Preliminary and Incomplete

Real Effects of Frequent Financial Reporting

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April 2014

Abstract: Using the transition of US firms from annual reporting to quarterly reporting over the period 1950-1970 as a natural experiment, we provide causal evidence on the effects of increased reporting frequency on firms' investments. Estimates from difference-in-differences specifications show that increased reporting frequency is associated with economically large decline in investments. Additional analyses reveal that these findings are most consistent with managers behaving myopically following increases in reporting frequency.

We thank Christian Leuz and workshop participants at Nazarbayev University for helpful comments and suggestions. We acknowledge financial support from the Fuqua School of Business, Duke University and the Cass Business School, City University London.

1. Introduction

Choice of reporting frequency is an important national policy decision and its economic consequences are of considerable interest to regulators and standard-setters. Whether financial statements should be more frequently reported has been the subject of considerable debate by regulatory bodies in several countries including the US. Proponents of frequent reporting (e.g., quarterly reporting) of firms' financials argue that greater frequency reduces information asymmetry between managers and shareholders and improves the timeliness of earnings. Opponents of frequent reporting cite excessive management focus on short term results and myopic tendencies to report positive performance in each period as reasons for not requiring quarterly reporting (Fong 2007). In June 13, 2013 the EU parliament voted to approve the new Accounting and Transparency Directives that included the abandonment of the requirement to publish quarterly financial information for small and medium enterprises citing the main reasons as "reducing the administrative burden and encouraging long term investment." The purpose of this paper is to inform this debate by examining the real "investment" effects of increasing the financial reporting frequency.

Whether increased financial reporting frequency improves or adversely influences a manager's investments decision is ambiguous because of two opposing forces. On the one hand, increasing the frequency of financial reporting has beneficial effects leading to improved investment decisions. Timely release of information may reduce information asymmetry, improve liquidity that in turn reduces the firm's cost of capital allowing them to attract new capital. Furthermore, frequent reporting potentially improves external monitoring thereby mitigating over- or under-investment stemming from managerial agency problems. On the other hand, frequent reporting can potentially distort investment decisions. In particular, frequent reporting can lead to myopic tendencies (e.g., an over-emphasis on reported short-term performance measures) leading to suboptimal investment behavior. Which of these two forces dominate is an empirical question that we examine in this study.

To provide empirical evidence, we use data from a natural experiment – the transition of US firms from annual reporting to semi-annual reporting and then to quarterly reporting over the period 1950-1970. The SEC required annual reporting of financial statements in 1934 and changed the required frequency to semi-annual reporting in 1955 and eventually to quarterly reporting in 1970. Independent of the various mandates, several firms voluntarily reported at more than the required frequency even prior to 1970.[1]

Two features of this setting enable causal identification. First, the staggered timing of the shift in reporting frequency allows us to implement a difference-in-differences (DID) design that mitigates concerns that our findings are influenced by time trends or unobservable differences across firms. Second, by focusing on a sample of firms that changed the reporting frequency following the mandated rule change, we are able to circumvent potential endogeneity problems associated with analyzing firms that voluntarily changed the reporting frequency.

We implement the DID design on a sample of treatment firms (firms increasing reporting frequency) matched to an equal number of control firms (firms with unchanged reporting frequency) based on variables known to be associated with investments including size, industry, and grow opportunities. We include firm and year fixed effects to absorb the effect of time-invariant firm characteristics and secular trends in investments. The DID estimate captures the relative change in investments of treatment firms following reporting frequency increases to contemporaneous change in investments of control firms.

[1] Butler et al. (2007) note that over 70% of firms reported at quarterly frequency during this period.

Our DID estimates suggest that firms significantly reduce investments following an increase in reporting frequency. Specifically, firms which increase their reporting frequency reduce investments in fixed assets by 1.7% of total assets. This an economically significant decline as it is equivalent to a 22% decline in investments relative to the mean level of investment opportunities. Furthermore, the reduction in investments is persistent up to at least 5 years, and is robust to controlling for a range of alternative proxies for investment opportunities. Supporting a causal interpretation, the reduction manifests only after the reporting frequency increase but not before. The findings are robust to estimation on a subsample of firms that increased reporting frequency following mandated rule changes, further mitigating endogeneity concerns. Finally, the results are robust to inclusion of industry-year interactive fixed effects, indicating that any industry level shocks to investment opportunities coinciding with reporting frequency increases cannot explain our findings.

Our finding that investments decline following a reporting frequency increase is consistent with two plausible explanations. It could reflect myopic underinvestment by managers because of amplified capital market pressures induced by frequent reporting (myopia channel). Alternatively, the decline could be a manifestation of improved monitoring by stakeholders stemming from frequent reporting (monitoring channel). That is, the decline represents a correction of previous excess investments by managers.

We conduct a series of tests to distinguish between these two alternative explanations. First, we exploit the contrasting predictions offered by these two channels regarding the relation between decline in investments and earnings timeliness. The monitoring channel predicts that the decline in investments would be greater with timelier earnings because earnings that are a more timely source of information about managerial actions facilitate shareholder monitoring.

In contrast, the myopia channel predicts a greater decline in investments when earnings are less timely. Theoretical models of myopic behavior assume that managers have private information about the firm's long term prospects. Consequently, shareholders are forced to make forecasts about long term prospects using noisy metric of current performance such as periodic accounting reports. This allows managers to inflate shareholders' beliefs about long term prospects and inflate the stock price by boosting current period performance measures. Thus, timely release of information about future prospects constrains the manager's ability to inflate shareholders' beliefs about the firm's future prospects by boosting current earnings. This suggests that myopic behavior would be less likely in industries in which earnings reveal managers' private information about firms' future prospects in a timely fashion. Our evidence is consistent with the myopia channel – that is, the decline in investments is lower in industries where earnings are a timely source of information.

Next, we examine firms' operating performance following increases in reporting frequency. If the decline in investments following increased reporting frequency reflects correction of prior overinvestment, then the monitoring channel would predict that the decline should result in improved firm performance. However, inconsistent with the monitoring channel, we do not find evidence of improvements in firm performance. If anything, the evidence is supportive of decline in performance in the years following the increase in reporting frequency.

Finally, we examine the effect of financial slack prior to reporting frequency increases. Because managers are likely to overinvest only when they have surplus cash, the monitoring channel suggests that the decline in investments should manifest only when there is sufficient financial slack prior to reporting frequency increase. Inconsistent with the monitoring story, we find that the decline in investments manifests even for firms with the least financial slack and find some evidence that the decline is greater for firms with lower slack. Collectively the evidence is inconsistent with the monitoring story and more consistent with managerial myopia. That is, the decline in investments, for the most part, reflects the effect of managerial myopia induced by increased reporting frequency.

Our paper makes two contributions to extant literature and practice. First, we add to our understanding of the economic consequences of frequent reporting by examining its effects on investments. Prior research suggests that timelier release of information through frequent reporting offer significant benefits in the form of reduced cost of capital, improved liquidity and reduced agency costs. Our findings, consistent with predictions from recent theoretical work by Gigler et al. (2013), suggest that frequent reporting can also impose significant costs by inducing myopic behavior and distorting managerial investment decisions. Thus, as Verdi (2012) points out it would be premature to extol the virtues of increasing the frequency of financial reporting based on the findings in Fu et al. (2012). Also, our paper provides empirical support for the recent vote by the European Parliament to approve the new Accounting and Transparency directives that include abolishing the requirement to publish quarterly financial information.

