decreases by an economically large magnitude of 1.4% during the subsequent three years (statistically significant at the 5% level). With reduced investments, *ceteris paribus*, we would expect *ROA* to mechanically increase because of denominator effects. Thus, our finding of no increase in *ROA* during the first couple of years followed by considerable decreases in years 3 through 5 makes for a stronger case against productivity improvement. In column (3) we find that sales growth starts deteriorating in the first two years by 4.9% (statistically significant at the 10% level) and the deterioration becomes larger (5.8%) in the next three years (statistically significant at the 5% level). Collectively, we view the evidence from productivity and growth results as more consistent with myopia channel being the dominant force behind the reduction in investments following reporting frequency increases.

Similar to the analysis for investments in Table 3, we explore cross-sectional differences in the levels of performance measures for treatment and control firms both before and after the regulation by estimating a modified version of equation (2). That is, we replace firm fixed effects with firm random effects, which allows us to estimate the coefficient on *TREAT*. Estimates from the random effects models in Columns (4)-(6) of Table 7 indicate that the evidence is consistent with what was depicted in Figure 3. Consistent with treatment firms exhibiting superior performance prior to the frequency shock, the coefficient on *TREAT* is positive and is economically significant for all performance measures; the coefficient is also statistically significant at traditional levels for all models except for asset turnover where it is significant at 12% level. We also assess the long run differences in performance levels of the two groups after the shocks by reporting the sum of coefficients on *TREAT* and *TREAT*AFTER(+3,+5)*. Across all models it can be seen that the sum is considerably lower in economic magnitude and statistically indistinguishable from zero, suggesting that there are no detectable differences in performance levels of treatment and control firms after the shocks.

The above results raise an interesting question: why do managers behave myopically when it ultimately hurts firm performance, and therefore their own welfare, over longer horizons? As discussed in detail in Section 2, although the preferred equilibrium outcome for both managers and investors involves no myopic behavior, theory shows that such an equilibrium is not sustainable because of managers' inability to commit to non-myopic behavior.

6.2 Cross-sectional analysis using investment duration

We next test a direct implication of the myopia channel by exploiting the industry level variation in the lag with which the benefits of capital investments manifest in accounting earnings. Myopia theories predict that frequent reporting is more likely to induce myopic underinvestment in industries in which capital investments take longer to generate value. More specifically, myopia channel predicts that as the reporting frequency is increased from annual to quarterly, managers are more likely to forgo investments in industries where the investments tend to generate value over periods one year or longer, and consequently there is greater risk that investors may get misguided by earnings measures within the year (e.g., quarterly) as a sign of poor investment choices. To test this prediction, we create an industry level measure of the fraction of payoffs from capital investments that get reflected in accounting earnings within one year of the investment.

Specifically, we estimate the following cross-sectional regression for each industry-year:

$$OI_{i,t} = \alpha + \sum_{k=1}^{L} \beta_k INVESTMENT_{i,t-k} + \gamma PP\&E_{i,t-L-1} + \varepsilon_{i,t} \quad (3)$$

where *OI* represents the operating income before depreciation, interest, and taxes; *INVESTMENT* represents the annual investment in fixed assets; *PP&E* is the gross investment in plant, property, and equipment. All variables are scaled by lagged assets to facilitate cross-sectional comparison across firms. Coefficient β_k can be interpreted as the gross-payoff from a dollar of investment that reflects in accounting earnings, k years after the investment has been made. The equation includes up to L lags of investments. The model includes PP&E at the beginning of year t-L to control for the effect of all investments made prior to the year t-L. We create an approximate measure of the fraction of payoff from a dollar of investment that manifests in accounting earnings within one year as follows:

$$1yrPayoff\% = \beta_1 / (\textstyle\sum_{k=1}^{L} \beta_k) \quad (4)$$

We estimate equations (3) and (4) for each industry and year combination separately using six lags of *INVESTMENT* (i.e., L=6). Ideally, we would like to estimate Equation (3) using all lags of investments, but increasing the number of lags reduces sample size and decreases the precision of our estimates. In untabulated analysis, we experiment with lags of up to 8 years and obtain similar inferences. To ensure reasonably precise estimates and to minimize the impact of measurement error, we require at least five degrees of freedom for each regression estimation, require the cumulative payoff (i.e., $\sum_{k=1}^{L} \beta_k$) to be positive, and delete estimates at the 1st and 99th percentiles. We conduct the analysis using both Fama-French 48 and Fama-French 30 industry classifications. Use of a relatively coarser Fama-French 30 classification allows us to pool more observations, providing more precise estimates.

Table 8 presents the results of this analysis. Panel A presents the descriptive statistics for each of the 6 lags of β_k estimates and *1yrPayoff%*. For brevity, we present these estimates using Fama-French 48 classification using *CHPPE* as the investment measure; the estimates using Fama-French 30 classification and *CAPEX* as the investment measure are similar. The average β_k coefficient decreases with k although not monotonically, suggesting that, on average, payoffs to investments occur more in the first year and decrease over time. On average, investments generate about 22% gross returns in year 1, declining to about 9% in year 6. In proportional terms, investments generate about 26% in the first year relative to the total operating income generated during the first 6 years (i.e., *1yrPayoff%* = 0.261). There is considerable variation in the *1yrPayoff%* across industries (standard deviation = 0.526).

