Real Effects of Frequent Financial Reporting

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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' investment decisions. Estimates from difference-in-differences specifications show that increased reporting frequency is associated with an economically large decline in investments. Additional analyses reveal that these findings are most consistent with managers behaving myopically following increases in reporting frequency. We provide some of the first archival evidence on the potential costs of increasing financial reporting frequency.

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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 extensive debate by regulatory bodies across the globe. Proponents of frequent reporting (e.g., quarterly reporting) of firms' financials argue that greater frequency improves the timeliness of earnings and reduces information asymmetry between managers and shareholders. Opponents of frequent reporting cite excessive management focus on short term results and myopic tendencies to report positive performance every 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 citing that less frequent reporting "reduces the administrative burden and encourages long term investment." The purpose of this paper is to inform this ongoing 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 theoretically ambiguous. On the one hand, increased transparency through higher reporting frequency can beneficially affect firms' investment decisions in two ways. First, increased transparency can reduce firms' cost of capital and improve access to external financing, allowing firms to invest in a larger set of positive NPV projects. Second, increased transparency can improve external monitoring and help mitigate over- or under-investment stemming from managerial agency problems. On the other hand, frequent reporting can distort investment decisions. In particular, frequent reporting can cause managers to make myopic investment decisions that boost short term performance measures at the cost of long run firm value (e.g., Gigler et al., 2013; Bhojraj and Libby, 2005). Which of these two forces dominate is an open empirical question that we explore in this study.

To provide 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 that increase reporting frequency) matched to an equal number of control firms (firms with unchanged reporting frequency) based on variables known to be associated with investments such as size, industry, and growth 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 change in investments of treatment firms following reporting frequency increases relative to the contemporaneous change in investments for the control firms.

Our DID estimates suggest that firms significantly reduce investments following an increase in reporting frequency. Specifically, firms which increase their reporting frequency

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

reduce investments in fixed assets by 1.7% of total assets. This is an economically significant decline as it is equivalent to a 22% decline from the mean level of investments. 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 examine the effect of financial slack prior to reporting frequency increases. Because managers are likely to overinvest only when they have surplus cash (e.g., Jensen, 1986), the monitoring channel suggests that the decline in investments should manifest more when there is sufficient financial slack prior to reporting frequency increase. In contrast, the myopia channel predicts greater investment decline when there is less financial slack as managers of such firms face greater capital market pressure to boost short term stock price. Stein (1989) notes that lack of financial slack can cause managers to boost short term earnings at the expense of longer run value in anticipation of future equity issuances and enhanced capital market scrutiny. 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 financial slack. Collectively, the evidence is inconsistent with the monitoring story and more consistent with managerial myopia.

Next, we examine firms' operating performance and operating efficiency 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 and operating efficiency. However, inconsistent with the monitoring channel, we do not find evidence of improvements in firm performance and efficiency. If anything, the evidence is supportive of a decline in performance and efficiency in the years following the increase in reporting frequency.

Finally, 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 timelier 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 show that myopia manifests when managers have private information about the firm's long term prospects. Consequently, in the absence of signals about long term prospects, 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. Managers' ability to inflate shareholders' beliefs, however, would be constrained when earnings are a timely signal of firm's future prospects. 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 manner. 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.

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 offers benefits in the form of reduced cost of capital, improved liquidity and reduced agency costs. Our findings 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 identify different sources of capital market pressures that can induce myopia. For example, Asker et al. (2014) 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 is another mechanism that can encourage myopic managerial behavior.

A caveat is in order. Although we offer evidence on one potential cost of frequent reporting, our paper does not speak necessarily to whether this cost outweighs the benefits 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; Leuz and Verrecchia 2000) 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) documents 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.[2] However, research by Butler et al. (2007) finds evidence that although earnings timeliness

[2] In a descriptive study, Leftwich, Watts and Zimmerman (1981) examine agency explanations for firms that voluntarily report at a higher frequency than that mandated by the regulators. However, their results are unable to support any explanations offered by agency theory.

improves for firms voluntarily changing their reporting frequency, earnings timeliness is unaffected for firms that are forced to shift the reporting frequency via a mandate.