Second, we contribute to the literature on managerial myopia. While academics and practitioners have often expressed concerns about managerial myopia, empirical evidence has been mostly indirect and sparse. Prior studies have identified different sources of capital market pressures that can induce myopia. For example, Asker et al. (2013) find that public ownership induces myopia whereas Edmans et al. (2013) and Ladika and Sautner (2013) find that short vesting horizon of managers' equity based compensation can induce myopia. We suggest that frequent financial reporting, although beneficial at first blush and may appear benign, is another mechanism that can result in myopic managerial behavior to the detriment of economy wide investments.

A caveat is in order. Although we identify an important cost of frequent reporting, our paper does not speak necessarily to the net effect of frequent reporting. Therefore, we are unable to make strong policy recommendations as it would require a comprehensive analysis of the cost-benefit tradeoffs of frequent reporting. Nevertheless, our findings offer a starting point to evaluate this cost-benefit tradeoff by highlighting a significant cost of frequent reporting apart from the myriad benefits reported in prior research.

The rest of the paper is organized as follows. Section 2 discusses prior literature and develops the hypothesis. Section 3 presents the research design, while Section 4 reports our main findings. In Section 5 we discuss alternative explanations for our findings and in Section 6 we offer concluding remarks.

2. Prior literature and hypothesis development

2.1 Related research

Although the desirability of frequent financial reporting has been the subject of intense debate for a very long time, much of the prior literature focuses on the benefits of frequent financial reporting. Analytical research on disclosures (e.g., Diamond 1985; Bushman 1991) and subsequent empirical evidence (e.g., Welker 1995; Botosan 1997) suggest that more disclosures improve a firm's liquidity and reduce its cost of capital. With respect to the frequency of financial reporting, empirical research by Fu et al. (2012) document that firms voluntarily or mandatorily increasing their reporting frequency experience a reduction in information asymmetry and a decrease in their cost of capital by more than 60 basis points. However, research by Butler et al. (2007) finds evidence that although earnings timeliness improves for firms voluntarily changing their reporting frequency, earnings timeliness are unaffected for firms that are forced to shift the reporting frequency via a mandate.

Recent research suggests that frequent reporting could impose significant costs by distorting managers' investment decisions.[2] Seminal work by Stein (1989) shows that even in efficient capital markets managers exhibit myopic behavior and underinvest due to their excessive preference for short-term stock prices relative to long-term value. Gigler et al. (2013) extend this work to show that when the firm reporting frequency increases, this myopic management behavior gets exacerbated. Myopia manifests because frequent reporting induces premature evaluation of managerial actions whose value gets reflected in reported financial measures only in the long run, resulting in short-term price pressure from impatient investors. In an experimental setting, Bhojraj and Libby (2005) use experienced financial managers as experimental subjects to manipulate the reporting frequency and find that managers behave myopically when faced with increased capital market pressure and increased reporting frequency. We extend this work by providing archival evidence on the "real" investment effects of increased reporting frequency.

2.2 Hypothesis development

Increased reporting frequency can affect investments through three potential channels each of which influences investments differently. The first channel is the financing channel. As prior research (e.g., Fu et al. 2012) suggests, timely financial reporting through increased frequency reduces a firm's long-term information asymmetry, and consequently its cost of capital. Such reduction in cost of capital relaxes a firm's financing constraints, thereby allowing the firm to invest in a larger set of positive NPV projects. Thus, the financing channel predicts

[2] Other costs of increasing the reporting frequency potentially includes compliance costs, information intermediaries' information collection costs, and reduced managerial voluntary disclosures (see Bushee and Leuz, 2005 and Gigler and Hemmer, 1998).

that frequent reporting would lead to an increase in investments. The second channel that we consider is the monitoring channel. Directors in a firm's board need timely information to help them with their monitoring and advising responsibilities (Bushman et al. 2004). Frequent reporting could improve the quality of firms' investment decisions by reducing agency problems through improved monitoring by shareholders. In the absence of monitoring, agency problems create incentives to either overinvest (Jensen, 1986) or underinvest (Myers, 1977 and Myers and Majluf, 1984). Improved monitoring due to more timely release of information via increased reporting frequency could mitigate both the underinvestment and the overinvestment problem. That is, the influence of the monitoring channel is ambiguous in that reporting frequency could either increase or decrease the level of investments. Stated differently, a reporting frequency increase could cause an increase or decrease in investments depending on whether a firm faces an underinvestment or an overinvestment problem. Therefore, the monitoring channel does not offer a directional prediction about the change in investment policy following a change in reporting frequency.

The third and final channel is the myopia channel. Early theoretical work (e.g., Stein 1989, Narayanan 1985) on managerial myopia suggests that when managers have short horizons and focus on short-term stock price, they tend to underinvest in order to boost current profitability and current stock prices at the expense of longer term firm value. Contrary to this view, Bebchuk and Stole (1993) argue that in the presence of imperfect information about the productivity of investments, suboptimal investment can occur in the form of managerial overinvestment because managers attempt to signal higher quality of investment opportunities through such overinvestment. Consistent with the underinvestment perspective, Graham, Harvey and Rajgopal's (2005) survey evidence report that a significant proportion (about 60%) of the managers would avoid positive NPV investments if such investments lead to reduced earnings and missing analysts' consensus earnings estimate. Further, recent empirical work by Asker et al. (2012) finds that public firms invest considerably less than comparable private firms consistent with underinvestment behavior by short-term oriented managers.

Extending Stein's (1989) model, Gigler et al. (2013) explicitly examine the investment consequences of frequent financial reporting. They show that in the presence of noisy information signals, frequent reporting coupled with capital market's impatience give rise to myopic managerial behavior. That is, frequent reporting induces price pressures that are similar in respects to evaluating managerial actions prematurely, whose value is only revealed in the long run. This premature evaluation of managerial action induces managers to behave myopically by undertaking investments that offer short term results or altogether avoid investments that only offer results in the long run. Thus, Gigler et al. (2013) model would predict that frequent financial reporting imposes real effects on the firm by exacerbating the underinvestment problem. In other words, the myopia channel predicts that investments would decline following a reporting frequency increase.

3. Sample and Research design

3.1 Sample

To construct our sample, we begin with the data on reporting frequency from Butler et al. (2007).[3] From this sample, we isolate a set of 976 "treatment" firm-years consisting of firm-years when a firm increased its reporting frequency either voluntarily or mandatorily during the treatment year, but not during the two year period prior to the treatment year. Panel A provides the frequency distribution of treatment firms across years prior to the SEC's mandating of

[3] For more details on the data sources and composition of the original sample, please see Butler et al. (2007) and Fu et al. (2012).

increased frequency in 1955 (semi-annual) and 1970 (quarterly). Obvious from the panel, a significant number of firms reported more frequently than that required by the SEC mandate.[4]

We next match each treatment firm-year to a "control" firm that does not experience a change in the reporting frequency during the year of the reporting frequency increase of the matched treatment observation (i.e., during the treatment year).[5] We also require that the control firms have not changed the reporting frequency two years before and two years after the treatment year. We match the control firms to treatment firms using a propensity score matching methodology.[6]