In Panel B, we examine whether the investment decline following the reporting frequency increase varies across industries with differing speeds of generating return on the investments. Specifically, we estimate the investment effects separately for firms belonging to industries with above and below median values of *1yrPayoff%*, measured in the year prior to the reporting frequency increase. That is, we estimate equation (1) for the high (above median) and low (below median) subsamples. Columns (1) and (2) (Columns (3) and (4)) present results using the distribution of *1yrPayoff%* measured using Fama-French 48 (Fama-French 30) industry classification. Across all specifications, the investment decline is statistically and economically significant only in industries where a larger fraction of the payoffs from capital investments flow into accounting earnings after one year. That is, the coefficient on the interaction term *TREAT*AFTER* for the low *1yrPayoff%* sample is on average -0.030 and statistically significant at the 1% level across all specifications; whereas for the high *1yrPayoff%* sample none of the interaction terms is significant.

In a related analysis, we conduct falsification tests by examining the effect of reporting frequency shocks on investments in alternative assets that yield benefits over relatively short horizons. Myopia channel would not predict a decline in these assets following reporting frequency increases. We consider the following three alternative investment measures for this analysis: (i) cash and marketable securities (*STA_CASH*), (ii) accounts receivables and inventories (*STA*_non*CASH*), and (iii) total short term assets (*STA_TOT*) measured as the sum of *STA_CASH* and *STA*_non*CASH*. Because cash is the dependent variable in this analysis, unlike capital investment regressions, we drop cash as a control variable, but continue to include firm and state-year fixed effects as well as other control variables. In the results presented in Panel C we do not find evidence of changes in short-term assets around reporting frequency increases.

7. Conclusions

This paper examines the real investment effects of increasing the financial reporting frequency using a quasi-natural experiment based on the transition of US firms from annual reporting to semi-annual reporting and then to quarterly reporting during the period 1950-1970. We find a statistically and economically significant decline in investments after firms increase their reporting frequency. The decline is particularly prominent in industries where investments tend to take longer time to generate earnings. Moreover, the adoption of greater reporting frequency is associated with a subsequent decline in operating efficiency and sales growth. Together, these findings suggest that at least part of the investment decline reflects the effect of enhanced managerial myopia following increases in reporting frequency.

Our paper has implications for practice because several regions including Europe, Singapore and Australia have debated the merits of mandating quarterly reporting. While prior research offers support in favor of increasing the reporting frequency by documenting information and cost of capital benefits, our paper offers a more cautionary view. We provide evidence that increasing the frequency has important “real” investment effects that are suggestive of myopic managerial behavior.

Our evidence, therefore, supports the recent decision by the EU and the UK to abandon requiring quarterly reporting for listed companies with an apparent intent to preventing short-termism and promoting long term investments.

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Figure 1: Size distribution of treatment and control firms

This graph presents the size distribution of 937 treatment (cases with reporting frequency increase) and control observations (cases with unchanged reporting frequency) before the post-treatment period. Size is measured as the natural logarithm of the book value of total assets. The kernel densities have been obtained using the epanechnikov kernel function with a bandwidth of 0.4.

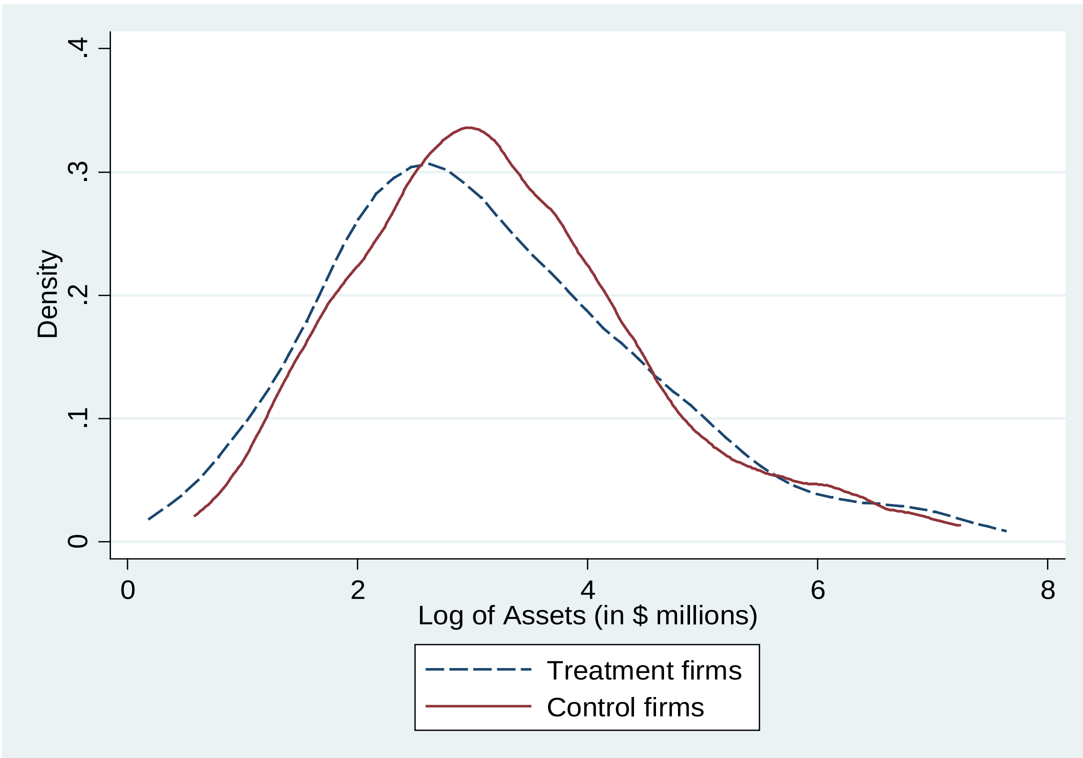
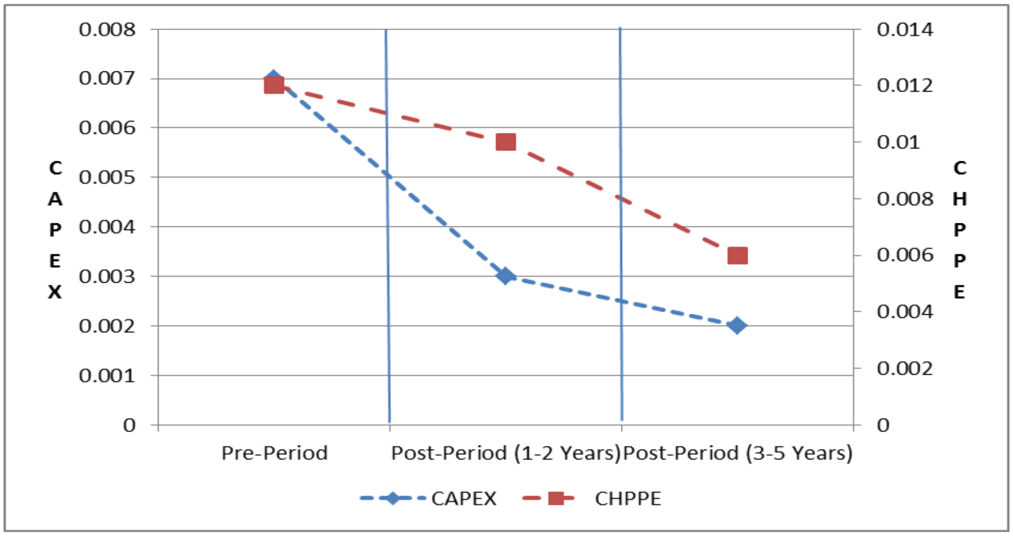