Frequent reporting could also impose significant costs by distorting managers' investment decisions.[3] An extensive body of theoretical work shows that even in efficient capital markets managers can make myopic investment decisions that boost short term profits at the cost of longer run firm value.[4] 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.

Our work is also related to a recent paper by Ernstberger, Link and Vogler (2011) that examines the effect of reporting frequency on real earnings management decisions. Using cross-sectional variation in reporting frequencies across EU countries they document that firms with quarterly reporting have greater abnormal production costs and discretionary expenses. The cross-sectional nature of the study, however, makes it difficult to draw causal inferences because it is hard to separate the effect of reporting frequency from other features of countries'

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

[4] See, for example, Narayanan (1985), Miller and Rock (1985), Stein (1988, 1989), Shleifer and Vishny (1990), Bebchuk and Stole (1993), Von Thadden (1995), and Holmstrom (1999).

institutional environment. In contrast, our study exploits time series variation in reporting frequency regime within a single country to provide causal evidence.

Finally, our paper is related to prior work on the relation between financial reporting quality and investments (e.g., Biddle and Hilary, 2006; Biddle et al., 2009; Shroff et al., 2014; Balakrishnan et al., 2014). These studies provide evidence consistent with greater financial reporting quality having a beneficial effect on firms' investment decisions. We add to this literature by providing causal evidence on the effects on investments of an important feature of firms' reporting environment: financial reporting frequency.

2.2 Hypothesis development

Increased reporting frequency can affect investments through three channels. The first channel is the financing channel. As prior research (e.g., Fu et al. 2012) suggests, timelier disclosure of information through increased reporting frequency reduces a firm's information asymmetry, and consequently its cost of capital. Such a reduction in cost of capital relaxes the firm's financing constraints, thereby allowing the firm to invest in a larger set of positive NPV projects. Thus, the financing channel predicts 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. Monitoring channel therefore suggests a beneficial effect on investment decisions, but the direction of the effect is ambiguous. That is, a reporting frequency increase could cause an increase or decrease in investments depending on whether a firm faces an underinvestment or an overinvestment problem.

The third and final channel is the myopia channel, which, unlike the financing and monitoring channels, adversely affects investment decisions. Building upon early theoretical work (e.g., Stein 1989) on managerial myopia, Gigler et al. (2013) show that increased reporting frequency can cause managers to make myopic investment decisions that boost short term profits at the cost of longer run firm value. Myopia manifests because frequent reporting induces premature evaluation of managerial actions that do not get reflected in reported financial measures in the short run, resulting in short-term price pressure from impatient investors. Bhojraj and Libby (2005) provide experimental evidence that increasing reporting frequency can cause managers to behave myopically.

Bebchuk and Stole (1993) theoretically show that myopia can lead to either too little or too much investment.[5] Extant evidence, however, strongly suggests that myopia manifests in the form of underinvestment. Consistent with the underinvestment perspective, Graham, Harvey and Rajgopal's (2005) provide survey evidence 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. Asker et al. (2014) find that public firms invest considerably less than comparable private firms, consistent with underinvestment behavior by short-term oriented managers. Similarly, Ladika and Sautner (2013) and Edmans et al. (2013)

[5] Bebchuk and Stole (1993) note that the direction of the distortion depends on the nature of information asymmetries between managers and investors. Overinvestment results when manager has better information about the high quality of its investment opportunities, which he signals by overinvesting. Underinvestment results when investors do not know how much the firm should optimally invest.

document that executives with more short-term equity based incentives engage in myopic behavior by reducing real investments. Based on the broad set of theoretical work and empirical evidence, the myopia channel suggests that increased reporting frequency can manifest in either an overinvestment or an underinvestment problem.