For the propensity score model we consider variables that are known to be associated with investments. Specifically, the model includes: (1) firm size measured as the natural logarithm of book value of assets (*LOG*(*ASSETS)*), (2) indicator variables for industry membership using the Fama-French 10 industry classification, (3) investment opportunities (*INVESTOPP*), defined in section 3.3, (4) book leverage (*LEVERAGE)* measured as the book value of long term debt scaled by total assets, (5) Operating income before depreciation and amortization scaled by assets (*EBITDA*), and (6) cash scaled by assets (*CASH*). All variables are measured at the end of the treatment year. We estimate the propensity score model separately for each treatment year using a logit specification; We employ nearest neighbor matching and impose the restriction of common support to ensure high match quality.[7]

[4] Notice also that several firms reported at a frequency less than the mandated level of frequency. We are unable to discern the exact reason for this reduced reporting frequency but conjecture that these may be smaller firms facing financial distress or that these firms received some special exemptions from the SEC during this time period.
[5] We do not require that the matched control firm has the same reporting frequency either in the pre- or the post-treatment years. We only require that the control firm experiences no change in the reporting frequency.
[6] Our inferences are robust to using a simpler approach of identifying control firms based on size and industry.
[7] Our inferences remain unchanged if, instead of nearest neighbor matching, we identify control firms using caliper based matching using caliper of 1%, 5%, or 10%. Our inferences are also robust to use of probit model instead of logit model for estimating propensity scores.

Table 1, Panels B and C provide descriptive statistics to assess the quality of the match. 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 very similar. A chi-square test (not tabled) of the difference in proportions across industries between the treatment and control sample is not statistically significant at conventional levels. Panel C presents the mean values of the variables used in the propensity score model. The difference in means of the treatment and control firms is not statistically different for any of the variables included in the propensity score model. Thus, the covariate balance between the treatment and control firms is achieved.

3.2 Research Design

To examine the effect of reporting frequency on investments, we estimate the following DID specification on the matched sample:

$$INVESTMENT_{i,t} = \beta_0 + \beta_1 TREAT_i + \beta_2 AFTER_{it} + \beta_3 TREAT_i * AFTER_{it}$$
$$+\Gamma CONTROLS + \Delta FIRM + \Theta YEAR + \varepsilon_{it}\,, \quad (1)$$

where *INVESTMENT* is the amount of change in investments during the year; *TREAT* is an indicator variable for treatment firms; *AFTER* is a dummy variable that equals 1 for periods after the treatment year and 0 for periods prior to the treatment year. We include data for up to five years after the treatment year, i.e., (+1,+5), and up to two years prior to the treatment year, i.e., (-2,-1). Following Butler et al. (2007) and Fu et al. (2012) we exclude the treatment year (t=0). *CONTROLS*, *FIRM*, and *YEAR* represents a vector of control variables, firm fixed effects, and year fixed effects, respectively.

A tacit advantage of this difference-in-differences design is that it allows us to make causal inferences about the effect of reporting frequency as it mitigates concerns that our inferences are confounded by time trends or any unobservable differences between treatment and control firms. For example, a potential concern is that firms that experience an expansion in investment opportunities choose to voluntarily increase reporting frequency to obtain external capital at a reasonable price; change in investments following reporting frequency increases may therefore reflect the effect of investment opportunities. The DID design mitigates this concern because we evaluate the effect of the reporting frequency increase on treatment firms' investments after subtracting any change in investments experienced by matched control firms that do not change the reporting frequency but experience a similar expansion in investment opportunities. In additional analysis we further mitigate this concern by examining a subsample of firms that increased reporting frequency following mandated rule changes. Because reporting frequency increase is exogenously imposed for these firms, the increase will not be systematically associated with investment opportunities. Thus, for these firms, the results will not be confounded by any unobservable factors that drive firms' decision to voluntarily increase the reporting frequency.

Our coefficient of interest in equation (1) is β_3 on the interaction term between *TREAT* and *AFTER*, which captures the DID estimate of the effect of reporting frequency increase on investments for the treatment firms. A positive (negative) β_3 implies that an increase in reporting frequency results in an increase (decrease) in investments. A positive β_3 would be consistent with benefits from either the financing channel or the monitoring channel whereas a negative β_3 is consistent with either the monitoring channel or the myopia channel.

3.2 Measurement of investments

Following Asker et al. (2013), we use three measures of investments that capture firms' growth in fixed assets. Firms can grow fixed assets by either building new capacity through capital expenditures, obtaining a long term lease, or by purchasing existing assets of other firms through mergers and acquisitions (M&As). Our first measure focuses on the former mechanism and is defined as the amount of capital expenditure scaled by beginning of year total assets (*CAPEX*). Our second measure is a more comprehensive measure of investments defined as the change in gross fixed assets scaled by beginning of year total assets (*INVGROSS*). Unlike, capital expenditure, *INVGROSS* captures growth in investments not only through direct capital expenditures, but also 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. However, this measure does not take into account long-term leases accounted for as operating leases. Finally, for robustness, we also model growth in net fixed assets scaled by beginning of year total assets (*INVNET*). The only difference between *INVGROSS* and *INVNET* is accumulated depreciation. For parsimony, we do not table results using *INVNET* but our inferences remain unchanged if we use *INVNET* instead of *INVGROSS* in all our empirical specifications.

We considered other commonly used investment measures in prior work such as research and development expenditures and advertising expenses, as these measures capture attributes not present in our three measures. However, data on R&D and advertising expenses are not available during our sample period. We acknowledge this as a limitation of our analysis.

3.3 Control variables

Following recent studies that model firm-level investments (e.g., Campello and Graham, 2013; Aasker et al., 2013, Balakrishnan et al., 2013; Chava and Roberts, 2008), we control for investment opportunities (*INVESTOPP*) and operating income before depreciation and amortization scaled by total assets (*EBITDA*) in all specifications. Our measure of investment opportunities is based on the approach in Campello and Graham (2013) and Asker et al. (2013). 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. Specifically, for every 2-digit SIC 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 dummies. In addition, we include firm fixed effects to control for time-invariant firm characteristics. *INVESTOPP* is computed for each firm-year as the predicted value from these regressions. For measurement of these control variables we obtain data from Compustat and CRSP databases.

Table 1, Panel D presents descriptive statistics for the entire sample used to estimate equation (1). The full sample comprises a maximum of 12,432 observations for which sufficient information is available to estimate Equation (1). The mean (median) value of total assets for the sample firms is $83.5 million ($24.2 million). The mean (median) firm experience an increase of 7.8% (4.6%) in gross fixed assets and reports capital expenditures as a percentage of assets of 8.7% (6.3%). The higher proportion of capital expenditures relative to the increase in fixed assets suggests significant amount of disposals of fixed assets during this time period.

4. Results

4.1 Main findings

Table 2 provides evidence on the effect of reporting frequency increases on investments by estimating equation (1). For completeness, we provide results with (columns 3 and 4) and without (columns 1 and 2) the control variables. In column (1) we report the results when using *INVGROSS* as the dependent variable, while column (2) uses *CAPEX* as the dependent variable.

As mentioned earlier, our main coefficient of interest is that on the interaction term *TREAT*AFTER*, which captures the DID estimate of reporting frequency increase on investment outcomes. The coefficient is negative and statistically significant at the 5% level in both columns (1) and (2), suggesting that, relative to control firms, treatment firms decrease their investment levels following a reporting frequency increase. Findings in column (1) suggests that treatment firms reduce annual investment in gross fixed assets by 1.7% of total assets whereas column (2) shows that treatment firms reduce their annual capital expenditures by 1.0% of assets following an increase in reporting frequency.