Figure 2: Difference between investments for treatment firms and control firms across time periods

This graph presents the difference between the size and industry-adjusted mean investment levels of treatment and control firms for the three time periods surrounding the reporting frequency increases: (i) Pre-period – period before reporting frequency shocks, (ii) Post-Period (1-2 years) – two years subsequent to the reporting frequency increase, and (iii) Post-Period (3-5 years) – years 3 through 5 subsequent to the reporting frequency increase. *CAPEX* is the capital expenditure scaled by beginning of year assets. *CHPPE* is the change in net fixed assets scaled by beginning of year assets.

Panel A: Full Sample



Panel B: Involuntary adopters

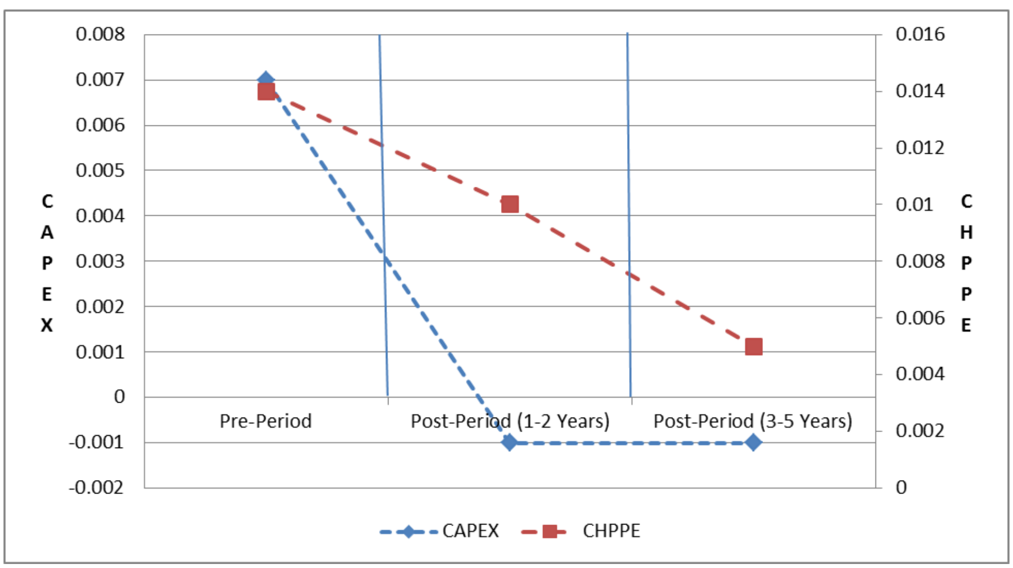
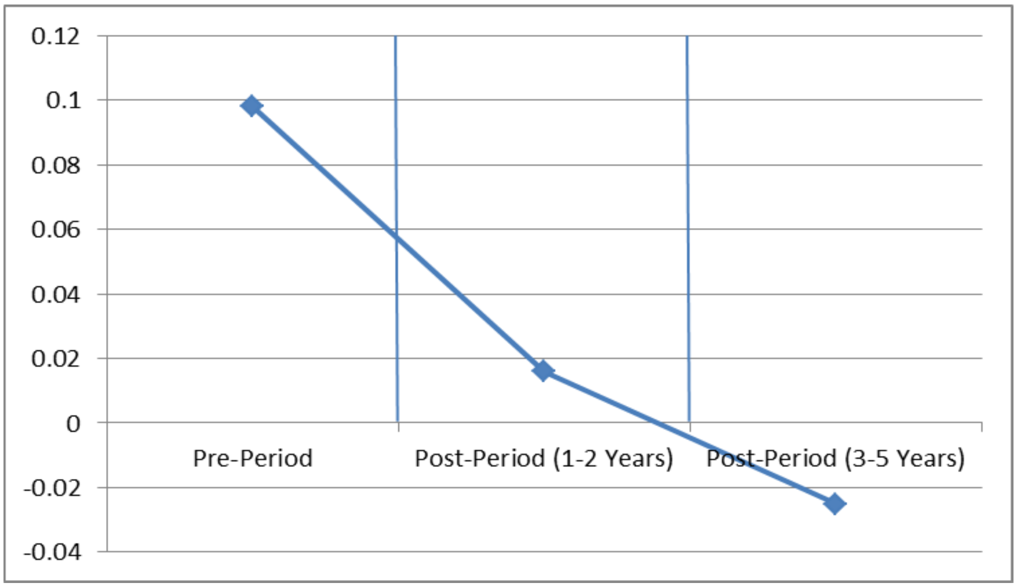