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).[6] 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.[7] Panel A provides the frequency distribution of treatment firms across years prior to the SEC's mandating of increased frequency in 1955 (semi-annual) and 1970 (quarterly). As is obvious from the panel, a significant number of firms reported more frequently than that required by the SEC mandate.[8]

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).[9] We also require that the control

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

[7] We also considered a sample of firms that reduced the reporting frequency. However, we were able to identify only a small subset of 71 cases where the firm reduced the reporting frequency without a subsequent reversal of the frequency reduction within 2 years. Nevertheless, we examined the effect of this change on investments but we did not observe any discernible patterns subsequent to the reduction in reporting frequency. The lack of an effect on investments may be due to two reasons. First, we believe that a reduction in reporting frequency is a less credible change because of the predominance of frequency increase both by the SEC and by the stock exchanges. Second, the tests may lack statistical power given the smaller subsample.

[8] Notice also that some firms reported at a frequency less than the mandated level of frequency. We are unable to determine the exact reason for this reduced reporting frequency but conjecture that these firms received some special exemptions from the SEC during this time period. Our inferences are robust to dropping these firms from our analysis.

[9] Note that because our DID design compares the *change* in investment of treatment firms to *change* in investment of control firms, we only require control firms to have no change in reporting frequency. We do not need the matched control firms to have the same reporting frequency as treatment firms either in the pre- or the post-

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.[10] That is we estimate a propensity score model for each year to identify a control firm for each treatment firm in that year.

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*). We estimate the propensity score model separately for each treatment year using a logit specification based on the variables measured prior to the post-treatment period (described later) during which we measure firms' investment response to the reporting frequency increase. We employ nearest neighbor matching and impose the restriction of common support to ensure high match quality.[11]

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

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treatment years. Furthermore, because the vast majority of our sample firms voluntarily report at quarterly frequency even before the mandate, requiring the control firms to have the same pre-treatment reporting frequency as treatment firms greatly reduces sample size, making the analysis infeasible. As noted later, 83% of our control firms are quarterly reporters.

[10] Our inferences are robust to using a simpler approach of identifying control firms based on size and industry.

[11] 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.

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 net additional investments during the year; *TREAT* is an indicator variable for treatment firms; *AFTER* is an indicator 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 Fu et al. (2012), we exclude the treatment year (t=0). *CONTROLS*, *FIRM*, and *YEAR* represent a vector of control variables, firm fixed effects, and year fixed effects, respectively.

Our main coefficient of interest in equation (1) is β_3, the coefficient on the interaction term between *TREAT* and *AFTER*, which measures 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 a greater increase (decrease) in investments for treatment firms relative to contemporaneous change in investments for control firms with no change in reporting frequency.

A distinct advantage of this DID 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.

Although not of primary interest, some remarks about the estimation and economic interpretation of coefficient β_1 on *TREAT* are in order. First, β_1 is estimable even in the presence of firm fixed effects because some firms appear as both treatment and control firms at different points in time.[12] Second, β_1 measures the mean difference in investment levels of treatment and control firms prior to the reporting frequency increase. Coefficient β_1 therefore reflects the effect

[12] For example, a treatment firm that increased its reporting frequency to quarterly in 1955 can get matched as a control firm in 1970 to a treatment firm that increased its reporting frequency to quarterly in 1970. This approach expands the set of control firms for propensity score matching, leading to better match quality and greater sample size. All of our inferences are robust to use of an alternative approach in which we do not allow treatment firms to be control firms and vice versa.

of any systematic differences between treatment and control firms in the determinants of investments in the pre-treatment period. Because treatment and control firms are matched on known determinants of investments and the majority of control firms are quarterly reporters (83%), we expect β_1 to capture the effect of pre-treatment differences in reporting frequencies.[13] For example, if higher reporting frequency induces myopic underinvestment, we would expect β_1 to be positive because, ceteris paribus, control firms with quarterly reporting frequencies would exhibit lower investments than treatments firms that report at lower frequencies in the pre-treatment period. However, in addition to the effect of cross-sectional differences in reporting frequencies, coefficient β_1 could potentially also capture the effect of pre-treatment differences in any unobservable determinants of investments omitted from our propensity score model. Therefore, one cannot draw causal inferences about the effect of reporting frequency from β_1. For causal inferences, we rely on the DID estimate β_3, which is free of the confounding effect of such unobservable cross-sectional differences.