Coefficient estimates reported in columns (3) and (4) indicate that the results are robust to the inclusion of control variables for investment opportunities (*INVESTOPP*) and operating performance before depreciation and amortization (*EBITDA*). If anything, the statistical significance is greater for the interaction term. As expected, both *INVESTOPP* and *EBITDA* are positively associated with investments. More important, the economic magnitudes of the decrease in annual investment is unaffected by the inclusion of controls. This highlights the efficacy of our DID design in absorbing the effects of any cross-sectional differences between treatment and control firms; It also suggests that the DID estimates are unlikely to be contaminated by the effects of any unobserved cross-sectional differences between treatment and control firms.

The economic magnitude of the decline in investments is quite large: A 1.7% change in *INVGROSS* – our comprehensive measure of investments – represents a 22% change in its unconditional mean value of 7.8% in our sample (see Table 1, Panel D). To further assess the economic significance, we compare the effect of reporting frequency increase to the effect of investment opportunities. Estimates show that the effect of reporting frequency increase on

INVGROSS is approximately equivalent to the effect of a one standard deviation decrease in investment opportunities.[8]

In Table 3, we explore the specific timing of the changes in investments around reporting frequency increases. We first examine whether treatment and control firms exhibit any differences in investments during the year prior to the reporting frequency increase. This test enables us to confirm that any differences in investments observed between the treatment and control firms occur only in the period following the reporting change. We create an indicator variable *BEFORE*, which is coded as one for the year prior to the reporting frequency increase and zero otherwise. We then estimate equation (1) augmented with *BEFORE* and an interaction term *TREAT*BEFORE*. Estimates in columns (1) and (2) with *INVGROSS* and *CAPEX* as dependent variables show that the coefficient estimates on the interaction term, *TREAT*BEFORE*, are statistically indistinguishable from zero. This indicates that treatment and control firms invest similarly prior to the treatment year and the relative decline in investments for the treatment firms occurs only after the reporting frequency increase.[9] This finding also mitigates concerns about anticipation effects and reverse causality. The coefficients on the main variable of interest, *TREAT*AFTER*, continues to be negative, although the coefficient in the *CAPEX* regression loses statistical significance.

Next, we present evidence on the persistence of the investment decline for the treatment firms. To determine 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

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[8] The effect of a one standard deviation change in investment opportunities on *INVGROSS* equals the coefficient on *INVESTOPP* * standard Deviation of *INVESTOPP* = 0.0267*0.645 = 0.0172.

[9] We explored whether the decline in investments vary depending on whether the reporting frequency increased from annual to semi-annual reporting, semi-annual to tri-annual reporting, or semi-annual to quarterly reporting. However, untabulated findings do not reveal significant differences across these different reporting frequency changes.

frequency 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 interaction terms with *TREAT*. Estimates of the modified specification presented in columns (3) and (4) show that the coefficient on both *TREAT*AFTER(+1,+2)* and *TREAT*AFTER(+3,+5)* are negative and statistically significant. This suggests that the decline in investment following a reporting increase is not short-lived. In fact, the decline in investments is persistent and becomes more pronouncedly negative over time. For example, in the model with *INVGROSS* as the dependent variable (column 3), investments decline by 1.7% during the first two years and then the decrease in investments drops even further to 2% (a 16% decrease) in the subsequent years. A similar pattern is observed when CAPEX is the dependent variable (column 4) – capital expenditures decline by 0.9% in the first two years and then continue to decline by 1% in the subsequent three years.

4.2 Robustness tests

In this section, we consider several tests to ensure robustness of our findings. In our primary analysis, a crucial control variable is the level of investment opportunities. However, investment opportunities are notoriously difficult to measure and we recognize that the proxy that we use captures this construct with noise. We therefore examine the sensitivity of our findings to two other proxies for investment opportunities commonly employed in prior studies: (1) annual sales growth (*SALESGROWTH*), and (2) industry level growth opportunities measured as the size weighted Tobin's Q of all firms within the same two-digit SIC industry (*Q_SIC*). Table 4, Panel A presents our results. Estimates show that our main findings are robust to using these alternative proxies for growth opportunities. Particularly, the coefficients on the interaction term, *TREAT*AFTER*, continue to be negative and statistically significant.

Next, we examine the sensitivity of our findings to including additional control variables considered in more recent work. In particular, we control for book leverage (*LEVERAGE)* and cash scaled by assets (*CASH)* because firms with low leverage and high levels of cash reserves may be more easily able to take advantage of any improvements in growth opportunities (e.g., Asker et al., 2013; Badertscher et al., 2013). We also include logarithm of book value of assets (*LOG(ASSETS)*) to control for any size effects. Table 4, Panel B presents estimates of equation (1) augmented with these three additional control variables. It can be seen that there is slight increase in the statistical significance of the coefficient estimates on the interaction term *TREAT*AFTER* and the economic magnitudes remain large as before.

Next, in Table 4, Panel C, we examine the robustness of our findings to the inclusion of industry-year interactive fixed effects. The purpose of this analysis to ensure that our findings are not caused by any unobservable industry shocks (such as shocks to growth opportunities or fundamentals) that coincide with increases in reporting frequency increases. Inclusion of industry-year fixed effects absorbs the effect of any time varying industry shocks. We use the Fama-French 10 industry classification for this test.[10] It can be seen that the coefficient estimates on *TREAT*AFTER* remain statistically significant and the economic magnitudes are very similar to those reported in Table 2: the coefficient estimate for *INVGROSS* model is -0.020 and for *CAPEX* model is -0.010. This result suggests that our findings cannot be explained by any industry level shocks that coincide with reporting frequency increases.

Finally, we consider the endogeneity problem associated with voluntarily adoption of increased reporting frequency. Recall that our analysis does not distinguish between voluntary

[10] Inferences remain unchanged if we use SIC two-digit level industry classification.

and mandatory changes in reporting frequency. Including firms that voluntarily changed their reporting frequency in the sample raises the concern that our findings may be due to unobservable firm factors that drive the treatment firms' decision to increase reporting frequency. It is worth noting that one of the main advantages of the DID design is to eliminate the confounding effect of unobservable differences between treatment and control firms. Therefore, our findings are unlikely to be affected by this problem. Nonetheless, to buttress our findings, we examine a subsample of treatment firms that increased their reporting frequency following mandatory rule changes. Because the increase in reporting frequency is likely to be exogenously imposed for such treatment firms, a decline in investments for these firms cannot be confounded by endogeneity concerns.

Table 5 presents results on the subsample of firms with mandatory increases in reporting frequency. Following Butler et al. (2007), we define mandatory increases as firm-specific increases to semiannual reporting occurring after 1954 and increases to quarterly reporting occurring after 1967.[11] The coefficient estimates on *TREAT*AFTER* are significantly different from zero at the 5% level or better and there is an increase in the economic magnitudes: estimate is -0.021 for *INVGROSS* and -0.016 for *CAPEX*. This finding further supports our claim that our inferences cannot be explained by unobservable firm factors occurring around the same time as the reporting frequency increase.