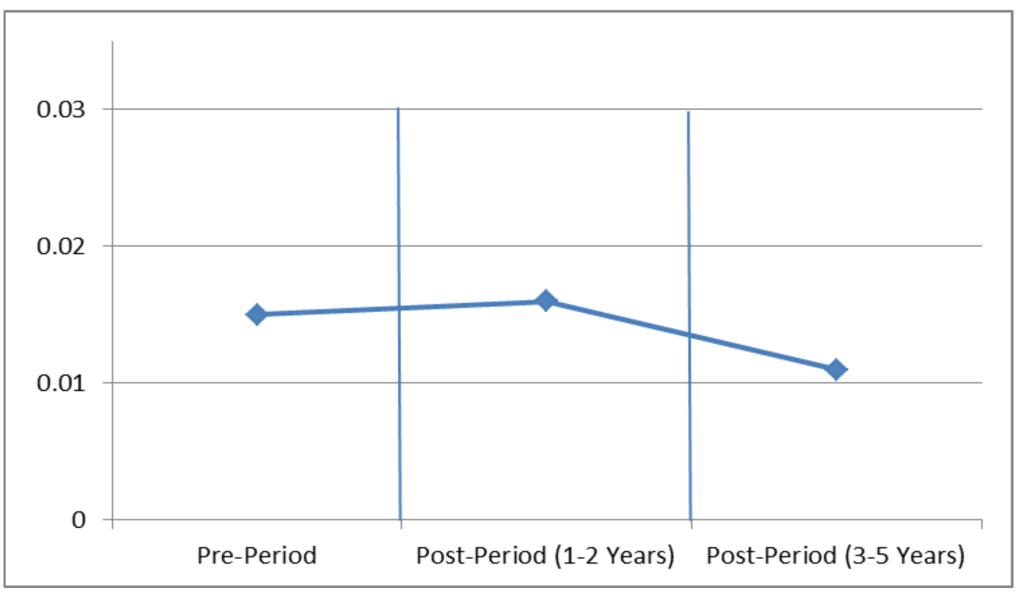
Figure 3: Difference between performance metrics for treatment firms and control firms across time periods

This graph presents the difference between the size and industry-adjusted measures of future productivity and growth for treatment and control firms for the three time periods surrounding the reporting frequency increases: (i) Pre-period – period before reporting frequency shocks, (ii) Post-Period (1-2 years) – two years subsequent to the reporting frequency increase, and (iii) Post-Period (3-5 years) – years 3 through 5 subsequent to the reporting frequency increase. Measures of productivity include: (i) asset turnover computed as sales scaled by lagged assets (*ASSETTURN*), (ii) net income scaled by lagged assets (*ROA*), and (iii) growth measured as percentage change in sales (*SALESGROWTH*).