3.2 Measurement of investments

We use three measures of investments that capture firms' growth in fixed assets. Firms can grow fixed assets by building new capacity through capital expenditures, by 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 first 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

[13] Most control firms are quarterly reporters because the vast majority of our sample firms voluntarily report at quarterly frequency even before the mandate. Our inferences remain unchanged if we require 100% of our control firms to be quarterly reporters.

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. 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; Asker et al., 2014, 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*). Our measure of investment opportunities is based on the approach in Campello and Graham (2013) and Asker et al. (2014). 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 fixed effects. *INVESTOPP* is computed for each firm-year as the predicted value from these regressions. Following prior studies, 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, we include firm fixed effects to control for time-invariant firm characteristics. 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 12,350 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 pertaining to *INVGROSS* as the dependent variable, while column (2) uses *CAPEX* as the dependent variable. The coefficient, *TREAT*, is positive and statistically significant (coefficient = 0.020 and 0.013 for *INVGROSS* and *CAPEX* respectively). This suggests that, on average, the treatment firms have higher investment levels when compared to control firms in the pre-treatment years. As explained in Section 3, we expect the coefficient on *TREAT* to capture the effect of pre-treatment differences in reporting frequencies in treatment and control firms because the vast majority of our control firms (83%) are quarterly reporters. Positive coefficient on *TREAT* therefore provides preliminary cross-sectional evidence that reporting frequency is negatively associated with investment levels.

We next turn to our main coefficient of interest on the interaction term *TREAT*AFTER*, which captures the DID estimate of the effect 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) suggest 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.[14]

Coefficient estimates reported in columns (3) and (4) indicate that the results are robust to the inclusion of control variables. In fact, the statistical significance is greater for the interaction term. 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: For the base model, a 1.7% change in *INVGROSS* – our comprehensive measure of investments – represents a 22%

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[14] Combining the main effect (*TREAT*) with the interaction effect (*TREAT*AFTER*) we can infer that the treatment firms' investment levels are similar to those of the control firms in the post-treatment period when both treatment and control firms are predominantly quarterly reporters. That is, the combined effect of 0.003 (0.020-0.017) in column (1) and 0.003 (0.013- 0.010) in column (2)) is not significantly different from zero.

change in its unconditional mean value of 7.8% in our sample (see Table 1, Panel D).[15] 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 1.3 standard deviation decrease in investment opportunities.[16]

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 differential changes in investments immediately prior to the reporting frequency increase. If reporting frequency increase causes investment changes, we would expect to see the changes only in the period after the reporting frequency change but not before. We create an indicator variable *BEFORE(-1)*, which is coded as one for the one year period prior to the reporting frequency increase and zero otherwise. We then estimate equation (1) augmented with *BEFORE(-1)* and an interaction term *TREAT*BEFORE(-1)*. Estimates in columns (1) and (2) with *INVGROSS* and *CAPEX* as dependent variables show that the coefficient estimates on the interaction term, *TREAT*BEFORE(-1)*, are statistically indistinguishable from zero. Consistent with a causal interpretation, this indicates that the decline in investments manifests only after the reporting frequency increase.[17] This finding also mitigates concerns about anticipation effects and reverse causality. The coefficients on the main variable of interest, *TREAT*AFTER*, continue

[15] Given the economically significant decline in investments one might wonder why firms would voluntarily change their reporting frequency. Clearly, rational managers must weigh the cost benefit tradeoffs associated with the reporting frequency change. We conjecture that managers may weigh the benefits from greater liquidity, lower cost of capital (Fu et al., 2012) and reduced contracting costs against the costs of reporting frequency change. However, we are unable to evaluate the specific nature of benefits that managers consider when evaluating the cost benefit tradeoff.