5. What causes the decline in investments?

The analysis thus far offers compelling evidence of a decline in investments following an increase in the reporting frequency. The decline in investments is inconsistent with the financing

[11] Butler et al. (2007) use the years 1954 and 1967 instead of the actual SEC mandate years accommodates firms that change reporting frequency in anticipation of the mandate. For robustness, we also consider a different subsample by classifying firms based on the actual years of mandated change, i.e., years 1955 and 1970, and find that our inferences are unaltered.

channel as improvements in information asymmetry due to timely release of information should lead to an increase in the ability to finance investments and a consequent increase rather than a decrease in investments. Thus, the decline is attributable either to the monitoring channel or the myopia channel. The monitoring channel argues that reduced investment reflects a correction in prior overinvestment due to unresolved agency problems; timelier release of information through increased reporting frequency facilitates external monitoring, reducing managerial agency problems and the associated overinvestment. The myopia channel argues that the reduced investment reflects myopic underinvestment due to increased capital market pressures following reporting frequency increases. We conduct a battery of additional tests to disentangle between these two alternative explanations.

5.1 Earnings timeliness

A key ingredient of models of myopic behavior is the presence of information asymmetries between the firm and the manager about the firm's business prospects. In these models, managers have private information about the firm's long term prospects. Consequently, shareholders are forced to make forecasts about long term prospects using noisy metric of current performance such as periodic accounting reports. This allows managers to inflate shareholders' beliefs about long term prospects thereby inflating the stock price by boosting current period performance measures. Therefore managers, who care sufficiently about near term stock price, have incentives to make myopic investment choices that boost current performance at the cost of long run firm value.

The above discussion suggests that myopic behavior would be less likely in industries in which earnings reveal managers' private information about firms' future prospects in a timely fashion. Timely release of information about future prospects constrains the manager's ability to inflate shareholders' beliefs about the firm's future prospects by boosting current earnings. Myopia channel therefore suggests that the decline in investments following reporting frequency increases should be mitigated in industries in which earnings are timely.

In contrast to the myopia channel, the monitoring channel predicts that the decline in investments should be greater in industries in which earnings are timely. Increased reporting frequency can meaningfully aid shareholder monitoring only when earnings are a timely source of information about firm performance. If earnings are not timely, shareholders may rely more on other sources of information to monitor managers.

Following prior studies (e.g., Bushman et al. (2004) and Barth et al. (2013)), we use the earnings-return relation to measure earnings timeliness. Specifically, we measure earnings timeliness at the two-digit SIC level as the coefficient estimate on earnings from annual cross-sectional regressions of stock returns on earnings scaled by market value of equity.[12] To ensure that the information in current earnings is reflected in stock prices, we measure stock return for the 12 month period ending three months after the fiscal year end.

To examine the effect of earnings timeliness on the relation between change in investments and reporting frequency increases, we create an indicator variable of timeliness (*TIMELY*) that takes the value of one for industries in which earnings timeliness (i.e., the coefficient in the earnings-returns relation) falls above the median timeliness, zero otherwise. We measure *TIMLEY* one year prior to the treatment year. We estimate equation (1) after including *TIMELY* and an interaction term of *TIMELY* with *TREAT*AFTER* as additional covariates. Table 6 presents the results of this analysis. The coefficient of interest in Table 6 is that on the interaction term *TREAT*AFTER*TIMELY* that captures the incremental DID estimate of the

[12] Inferences are similar if we use the R-squared from this regression or asymmetric loss recognition measured using Basu (1997) regressions as the partitioning variable.

effect of reporting frequency increase on investments for firms in industries with above median level of earnings timeliness. The coefficient on *TREAT*AFTER* captures the investment effects of reporting frequency increase for below median level of earnings timeliness. The myopic channel would predict that the coefficient on the interaction term, *TREAT*AFTER*TIMELY*, is positive whereas the monitoring channel would predict that this coefficient is negative.

Consistent with myopia channel, estimates show that the decline in investments is significantly lower in industries with high earnings timeliness. Specifically, relative to industries with low earnings timeliness, the decline in investments in industries with high timeliness is lower by about 29% for *INVGROSS* (incremental coefficient = 0.006 compared to a coefficient of -0.021 for TREAT*AFTER) and by about 71% for *CAPEX* (incremental coefficient = 0.010 compared to a coefficient of -0.016 for TREAT*AFTER). Despite the large economic magnitude, for the *INVGROSS* specification the incremental coefficient on *TREAT*AFTER*TIMELY* is not statistically significant at conventional levels; while the incremental coefficient on CAPEX is both economically and statistically significant. To enable a comparison of the total investment effects for firms in industries with above median level of timeliness we present near the bottom of the table the sum of the coefficients on *TREAT*AFTER* and *TREAT*AFTER*TIMELY*. It is interesting that in the *CAPEX* regression, the investment effect for firms in industries with above median timeliness is not statistically different from zero. Collectively, we view the evidence in Table 6 as more consistent with the myopia channel.

5.2.Operating performance tests

The monitoring channel predicts improved firm operating performance following a change in investments because any change in a firm's investments represents a correction of a previous overinvestment or underinvestment problem. Thus, under the monitoring channel, our evidence of a decline in investments following a reporting frequency increase suggests a prior overinvestment problem. Because an underinvestment in the current period represents a correction of a previous overinvestment problem, the decline in investments should manifest in improved subsequent operating performance. Hence, the monitoring channel predicts improved operating performance following increases in reporting frequency.

Under the myopia channel, however, the prediction is ambiguous. The argument underlying this channel is that managers underinvest by avoiding positive NPV projects with a view to boosting short term performance measures. If such reductions in investment are severe enough it could result in poor long run operating performance because the manager favors investment choices that do not have longer term benefits. Furthermore, if the underinvestment problem manifests only in lost investment opportunities we may not observe a decline in subsequent operating performance relative to their control counterparts. Thus, under the myopia channel underinvestment by treatment firms may result either in no differential future operating performance or a decline in future operating performance.

It is an empirical challenge to identify an appropriate measure of long term operating performance. We consider the operating performance in the years subsequent to the treatment year as capturing long term operating performance. We use three measures of operating performance: (i) return on assets measured as operating income scaled by lagged assets (*ROA*), (ii) total factor productivity (*TFP*) and (iii) sales growth (*SALESGROWTH*). *TFP* measures how efficiently capital and labor are used in the production process. Specifically, it captures the portion of production (output) not explained by the inputs used in firms' production process. Similar to the approach used in studies such as Palia and Lichtenberg (1999), Schoar (2002), and

Shroff et al. (2014), we measure *TFP* as the residual from the following log-linear Cobb-Douglas production function:

$$Log(Y_{jt}) = \propto_0 + \propto_1 Log(K_{jt}) + \propto_2 Log(L_{jt}) + \varepsilon_{jt}, \qquad (2)$$

where *Y* is the output measured as net annual sales, *K* is the capital measured as net property, plant, and equipment, and *L* is the labor measured as the number of employees. We estimate the above equation at the SIC two-digit level separately for each year. Higher the residual in equation (2), higher is the excess productivity garnered by the firm for a given unit of labor and capital. Therefore, higher *TFP* implies greater operating performance.

The monitoring channel predicts an increase in *ROA*, *TFP*, and *SALESGROWTH* following reporting frequency increases because a reduction in overinvestment should allow firms to generate prior level of earnings by deploying lower levels of resources. The myopia channel, on the other hand, predicts either a decrease in *ROA*, *TFP*, and *SALESGROWTH* or no change in either variable.

We estimate the following specification to examine the effect of reporting frequency on operating performance:

$$PERFORMANCE_{i,t} = \gamma_0 + \gamma_1 TREAT_i + \gamma_2 AFTER_{it} + \gamma_3 TREAT_i * AFTER_{it}$$

$$+\Delta FIRM + \Theta YEAR + \varepsilon_{it}\ , \qquad (3)$$

where *PERFORMANCE* represents either *ROA*, *TFP*, or *SALESGROWTH*. The coefficient of interest is γ_3, which captures the effect of reporting frequency increase on a firm's subsequent operating performance.