Panel A: Asset Turnover



Panel B: ROA



Panel C: Sales Growth

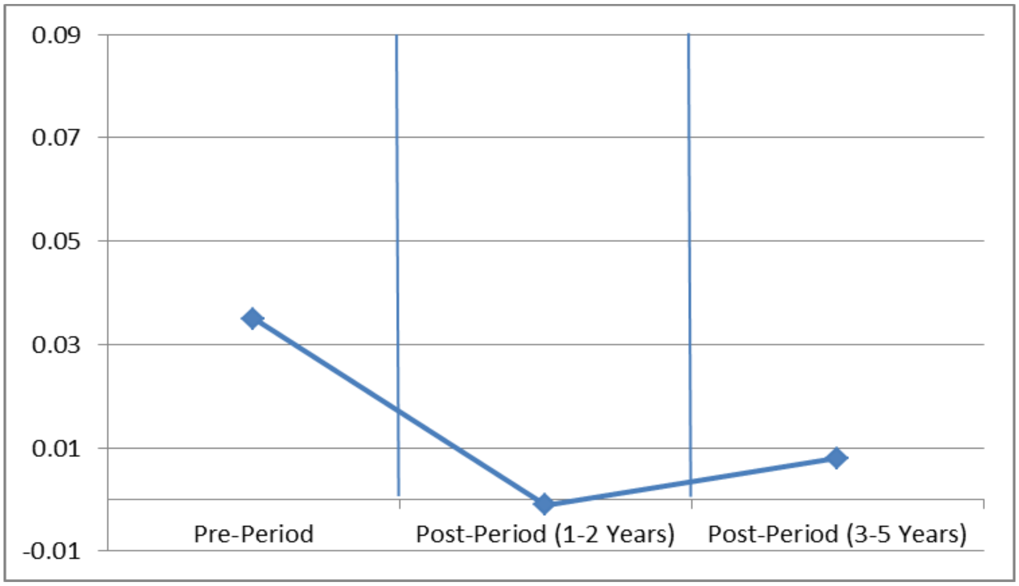


Table 1: Sample Distribution and Descriptive Statistics

Panel A provides the frequency distribution of treatment observations (cases with reporting frequency increase) across years 1951-1974. Panel B presents the industry distribution for treatment observations and control observations (cases with unchanged reporting frequency) using the Fama-French 10 industry classification. Panel C presents the descriptive statistics of the key variables for the treatment and control firms for both the full sample and the restricted sample of involuntary adopters of higher reporting frequency. For both samples, we consider data for up to 5 years before and 5 years after the treatment year. The full sample contains a maximum of 10,115 (12,217) observations for the *CAPEX* (*CHPPE*) regressions whereas the involuntary adopter sample contains a maximum of 5,791 (6,902) observations. *CAPEX* is the capital expenditure scaled by beginning of year assets. *CHPPE* is the change in net fixed assets scaled by beginning of year assets. *ASSETS* is the book value of total assets. *INVESTOPP* represents a measure of investment opportunities; Following Campello and Graham (2013), *INVESTOPP* is measured as predicted values from regressions of Tobin's Q on sales growth, return on assets, book leverage, net income, and year fixed effects estimated at Fama-French 48 industry level. *EBITDA* is operating income before depreciation and amortization scaled by total assets. *LEVERAGE* is the book value of long term debt scaled by total assets. *CASH* is cash balance scaled by total assets.

Panel A: Time series distribution of treatment firms

Frequency Increases to	*Full Sample*	*Involuntary Adopters Sample*
Semi-Annual	165	148
Three times	138	0
Quarterly	634	397
Total	937	545

Panel B: Industry distribution (Full Sample)

Industry	Treatment firms	Control firms
Durable goods	47	53
Energy	41	30
HiTech	78	81
Health	13	18
Manufacturing	324	323
Nondurable goods	168	167
Shops	150	159
Telecommunications	8	5
Other	108	101
Total	937	937

Panel C: Descriptive Statistics

	Mean	Std dev	10th percentile	25th percentile	50th percentile	75th Percentile	90th percentile
Full Sample							
CAPEX	0.086	0.084	0.019	0.035	0.062	0.107	0.177
CHPPE	0.047	0.098	-0.022	-0.000	0.021	0.064	0.144
ASSETS ($ millions)	87.997	200.765	5.500	11.337	25.500	65.700	183.627
EBITDA	0.179	0.121	0.047	0.104	0.164	0.237	0.326
INVESTOPP	1.486	0.534	0.862	1.139	1.451	1.779	2.154
LEVERAGE	0.158	0.135	0.000	0.037	0.142	0.242	0.344
CASH	0.106	0.093	0.023	0.040	0.075	0.143	0.235
Involuntary Adopters Sample							
CAPEX	0.089	0.089	0.018	0.034	0.062	0.110	0.186
CHPPE	0.048	0.103	-0.025	-0.001	0.020	0.064	0.152
ASSETS ($ millions)	82.441	207.428	5.034	9.700	22.192	56.765	153.144
EBITDA	0.175	0.125	0.041	0.100	0.161	0.234	0.329
INVESTOPP	1.458	0.565	0.786	1.094	1.410	1.772	2.171
LEVERAGE	0.167	0.140	0.000	0.042	0.153	0.258	0.358
CASH	0.099	0.091	0.021	0.036	0.067	0.131	0.219

Table 2: Reporting frequency and investments

This table presents evidence on the effect of increased reporting frequency on investments. Measures of investments include: (i) capital expenditure scaled by beginning of year assets (*CAPEX*), and (ii) change in net fixed assets scaled by beginning of year assets (*CHPPE*). *TREAT* is an indicator for treatment firms, which are firms that experience an increase in reporting frequency. *AFTER* is an indicator for firm-year observations after the treatment year. Coefficient estimates for *TREAT* are suppressed because of firm fixed effects. State represents the state in which a firm's headquarters is situated. For variable definitions of control variables refer Table 1. *t*-statistics, reported in parentheses, are calculated based on standard errors obtained by clustering at the firm level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

	Full Sample				Involuntary Adopters			
	CAPEX	*CAPEX*	*CHPPE*	*CHPPE*	*CAPEX*	*CAPEX*	*CHPPE*	*CHPPE*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
AFTER	0.002	0.002	0.003	0.002	0.007*	0.006*	0.008*	0.007*
	(0.847)	(0.861)	(0.968)	(0.731)	(1.796)	(1.757)	(1.791)	(1.735)
*TREAT*AFTER*	-0.012***	-0.012***	-0.013***	-0.014***	-0.019***	-0.018***	-0.015**	-0.015**
	(-2.731)	(-3.242)	(-2.814)	(-3.339)	(-2.612)	(-3.076)	(-1.966)	(-2.343)
EBITDA		0.186***		-0.112		0.160**		-0.095
		(2.941)		(-1.570)		(2.022)		(-1.094)
INVESTOPP		0.027		0.159***		0.039		0.151***
		(1.256)		(6.093)		(1.495)		(4.767)
LEVERAGE		-0.113***		-0.121***		-0.110***		-0.138***
		(-5.391)		(-5.714)		(-4.456)		(-5.418)
CASH		0.018		0.111***		0.021		0.108***
		(0.834)		(5.184)		(0.806)		(3.783)
LOG(ASSETS)		0.026***		0.043***		0.024***		0.049***
		(5.097)		(7.059)		(4.051)		(6.549)
Firm and State*Year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Observations	10,115	10,115	12,217	12,217	5,791	5,791	6,902	6,902
R-squared	0.530	0.606	0.338	0.482	0.568	0.644	0.377	0.518