[16] The effect of a 1.3 standard deviation change in investment opportunities on *INVGROSS* equals the coefficient on *INVESTOPP* * standard Deviation of *INVESTOPP**1.3 = 0.021*0.645*1.3 = 0.017.

[17] 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.

to be negative and economically large, although the coefficient in the *CAPEX* regression is now significant at 13% level.

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 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 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 frequency 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 2.0% during the first two years and then the decrease in investments drops even further to 2.5% (an additional 25% decrease) in the subsequent years. A similar pattern is observed when CAPEX is the dependent variable (column 4).

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 used 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. In particular, the coefficients on the interaction term *TREAT*AFTER*, continue to be negative and statistically significant.

Next, in Table 4, Panel B, 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. 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.[18] 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 -0.024 and for *CAPEX* model is -0.011. These results suggest that our findings cannot be explained by any industry level shocks that concurrently occur 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 and mandatory changes in reporting frequency. It is hard to clearly classify voluntary changers because firms may be required under some exchange rules or pressure from the exchanges to report at a higher frequency than that mandated by the SEC.[19] Nevertheless, including firms that voluntarily changed their reporting frequency in the sample raises the concern that our findings

[18] Inferences remain unchanged if we use SIC two-digit level industry classification.
[19] Butler, et al. (2007) discuss the efforts of the NYSE to encourage firms to adopt quarterly reporting. These efforts began as early as 1923 and continued until mandatory quarterly reporting was adopted in 1970. Unlike the NYSE, during this early period neither the AMEX nor the regional exchanges encouraged firms to adopt quarterly reporting.

may be due to unobservable firm factors that drive the treatment firms' decision to increase reporting frequency. It is worth noting that the DID design mitigates the confounding effects of such unobservable differences between treatment and control firms. However, 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.[20] 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 -0.025 for *INVGROSS* and -0.018 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 that firms experience a decline in investments following increases in reporting frequency. The decline in investments is inconsistent with the financing channel, which predicts an increase in investments due to reduction in cost of capital. Thus, the decline is attributable either to the myopia channel or the monitoring channel. The myopia channel suggests that the reduced investment reflects myopic

[20] Butler et al. (2007) use the years 1954 and 1967 instead of the actual SEC mandate years to accommodate 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 unchanged.

underinvestment due to increased capital market pressures to achieving short term performance objectives. Reduction in investment avoids depreciation expense and any attendant interest costs associated with necessary debt financing thereby improving earnings in the short run. In addition, reduced capital expenditures can increase free cash flows in the short run, which are often used by financial analysts to value firms. The monitoring channel, on the other hand, argues that reduced investment reflects a correction in prior overinvestment that existed due to unresolved agency problems; timelier release of information through increased reporting frequency facilitates external monitoring, reducing managerial agency problems and the associated overinvestment. In the sections that follow we conduct a battery of additional tests to disentangle between the myopia and monitoring explanations for our findings.

5.1 Financial slack tests

We first exploit the contrasting predictions offered by the monitoring and myopia channels about the effect of financial slack. Monitoring channel predicts that the decline in investments should be more pronounced for firms that had greater financial slack prior to the reporting frequency increase. Managers are more likely to overinvest when there is sufficient financial slack available to engage in overinvestment (Jensen, 1986 and Richardson, 2006). Therefore, if the decline in investment reflects a correction in prior overinvestment, we expect it to manifest for firms that had more financial slack prior to the reporting frequency increase.