Table 7 presents the results of this analysis. Columns (1), (2), and (3) present results for *ROA*, *TFP*, and *SALESGROWTH*, respectively. The coefficient on *TREAT*AFTER* in columns (1) and (2) is not significantly different from zero (For *ROA*, coefficient=0.004 and t-statistics=0.552; For *TFP*, coefficient=-0.003 and t-statistics=-0.171), indicating that there is no significant change in *ROA* and *TFP* following reporting frequency increases. In contrast, when *SALESGROWTH* is the operating performance measure, coefficient on *TREAT*AFTER* is negative and significant in column (3) (coefficient = −0.039 and t-statistic = −2.782), indicating that firms exhibit an average decline of 3.9% in annual sales growth following reporting frequency increases.[13] Collectively, we view this evidence as inconsistent with the monitoring channel.

5.3 Partitioning on propensity to overinvest

The evidence thus far suggests that the decline in investments is more likely to be attributable to the myopia channel than the monitoring channel. To provide more direct evidence on the potential role of monitoring channel, we examine whether the decline in investments following reporting frequency increases is driven by firms that are more likely to overinvest prior to the reporting frequency increase. Prior work suggests that managers are more likely to overinvest when there is sufficient financial slack available to engage in overinvestment (Jensen, 1986 and Richardson, 2006). Therefore, we examine whether the decline in investments is more pronounced when the managers propensity to overinvest is greater during the pre-treatment periods.

To accomplish this, we create an indicator variable for firms with high propensity to overinvest (*OVERINVEST*) using one of three different proxies for financial slack. Our first proxy measures the availability of excess cash beyond the normal operating and investing requirements and is estimated using the residuals from a regression that models the normal levels

[13] An alternative explanation for the decline in sales growth is abandonment of unprofitable customers, which potentially increases firm value. However, lack of significant results for *ROA* indicates that this possibility is unlikely because abandonment of unprofitable sales should, if anything, increase *ROA*.

of cash holdings.[14] Specifically, following Fresard and Selva (2010), we obtain the residuals from industry level annual cross-sectional regressions of cash scaled by assets on (i) total assets, (ii) operating income before depreciation scaled by lagged assets, (iii) net working capital scaled by assets, (iv) investment opportunities, (v) capital expenditure scaled by assets, (vi) book leverage, and (vii) common dividend scaled by assets. We estimate these regressions at the two-digit SIC level. We code *OVERINVEST* as 1 for firms with above median values of excess cash for the year prior to the treatment (reporting frequency increase) year.

Our second proxy for the propensity to overinvest is based on the firm's ability to pay dividends. Dividend payments indicate availability of internal cash and have been used in prior work as a measure of financing constraints. Under this approach, we code *OVERINVEST* as 1 for firms that paid a common dividend for the year prior to the treatment year.

The third proxy we consider is an index of financing constraints based on Kaplan and Zingales (1997). Firms with higher values of the Kaplan-Zingales index are more likely to experience difficulties financing their ongoing operations. For this approach, we code *OVERINVEST* as 1 for firms with below median values of Kaplan-Zingales index for the year prior to the treatment year.[15]

In Table 8, we present results of estimating equation (1) after including *OVERINVEST* and its interaction term with *TREAT*AFTER* as additional covariates. The coefficient on *TREAT*AFTER* represents the effect of reporting frequency increase for firms with low propensity to overinvest, and coefficient on *TREAT*AFTER*OVERINVEST* represents the

[14] Inferences remain unchanged if we use total cash balance to measure financial slack.

[15] Kaplan-Zingales Index is calculated as 1.001909*(net income + depreciation and amortization expense)/lagged total assets + 0.2826389*(Total assets-book value of common equity-deferred tax _balance sheet + market cap of common equity)/total assets + 3.139193* Total debt/total assets – 39.3678*total dividend/lagged total assets 1.314759* cash and equivalent/lagged total assets.

incremental effect for firms with high propensity to overinvest. The monitoring channel predicts a negative coefficient on *TREAT*AFTER*OVERINVEST*.

Table 8 presents results for the three proxies of propensity to invest for both investment variables, *INVGROSS* and *CAPEX*. Across all columns (1) through (8), the coefficient on *TREAT*AFTER* is negative and statistically significant. Thus, we find that, on average, firms with a lower propensity to overinvest experience a decline in investments following a reporting increase. More important, the coefficient on *TREAT*AFTER*OVERINVEST* is not negative as predicted by the monitoring channel. In fact, the coefficient is positive and statistically significant in six out of eight specifications. In only two of the specifications where Excess Cash is the proxy the coefficient is positive but not statistically significant. Thus, inconsistent with the monitoring channel, firms with higher propensity to overinvest appear to invest similarly or more relative to other treatment firms.

Together, our evidence from a series of additional tests suggests that the decline in investments is more likely to be due to myopic behavior on the part of managers rather than optimal change in investments due to greater monitoring efforts from increased reporting frequency. In other words, our findings are more consistent with increased reporting frequency imposing a cost on the firm's shareholders by managerial underinvestment.

6. Conclusions

This paper examines the real effects of increasing the financial reporting frequency. We use a 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, to examine whether firms that increased the reporting frequency changed their investment patterns following the reporting frequency increase. We explore three possible reasons why managers may change their investments following a reporting frequency increase, each with differing predictions.

We find a statistically and economically significant decline in investments following reporting frequency changes. A series of tests helps disentangle alternative explanations for the decline in investments. We conclude based on our collective analysis that the decline in investments is consistent with managerial myopia. That is, consistent with theoretical predictions in Gigler et al. (2013) we find that increasing the frequency of financial reporting motivates managers to reduce the level of investments to achieve improved short term performance at the expense of future performance. Thus, we document that the underinvestment problem is exacerbated in the presence of frequent financial reporting and imposes real costs on a firm's shareholders. This evidence is an important contribution to the literature because most prior empirical research focuses on the benefits of reporting frequency.

Our paper has implications for practice because several countries including Europe, Singapore and Australia have debated about 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 different view. We provide evidence that increasing the frequency has important "real" effects in the form of reduction in investments, consistent with myopic managerial behavior. Thus, our paper highlights the importance of understanding the costs associated with mandating an increase in reporting frequency. Whether these costs outweigh the benefits or vice versa will ultimately provide answers on the policy decision to mandate an appropriate frequency of financial reporting. Evaluating this tradeoff is an important question for future research.

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Table 1: Industry distribution and descriptive statistics

Panel A presents the industry distribution for treatment observations (cases with reporting frequency increase) and control observations (cases with unchanged reporting frequency) using the Fama-French 10 industry classification. Panel B presents covariate balance across the treatment and control firms. Panel C presents the descriptive statistics of the key variables for the entire sample (comprising a maximum of 12,352 firm-year observations), which includes data for up to 2 years before and 5 years after the treatment year. *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 dummies estimated at 2-digit SIC industry level. *EBITDA* is the 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. *INVGROSS* is the change in gross fixed assets scaled by beginning of year assets. *CAPEX* is the capital expenditure scaled by beginning of year assets.