Table 3: Evidence on cross-sectional differences in investments of treatment and control firms

This table presents evidence on the effect of reporting frequency increase on investments using a modified version of Equation (1) that replaces firm fixed effects with firm-level random effects. Measures of investments include: (i) capital expenditure scaled by beginning of year assets (CAPEX), and (ii) change in net fixed assets scaled by beginning of year assets (CHPPE). TREAT is an indicator for treatment firms, which are firms that experience an increase in reporting frequency. AFTER is an indicator for firm-year observations after the treatment year. State represents the state in which a firm's headquarters is situated. For variable definitions of control variables refer Table 1. t-statistics, reported in parentheses, are calculated based on standard errors obtained by clustering at the firm level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

	CAPEX	*CHPPE*
	(1)	(2)
TREAT	0.014*	0.012*
	(1.934)	(1.747)
AFTER	0.006*	0.006
	(1.873)	(1.512)
*TREAT*AFTER*	-0.016***	-0.012**
	(-2.895)	(-2.028)
EBITDA	0.217***	0.181***
	(4.996)	(4.442)
INVESTOPP	0.021	0.051***
	(1.547)	(3.643)
LEVERAGE	-0.061***	-0.061***
	(-3.411)	(-3.490)
CASH	0.013	0.061***
	(0.549)	(2.633)
Log(ASSETS)	0.015***	0.016***
	(5.697)	(6.128)
*TREAT+TREAT*AFTER*	-0.002	-0.000
	(-0.258)	(-0.049)
Firm random effects	YES	YES
State*Year fixed effects	YES	YES
Observations	5,791	6,902
R-squared	0.275	0.300

Table 4: Timing of changes in investments

This table presents evidence on the timing of changes in investments around increases in financial reporting frequency. Measures of investments include: (i) capital expenditure scaled by beginning of year assets (*CAPEX*), and (ii) change in net fixed assets scaled by beginning of year assets *TREAT* is an indicator for treatment firms, which are firms that experience an increase in reporting frequency. *BEFORE(-1)* (*BEFORE(-2)*) is an indicator variable that equals one for firm-year observations one year (two years) before the treatment year and zero otherwise. *AFTER(+1,+2)* is an indicator variables that equals one for observations during the two-year period after the treatment year and zero otherwise. *AFTER(+3,+5)* equals one for all observations for year 3 and beyond after the treatment year and zero otherwise. Coefficient estimates for *TREAT* are suppressed because of firm fixed effects. Coefficient estimates on the main effects of the two *BEFORE* indicator variables, *AFTER(+1,+2)*, and *AFTER(+3,+5)* have been omitted for brevity. State represents the state in which a firm's headquarters is situated. *t*-statistics, reported in parentheses, are calculated based on standard errors obtained by clustering at the firm level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

	Parallel trends test				Persistence test	
	CAPEX	*CHPPE*	*CAPEX*	*CHPPE*	*CAPEX*	*CHPPE*
	(1)	(2)	(3)	(4)	(5)	(6)
*TREAT*BEFORE(-2)*			0.001	-0.006		
			(0.216)	(-0.734)		
*TREAT*BEFORE(-1)*	0.006	0.002				
	(0.899)	(0.322)				
*TREAT*AFTER*	-0.016**	-0.014**	-0.018***	-0.017**		
	(-2.551)	(-2.097)	(-2.839)	(-2.446)		
*TREAT*AFTER(+1,+2)*					-0.016***	-0.014**
					(-2.582)	(-2.141)
*TREAT*AFTER(+3,+5)*					-0.019***	-0.016**
					(-3.072)	(-2.177)
Firm and State*Year fixed effects	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
Observations	5,791	6,902	5,791	6,902	5,791	6,902
R-squared	0.644	0.518	0.644	0.518	0.644	0.518

Table 5: Sensitivity to matching procedure and alternative treatment samples

This table presents evidence on the sensitivity of the findings in Table 2 to alternative matching procedures (Panel A) and treatment samples (Panel B). For Panel A, we examine the sensitivity of our prior results to two variations to our baseline matching approach based on size and Fama-French 10 industry classification. First, we alter our baseline matching approach to use of the finer Fama-French 48 industry classification. Second, we alter our baseline matching approach by including additional variables in the propensity score model in addition to size and Fama-French 10 industry classification. Additional variables included in the propensity score model are *EBITDA*, Leverage, Cash, growth opportunities and pre-treatment investment levels (*CAPEX* and *CHPPE*). For variable definitions refer Table 1. For Panel B, we use two alternative treatment samples. First, we consider a treatment sample of firms that altered the reporting frequency surrounding the SEC mandate including three years prior to the SEC mandate to allow for early adopters. Second, we consider a more restrictive treatment sample consisting of firms that altered reporting frequency in the years following the SEC mandate. Coefficient estimates for *TREAT* are suppressed because of firm fixed effects. Coefficient estimates for *AFTER* and all control variables (defined in the caption of Table 1) have been omitted for brevity. State represents the state in which a firm's headquarters is situated. *t*-statistics, reported in parentheses, are calculated based on standard errors obtained by clustering at the firm level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