The myopia channel predicts the opposite. Models of myopia show that myopia is more likely to manifest when there is greater capital market pressure and managers care more about short term stock price. Stein (1989) notes that lack of financial slack can be a source of capital market pressure. Managers of firms with less slack have greater incentives to pump short term earnings at the expense of longer run value in anticipation of future equity issuances and enhanced capital market scrutiny. Financial slack insulates managers from such capital market pressures.[21] Thus, the myopia channel predicts that the decline in investments is less pronounced when the firm has greater financial slack in the pre-treatment periods.

To determine which of these two predictions are borne in the data, we divide the sample into high slack and low slack samples using three different proxies for financial slack, all measured prior to the reporting frequency increase. Our first proxy for slack is the availability of cash on the firm's balance sheet. We classify firms with above (below) median values of cash scaled by assets as high (low) slack firms.[22] Our second proxy for financial slack is based on the firm's ability to pay dividends. Dividend payments indicate availability of free cash flow and have been used in prior work as a measure of financing constraints. Under this approach, firms that paid a common dividend for the year prior to the treatment year are classified as high slack firms. The last 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. Therefore, we classify firms with below median values of Kaplan-Zingales index for the year prior to the treatment year as high slack firms.[23]

We estimate separate equations for the two sub-samples. However, to facilitate comparisons of the coefficients across these two sub-samples we estimate the equations jointly as a seemingly unrelated regression. As before, the variables of interest are *TREAT* and *TREAT*AFTER*. The monitoring channel predicts that the coefficient on *TREAT* is greater for

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[21] Other reasons that cause managers to care about short term performance considered in the literature include career concerns, stock based compensation, takeover threat, and presence of impatient investors. We are unable to measure these incentives because of lack of data during our sample period.

[22] Our inferences are robust if we use a measure of excess cash following the approach in Fresard and Selva (2010).

[23] 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.

high slack subsample because treatment firms in this subsample are more likely to exhibit overinvestment prior to the reporting frequency increase. Additionally, the coefficient on *TREAT*AFTER* should be more negative for the high slack subsample to reflect the correction of prior overinvestment of treatment firms following reporting frequency increase.

The myopia channel predicts the opposite. Coefficient on *TREAT* is expected to be greater for the subsample with low slack. In this subsample, control firms that already report quarterly are more likely to myopically underinvest relative to the treatment firms in the pre-treatment period due to greater capital market pressures. Additionally, coefficient on *TREAT*AFTER* should be more negative because treatment firms would be expected to exhibit greater myopic underinvestment following the reporting frequency increase when financial slack is lower.

Table 6 presents results for the three proxies of slack and for both investment variables, *INVGROSS* and *CAPEX*. Inconsistent with the monitoring channel, we find that the *TREAT* coefficient is higher for the low slack sub-sample, with the difference between low and high slack sub-samples being significant for two out of three proxies of financial slack. We also find that across all the proxies the coefficient on *TREAT*AFTER* is less negative for firms with higher financial slack, which is also inconsistent with the monitoring channel. In fact, for two out of three proxies the coefficient is statistically less negative for firms with higher financial slack. Thus, we find that the decline in investments following a reporting increase is greater for firms which are likely to have greater incentives to engage in myopic behavior and lower ability to engage in overinvestment. Collectively, this evidence is inconsistent with the monitoring channel and more consistent with the myopia channel.

5.2.Operating performance tests

We next examine the implications of the monitoring channel for operating performance and efficiency. The monitoring channel predicts improved firm operating performance and efficiency following reporting frequency increases. If the decline in investments following reporting frequency increases represents correction of prior overinvestment, then firms should be able to generate prior levels of economic output by deploying fewer resources. This should result in improvements in both operating performance and efficiency.

Under the myopia channel, however, the prediction is ambiguous. Myopia channel suggests that managers will make investment choices that improve operating performance in the short run, but worsen it in the longer run. However, for going concern firms, the aggregate performance measure in each period contains the short term performance implications of the most recent investment choices as well as the long term performance implications of previous (older) investment choices. Thus the overall effect of myopic investment choices on performance in each period is ambiguous. We therefore do not make any predictions under the myopia channel regarding aggregate performance metrics.