Panel A: Time series distribution of treatment firms

Year	Frequency Increase to			Total
	Semi-annual	Three times	Quarterly	
1950-1954	17	11	32	60
1955-1969	152	108	440	700
1970-1974	10	18	188	216
Total	179	137	660	976

Panel B: Industry distribution

Industry	Treatment firms	Control firms
Consumer Durables	49	53
Energy	50	48
Hi-Tech	67	91
Healthcare	28	18
Manufacturing	336	320
Consumer Non-Durables	159	170
Shops	172	167
Telecom	7	8
Other	108	101
Total	976	976

Table 1 (continued)

Panel B: Covariate balance across treatment and control firms

	Mean		
	Treatment	Control	Difference (p-value)
ASSETS ($ millions)	75.785	67.195	8.589 (0.301)
INVESTOPP	1.592	1.590	0.001 (0.962)
EBITDA	0.202	0.203	-0.000 (0.944)
LEVERAGE	0.146	0.144	0.002 (0.703)
CASH	0.116	0.115	0.000 (0.961)
Observations	976	976	

Panel D: Descriptive statistics for the full sample

	Mean	StDev	25th percentile	50th Percentile	75th Percentile
INVGROSS	0.078	0.133	0.015	0.046	0.100
CAPEX	0.087	0.083	0.035	0.063	0.109
ASSETS	83.533	199.396	9.700	24.200	62.600
EBITDA	0.189	0.125	0.110	0.173	0.253
INVESTOPP	1.539	0.645	1.106	1.421	1.864
LEVERAGE	0.150	0.134	0.029	0.130	0.231
CASH	0.110	0.096	0.041	0.077	0.150

Table 2: Reporting frequency and investments

This table presents evidence on the effect of increased reporting frequency on investments. Measures of investments include: (i) change in gross fixed assets scaled by beginning of year assets (*INVGROSS*) and (ii) capital expenditure scaled by beginning of year assets (*CAPEX*). *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. All control variables are defined in the caption of 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.

	(1)	(2)	(3)	(4)
	INVGROSS	*CAPEX*	*INVGROSS*	*CAPEX*
TREAT	0.020***	0.013***	0.022***	0.013***
	(3.528)	(3.168)	(4.311)	(3.465)
AFTER	0.001	-0.000	0.008**	0.002
	(0.347)	(-0.132)	(2.259)	(0.794)
TREAT*AFTER	**-0.017*** **	**-0.010** **	**-0.019*** **	**-0.010** **
	(-2.601)	**(-2.024)**	**(-3.416)**	**(-2.252)**
EBITDA			0.446***	0.213***
			(11.49)	(8.200)
INVESTOPP			0.027***	0.011*
			(2.948)	(1.872)
Firm fixed effects	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES
Observations	12,352	10,519	12,352	10,519
R-squared	0.250	0.455	0.364	0.508

Table 3: 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) change in gross fixed assets scaled by beginning of year assets (*INVGROSS*) and (ii) capital expenditure scaled by beginning of year assets (*CAPEX*). *TREAT* is an indicator for treatment firms, which are firms that experience an increase in reporting frequency. *BEFORE* is a dummy variable that equals one for firm-year observations one year before the treatment year and zero otherwise. *AFTER(+1,+2)* is a dummy 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. All control variables are defined in the caption of 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.

	(1)	(2)	(3)	(4)
	INVGROSS	*CAPEX*	*INVGROSS*	*CAPEX*
TREAT	0.018***	0.009**	0.022***	0.013***
	(2.855)	(1.978)	(4.365)	(3.512)
BEFORE	0.006	-0.001		
	(1.200)	(-0.259)		
TREAT*BEFORE	**0.006**	**0.007**		
	(0.745)	**(1.329)**		
AFTER	0.011**	0.002		
	(2.509)	(0.510)		
TREAT*AFTER	**-0.015****	**-0.006**		
	(-2.197)	**(-1.110)**		
AFTER(+1,+2)			0.009**	0.003
			(2.501)	(1.205)
TREAT*AFTER(+1,+2)			**-0.017*****	**-0.009***
			(-2.836)	**(-1.890)**
AFTER(+3,+5)			0.006	0.000
			(1.429)	(0.161)
TREAT*AFTER(+3,+5)			**-0.020*****	**-0.010****
			(-3.299)	**(-2.253)**
EBITDA	0.445***	0.213***	0.444***	0.211***
	(11.48)	(8.194)	(11.44)	(8.149)
INVESTOPP	0.027***	0.011*	0.027***	0.011*
	(2.947)	(1.866)	(2.962)	(1.892)
Firm fixed effects	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES
Observations	12,352	10,519	12,352	10,519
R-squared	0.364	0.508	0.364	0.508

Table 4: Robustness tests

This table presents various robustness checks of the effect of increased reporting frequency on investments documented in prior tables. Measures of investments include: (i) change in gross fixed assets scaled by beginning of year assets (*INVGROSS*) and (ii) capital expenditure scaled by beginning of year assets (*CAPEX*). *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. Panel A presents robustness to use of two alternative measures of investment opportunities: (i) annual sales growth (*SALESGROWTH*) and (ii) industry level growth opportunities measured as the size-weighted Tobin's Q of all firms within the same two-digit SIC industry (*Q_SIC*). Panel B presents robustness to inclusion of controls for book leverage, cash holdings, and firm size. All control variables are defined in the caption of Table 1. Panel C presents robustness to inclusion of industry-year fixed effects measured using the Fama-French 10 industry classification. *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: Alternative proxies for growth opportunities

	(1)	(2)	(3)	(4)
	INVGROSS	*CAPEX*	*INVGROSS*	*CAPEX*
TREAT	0.013***	0.010***	0.023***	0.013***
	(2.808)	(2.882)	(4.395)	(3.451)
AFTER	0.005	0.002	0.008**	0.002
	(1.597)	(0.617)	(2.313)	(0.782)
TREAT*AFTER	**-0.012****	**-0.008***	**-0.019*****	**-0.010****
	(-2.275)	**(-1.854)**	**(-3.473)**	**(-2.257)**
EBITDA	0.282***	0.179***	0.525***	0.243***
	(10.370)	(9.547)	(17.990)	(14.310)
SALESGROWTH	0.160***	0.044***		
	(14.780)	(6.950)		
Q_SIC			0.007	0.011**
			(0.906)	(1.971)
Firm fixed effects	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES
Observations	12,352	10,519	12,352	10,519
R-squared	0.443	0.521	0.362	0.508

Table 4 (continued)

Panel B: Additional control variables

	(1) *INVGROSS*	(2) *CAPEX*
TREAT	0.029***	0.016***
	(5.380)	(4.043)
AFTER	0.004	0.001
	(1.271)	(0.365)
TREAT*AFTER	**-0.023*** **	**-0.011** **
	(-4.015)	**(-2.533)**
EBITDA	0.410***	0.198***
	(10.76)	(7.398)
INVESTOPP	0.021**	0.007
	(2.260)	(1.206)
LEVERAGE	-0.196***	-0.110***
	(-8.561)	(-7.263)
CASH	0.120***	0.045**
	(4.072)	(2.435)
LOG(ASSETS)	0.053***	0.022***
	(8.879)	(5.335)
Firm fixed effects	YES	YES
Year fixed effects	YES	YES
Observations	12,350	10,517
R-squared	0.396	0.524

Table 4 (continued)

Panel C: Controlling for time varying industry shocks

	(1) INVGROSS	(2) CAPEX
TREAT	0.023***	0.012***
	(4.561)	(3.384)
AFTER	0.008**	0.002
	(2.479)	(0.612)
TREAT*AFTER	-0.020***	-0.010**
	(-3.643)	(-2.320)
EBITDA	0.437***	0.205***
	(10.67)	(7.780)
INVESTOPP	0.030***	0.011*
	(2.993)	(1.852)
Firm fixed effects	YES	YES
Year fixed effects	YES	YES
Industry-Year fixed effects	YES	YES
Observations	12,352	10,519
R-squared	0.387	0.528

Table 5: Mandatory adopters

This table examines the effect of increased reporting frequency on investments on the subsample where treatment firms increased their reporting frequency due to mandated rule changes. Following Butler et al. (2007), mandatory increases are defined as increases to semiannual reporting frequency after 1954 and increases to quarterly reporting frequency after 1967. Measures of investments include: (i) change in gross fixed assets scaled by beginning of year assets (*INVGROSS*) and (ii) capital expenditure scaled by beginning of year assets (*CAPEX*). *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. All control variables are defined in the caption of 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.