Panel A: Sensitivity to matching procedure

	Fama-French 48 industry and Size		Fama-French 10 industry, Size, EBITDA, Leverage, Cash, Growth opportunities, Investments	
	CAPEX	*CHPPE*	*CAPEX*	*CHPPE*
	(1)	(2)	(3)	(4)
*TREAT*AFTER*	-0.013**	-0.017**	-0.018***	-0.015**
	(-2.182)	(-2.497)	(-2.708)	(-2.055)
Controls	YES	YES	YES	YES
Firm and State*Year fixed effects	YES	YES	YES	YES
Observations	5,469	6,490	5,104	5,495
R-squared	0.642	0.525	0.624	0.522

Panel B: Sensitivity to alternative treatment samples

	Sample of involuntary adopters excluding AMEX firms that were forced by the exchange to follow quarterly reporting		Sample of involuntary adopters comprising exclusively of firms that changed reporting frequency **after** the SEC mandates	
	CAPEX	*CHPPE*	*CAPEX*	*CHPPE*
	(1)	(2)	(3)	(4)
*TREAT*AFTER*	-0.018***	-0.017**	-0.024**	-0.022*
	(-2.832)	(-2.415)	(-2.293)	(-1.942)
Controls	YES	YES	YES	YES
Firm and State*Year fixed effects	YES	YES	YES	YES
Observations	4,887	5,447	2,723	3,026
R-squared	0.642	0.531	0.649	0.550

Table 6: Controlling for industry shocks and life-cycle effects

Panel A presents robustness to inclusion of industry-year interactive fixed effects to control for any contemporaneous industry level shocks. The interactive fixed effects are measured using the Fama-French 10 and 48 industry classification. Panel B presents robustness to inclusion of controls for lifecycle effects. We use two different proxies to control for lifecycle effects: (i) firm age (*AGE*) and (ii) Retained earnings scaled by total assets (*RE*). Coefficient estimates for *AFTER* and all other control variables (all defined in Table 1) have been omitted for brevity. Coefficient estimates for *TREAT* are suppressed because of firm fixed effects. *AGE* is scaled by 100 for expositional convenience. State represents the state in which a firm's headquarters is situated. *t*-statistics, reported in parentheses, are calculated based on standard errors obtained by clustering at the firm level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

Panel A: Controlling for time varying industry shocks

	CAPEX		*CHPPE*	
	FF10 classification	FF48 classification	FF10 classification	FF48 classification
	(1)	(2)	(3)	(4)
*TREAT*AFTER*	-0.018***	-0.014***	-0.015***	-0.010*
	(-3.644)	(-2.761)	(-2.608)	(-1.876)
Controls	YES	YES	YES	YES
Firm and Industry*Year fixed effects	YES	YES	YES	YES
Observations	6,625	6,625	8,103	8,103
R-squared	0.588	0.661	0.440	0.528

Panel B: Controlling for lifecycle effects

	CAPEX		*CHPPE*	
	Firm Age	Retained Earnings	Firm Age	Retained Earnings
	(1)	(2)	(3)	(4)
*TREAT*AFTER*	-0.017***	-0.018***	-0.014**	-0.019***
	(-3.008)	(-2.754)	(-2.170)	(-2.651)
AGE	-0.033		1.539	
	(-0.018)		(0.647)	
AGE^2	0.540***		0.850***	
	(2.729)		(4.266)	
RE		-0.044**		-0.063**
		(-2.322)		(-2.505)
RE^2		-0.113***		-0.172***
		(-3.279)		(-4.616)
Other controls	YES	YES	YES	YES
Firm and State*Year fixed effects	YES	YES	YES	YES
Observations	5,791	4,916	6,902	5,351
R-squared	0.645	0.664	0.520	0.561

Table 7: Reporting frequency and future performance

This table presents evidence on the effect of reporting frequency increase on future productivity and growth. Measures of productivity include: (i) asset turnover computed as sales scaled by lagged assets (*ASSETTURN*), (ii) net income scaled by lagged assets (*ROA*), and (iii) growth measured as percentage change in sales (*SALESGROWTH*). *TREAT* is an indicator for treatment firms, which are firms that experience an increase in reporting frequency. *AFTER(+1,+2)* is an indicator variables that equals one for observations during the two-year period after the treatment year and zero otherwise. *AFTER(+3,+5)* equals one for all observations for year 3 and beyond after the treatment year and zero otherwise. State represents the state in which a firm's headquarters is situated. Columns (1) – (3) present estimates from firm fixed effect models and Columns (4)-(6) present models with firm random effects. *t*-statistics, reported in parentheses, are calculated based on standard errors obtained by clustering at the firm level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