We use three measures of efficiency of resource deployment: (i) return on assets measured as operating income scaled by lagged assets (*ROA*), (ii) asset turnover measured as sales scaled by lagged assets (*ASSETTURN*), and (iii) total factor productivity (*TFP*). While *ROA* and *ASSETTURN* capture the aggregate efficiency of deployment of total assets, *TFP* measures how efficiently capital and labor are used in the production process. Specifically, *TFP* 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. The higher the residual in equation (2), the greater is the excess productivity garnered by the firm for a given unit of labor and capital. The monitoring channel predicts an increase in *ROA*, *ASSETTURN*, and *TFP*.

Finally, we also use annual sales growth (*SALESGROWTH*) as a measure of operating performance. If the decline in investments represents a correction in prior overinvestment, then the decline should not lead to a decrease in sales growth.

We estimate the following specification to examine the effect of reporting frequency on the above four performance measures:

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

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

where *PERFORMANCE* represents either *ROA*, *ASSETTURN*, *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), (3) and (4) present results for *ROA*, *ASSETTURN*, *TFP*, and *SALESGROWTH*, respectively. The coefficient on *TREAT*AFTER* for the ROA regression (column 1) is not significantly different from zero (coefficient=0.004 and t-statistics=0.625). Note that with reduced investments, *ceteris paribus*, we would expect ROA to mechanically increase. Our finding that the ROA does not increase despite this mechanical relation makes for a stronger case against the monitoring channel. For *ASSETTURN*, the coefficient on *TREAT*AFTER* is negative and statistically significant at the

10% level (see column 2). This suggests that treatment firms experience a decline in asset usage efficiency following reporting frequency increase. For *TFP*, the coefficient on TREAT*AFTER although negative (coefficient = -0.003) is not statistically significant (t-statistics = -0.168), indicating that there is no significant change in *TFP* following reporting frequency increases. Finally, when *SALESGROWTH* is the operating performance measure, coefficient on *TREAT*AFTER* is negative and significant in column (4) (coefficient = ─0.040 and t-statistic = -2.841), indicating that firms exhibit an average decline of 4.0% in annual sales growth following reporting frequency increases.[24] Collectively, the evidence indicates that firms do not experience an improvement in operating performance following an increase in reporting frequency. If anything, the evidence is supportive of a decline in operating performance. These results are inconsistent with the monitoring channel and broadly consistent with the myopia channel.

5.3 Earnings timeliness

In this section, we examine the effect of earnings timeliness to further distinguish between monitoring and myopia channels. A key ingredient of models of myopic behavior is the presence of information asymmetries between the managers and the investors about the firm's business prospects. In these models, managers have private information about the firm's long term prospects. Consequently, in the absence of signals about firms' long term prospects, 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 the stock price by boosting current period performance measures. Therefore managers, who care sufficiently about near term stock price, have

[24] 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*.

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 manner. For example, a manager that reduces investments by delaying replacement of old assets is likely to face asset impairments that would get immediately reflected in earnings in such industries. 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. Myopia channel therefore suggests that the decline in investments following reporting frequency increases should be mitigated in industries where earnings provide timely information.

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.[25] 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.

[25] Inferences are similar if we use the R-square from this regression or asymmetric loss recognition measured using Basu (1997) regressions.

To examine the effect of earnings timeliness on the relation between change in investments and reporting frequency increases, we create a high timeliness and a low timeliness subsample depending on whether a firm's earnings timeliness (i.e., the coefficient in the earnings-returns relation) falls above or below the median timeliness. We measure timeliness one year prior to the treatment year and estimate equation (1) for the two subsamples separately as a seemingly unrelated regression. Table 8 presents the results of this analysis. The coefficient of interest in Table 8 is again the interaction term *TREAT*AFTER.* The myopia channel would predict that the coefficient on *TREAT*AFTER* is less negative for the high timeliness subsample whereas the monitoring channel would predict that it is more negative.