	(1) *INVGROSS*	(2) *CAPEX*
TREAT	0.020**	0.017**
	(2.191)	(2.499)
AFTER	0.013**	0.005
	(2.145)	(1.267)
TREAT*AFTER	**-0.021****	**-0.016*****
	(-2.528)	**(-2.639)**
EBITDA	0.449***	0.239***
	(8.377)	(6.600)
INVESTOPP	0.028**	0.011
	(2.102)	(1.301)
Firm fixed effects	YES	YES
Year fixed effects	YES	YES
Observations	5,606	5,031
R-squared	0.420	0.559

Table 6: Effect of earnings timeliness

This table presents evidence on the effect of earnings timeliness on the relation between reporting frequency and investments. Earnings timeliness (*TIMELY*) is measured at the two-digit SIC level using the coefficient estimate on earnings from annual cross-sectional regressions of stock returns on earnings scaled by market value of equity. Specifically, *TIMELY* is an indicator that equals one if the coefficient on earnings falls above the median value of the distribution. The coefficient estimates are obtained from an augmented version of Eq. (1) that includes *TIMELY* and its interaction with *TREAT*AFTER* as additional covariates. *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. Measures of investments include: (i) change in gross fixed assets scaled by beginning of year assets (*INVGROSS*) and (ii) capital expenditure scaled by beginning of year assets (*CAPEX*). All control variables are defined in the caption of 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.

	(1) *INVGROSS*	(2) *CAPEX*
TREAT	0.021***	0.011***
	(3.946)	(3.034)
AFTER	0.006*	0.001
	(1.715)	(0.396)
TREAT*AFTER	**-0.021*** **	**-0.014*** **
	(-3.184)	**(-2.957)**
TIMELY	-0.001	-0.004
	(-0.320)	(-1.543)
TREAT*AFTER*TIMELY	**0.006**	**0.010** **
	(1.021)	**(2.390)**
EBITDA	0.427***	0.217***
	(11.51)	(8.459)
INVESTOPP	0.031***	0.010*
	(3.435)	(1.705)
DID Estimate when *TIMELY*=1:		
TREAT*AFTER + TREAT*AFTER*TIMELY	**-0.014** **	**-0.004**
	(-2.212)	**(-0.874)**
Firm fixed effects	YES	YES
Year fixed effects	YES	YES
Observations	11,165	9,613
R-squared	0.375	0.520

Table 7: Reporting frequency and operating performance

This table presents evidence on the effect of reporting frequency increase on operating performance. Measures of operating performance include: (i) operating income scaled by lagged assets (*ROA*), (ii) total factor productivity (*TFP*) measured as the residual from annual regressions at SIC two-digit level of the log-linearized Cobb-Douglas production function, and (iii) annual sales growth (*SALESGROWTH*). *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. *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.

	(1) ROA	(2) TFP	(3) SALESGROWTH
TREAT	-0.006	-0.018	0.053***
	(-0.989)	(-1.046)	(3.965)
AFTER	-0.012***	0.015	-0.000
	(-2.852)	(1.424)	(-0.005)
TREAT*AFTER	**0.004**	**-0.003**	**-0.039*** **
	(0.552)	**(-0.171)**	**(-2.782)**
Firm fixed effects	YES	YES	YES
Year fixed effects	YES	YES	YES
Observations	12,394	10,184	12,432
R-squared	0.569	0.756	0.214

Table 8: Propensity to overinvest

This table presents evidence on how the decline in investments following reporting frequency increases depends on firms' propensity to overinvest prior to the increase in reporting frequency. Coefficient estimates are obtained from an augmented version of Eq. (1) that includes a dummy for firms with high propensity to overinvest (*OVERINVEST*) and its interaction term with *TREAT*AFTER* as additional covariates. *OVERINVEST* is measured using three different approaches: (i) dummy for greater than median value of excess cash, where excess cash is measured as the residual from a model of normal level of cash holdings from Fresard and Selva (2010), (ii) dummy for payment of common dividends, and (iii) dummy for below median value of financing constraints index from Kaplan and Zingales (1997). *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. *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.

Propensity to overinvest measured using	Excess Cash		Dividend payment		Kaplan-Zingales index	
	(1)	(2)	(3)	(4)	(5)	(6)
	INVGROSS	*CAPEX*	*INVGROSS*	*CAPEX*	*INVGROSS*	*CAPEX*
TREAT	0.019***	0.013***	0.022***	0.012***	0.021***	0.011***
	(3.251)	(3.283)	(4.235)	(3.350)	(3.931)	(2.906)
AFTER	0.006	0.001	0.008**	0.002	0.008**	0.002
	(1.534)	(0.496)	(2.329)	(0.825)	(2.267)	(0.676)
TREAT*AFTER	**-0.021*** **	**-0.010** **	**-0.027*** **	**-0.019*** **	**-0.033*** **	**-0.018*** **
	(-2.993)	**(-2.033)**	**(-3.489)**	**(-2.915)**	**(-4.769)**	**(-3.193)**
OVERINVEST	-0.004	-0.005*	-0.010	-0.012**	-0.014***	-0.007**
	(-0.952)	(-1.781)	(-1.523)	(-2.493)	(-2.790)	(-2.039)
TREAT*AFTER*OVERINVEST	**0.007**	**0.001**	**0.013** **	**0.013** **	**0.028*** **	**0.016*** **
	(0.962)	**(0.285)**	**(1.988)**	**(2.494)**	**(4.562)**	**(3.314)**
EBITDA	0.406***	0.209***	0.447***	0.213***	0.431***	0.210***
	(8.148)	(6.982)	(11.530)	(8.269)	(10.470)	(7.756)
INVESTOPP	0.033***	0.012*	0.027***	0.011*	0.029***	0.012**
	(3.009)	(1.885)	(2.939)	(1.849)	(3.099)	(2.025)
DID estimate when *OVERINVEST*=1:						
TREAT*AFTER + TREAT*AFTER*OVERINVEST	**-0.015** **	**-0.009* **	**-0.014*** **	**-0.005**	**-0.006**	**-0.002**
	(-2.132)	**(-1.828)**	**(-2.676)**	**(-1.286)**	**(-1.044)**	**(-0.545)**
Firm and year fixed effects	YES	YES	YES	YES	YES	YES
Observations	8,290	8,127	12,339	10,515	10,885	9,483
R-squared	0.369	0.500	0.364	0.509	0.366	0.507