	Fixed Effects Models			Random Effects Models		
	ASSETTURN	*ROA*	*SALESGROWTH*	*ASSETTURN*	*ROA*	*SALESGROWTH*
	(1)	(2)	(3)	(4)	(5)	(6)
TREAT				0.181	0.017***	0.051**
				(1.539)	(2.690)	(2.229)
AFTER(+1,+2)	0.047	0.003		0.051*	0.003	0.014
	(1.515)	(0.970)		(1.721)	(0.843)	(0.991)
AFTER(+3,+5)	0.039	0.003		0.045	0.003	0.011
	(1.044)	(0.987)		(1.286)	(0.921)	(0.638)
*TREAT*AFTER(+1,+2)*	-0.079	-0.004	-0.049*	-0.075	-0.002	-0.037
	(-1.282)	(-0.708)	(-1.871)	(-1.284)	(-0.423)	(-1.552)
*TREAT*AFTER(+3,+5)*	-0.118*	-0.014**	-0.058**	-0.113*	-0.012*	-0.045*
	(-1.754)	(-2.039)	(-2.180)	(-1.777)	(-1.904)	(-1.852)
TREAT+ TREAT*AFTER(+3,+5)				**0.068**	**0.005**	**0.006**
				(0.617)	**(0.902)**	**(0.355)**
Firm Fixed Effects	YES	YES	YES			
Firm Random Effects				YES	YES	YES
State*Year fixed effects	YES	YES	YES	YES	YES	YES
Observations	6,873	6,863	6,873	6,873	6,863	6,873
R-squared	0.856	0.508	0.394	0.211	0.170	0.239

Table 8: Effect of investment duration

This table presents evidence on whether the effect of reporting frequency increases on investments depends on the speed with which benefits of investments flow into accounting earnings. Panels A and B present this analysis for fixed asset investments by exploiting industry level variation in in the speed with which the benefits of fixed asset investment flow into earnings measured as *1yrPayoff%*. Panel A reports the descriptive statistics for the parameter estimates of the industry-year estimation of equation (3) used to measure *1yrPayoff%*. For brevity, we report descriptives only using the Fama-French 48 industry classification and *CHPPE* as the investment measure. *1yrPayoff%* is the percentage of gross earnings generated in year 1 relative to years 1 through 6, i.e., *1yrPayoff%* = $\beta_1/(\sum_{k=1}^{L}\beta_k)$. Panel B presents the estimates of fixed asset investment decline for subsamples with high (above median) and low (below median) values of *1yrPayoff%*. Panel C presents the estimates of the effect of reporting frequency increases on investments in three alternative assets that yield benefits over relatively short horizons: (i) marketable securities and cash scaled by lagged assets (*STA_Cash*), (ii) accounts receivable and inventory scaled by lagged assets (*STA_nonCash*), and (iii) total short-term assets (*STA_TOT*) measured as the sum of *STA_CASH* and *STA_nonCASH*. Unlike regressions for our measures of investments in fixed assets, we do not include cash as a control variable in regressions for short-term assets. *TREAT* is an indicator for treatment firms, which are firms that experience an increase in reporting frequency. *AFTER* is an indicator for firm-year observations after the treatment year. State represents the state in which a firm's headquarters is situated. For variable definitions of control variables refer Table 1. *t*-statistics, reported in parentheses, are calculated based on standard errors obtained by clustering at the firm level. Statistical significance (two-sided) at the 10%, 5%, and 1% level is denoted by *, **, and ***, respectively.

Panel A: Descriptive statistics (using Fama-French 48 industry classification)

	Mean	Std dev	10th percentile	25th percentile	50th percentile	75th percentile	90th percentile
β_1	0.220	0.140	0.018	0.127	0.207	0.300	0.446
β_2	0.118	0.083	0.032	0.050	0.107	0.149	0.252
β_3	0.117	0.106	0.006	0.045	0.096	0.155	0.232
β_4	0.086	0.080	-0.018	0.044	0.075	0.129	0.204
β_5	0.106	0.088	0.016	0.064	0.092	0.138	0.228
β_6	0.086	0.106	-0.039	0.032	0.075	0.141	0.218
1yrPayoff%	0.261	0.526	-0.102	0.195	0.327	0.445	0.736

Panel B: Results for subsamples with high and low 1yrPayoff%

	Fama-French 48		Fama-French 30	
	CAPEX (1)	*CHPPE* (2)	*CAPEX* (3)	*CHPPE* (4)
*TREAT*AFTER (High 1yrPayoff% sample)*	-0.008	-0.003	-0.008	-0.006
	(-0.741)	(-0.370)	(-0.731)	(-0.666)
*TREAT*AFTER (Low 1yrPayoff% sample)*	-0.025**	-0.032***	-0.030***	-0.031***
	(-2.465)	(-2.831)	(-3.160)	(-2.937)
Controls	YES	YES	YES	YES
Firm and State*Year fixed effects	YES	YES	YES	YES
Observations	3,937	4,839	4,625	5,633
R-squared	0.620	0.526	0.638	0.527

Table 8 (Cont'd)

Panel C: Effect on short term investments

	STA_Cash (1)	*STA_nonCash* (2)	*STA_TOT* (3)
AFTER	-0.003	0.009	0.007
	(-0.872)	(1.215)	(0.941)
*TREAT*AFTER*	-0.003	-0.004	-0.008
	(-0.413)	(-0.289)	(-0.547)
EBITDA	0.307***	-0.774***	-0.409**
	(3.824)	(-4.670)	(-2.167)
INVESTOPP	0.001	0.507***	0.492***
	(0.054)	(9.287)	(7.845)
LEVERAGE	-0.062**	-0.160***	-0.205***
	(-2.425)	(-3.932)	(-3.983)
Log(ASSETS)	-0.007	0.058***	0.052***
	(-0.975)	(4.375)	(3.493)
Firm and State*Year fixed effects	YES	YES	YES
Observations	6,873	6,863	6,873
R-squared	0.856	0.508	0.394