Consistent with myopia channel, estimates show that the decline in investments following reporting frequency increase is 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 .4% for *INVGROSS* (although economically large, the difference is not statistically significant). For *CAPEX*, the high timeliness coefficient is lower by about 1.5% of assets and this difference is statistically significant (t-statistic = 1.93). Notice also that the coefficient on *TREAT* is much higher for the low timeliness subsample suggesting that the control firms (which are predominantly quarterly reporters) have much lower investments in the pre-treatment period relative to the treatment firms in this subsample. This is consistent with the control firms exhibiting greater myopic underinvestment in the pre-treatment periods when earnings timeliness is lower. Overall, we view the evidence in Table 8 as more consistent with the myopia channel, rather than the monitoring channel.

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 increases. A series of tests help disentangle alternative explanations for the decline in investments. We conclude based on our collective analysis that the decline in investments is most 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 could impose real costs on a firm’s shareholders. We view this evidence as a useful contribution to the literature particularly because most prior empirical research focuses on the benefits of reporting frequency.

Our paper has implications for practice because several regions 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 more cautionary view. We provide evidence that increasing the frequency has important “real” effects in the form of reduction in investments that is suggestive of myopic managerial behavior. Thus, our paper highlights the importance of understanding one important cost associated with mandating an increase in reporting frequency.

Whether there are additional costs beyond the underinvestment problem that we document here and whether such 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 provides the frequency distribution of treatment observations (cases with reporting frequency increase) across years. 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 covariate balance across the treatment and control firms. Panel D presents the descriptive statistics of the key variables for the entire sample (comprising a maximum of 12,350 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 fixed effects 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 C: Covariate balance across treatment and control firms

	Mean		
	Treatment	Control	Difference (p-value)
Log(ASSETS)	3.121	3.119	0.002 (0.978)
INVESTOPP	1.592	1.590	0.001 (0.957)
EBITDA	0.202	0.203	-0.000 (0.974)
LEVERAGE	0.146	0.143	0.002 (0.649)
CASH	0.116	0.115	0.000 (0.979)
Observations	976	976	

Panel D: Descriptive statistics for the full sample

				(N=12,350)	
	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 ($ millions)	83.550	199.428	9.700	24.200	62.600
EBITDA	0.189	0.125	0.110	0.173	0.253
INVESTOPP	1.538	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***	0.029***	0.016***
	(3.548)	(3.177)	(5.380)	(4.043)
AFTER	0.001	-0.000	0.004	0.001
	(0.376)	(-0.124)	(1.271)	(0.365)
*TREAT*AFTER*	-0.017***	-0.010**	-0.023***	-0.011**
	(-2.627)	(-2.035)	(-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	YES	YES
Year fixed effects	YES	YES	YES	YES
Observations	12,350	10,517	12,350	10,517
R-squared	0.250	0.455	0.396	0.524

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(-1)* is an indicator variable that equals one for firm-year observations one year 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 all control variables (all defined in the caption of Table 1) have been omitted for brevity. *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	0.026***	0.013***	0.029***	0.016***
	(4.079)	(2.689)	(5.442)	(4.092)
BEFORE(-1)	0.005	-0.001		
	(1.060)	(-0.412)		
*TREAT*BEFORE(-1)*	0.005	0.006		
	(0.604)	(1.118)		
AFTER	0.007*	0.000		
	(1.657)	(0.0894)		
*TREAT*AFTER*	-0.020***	-0.008		
	(-2.900)	(-1.509)		
AFTER(+1,+2)			0.006	0.002
			(1.589)	(0.752)
*TREAT*AFTER(+1,+2)*			-0.020***	-0.010**
			(-3.361)	(-2.127)
AFTER(+3,+5)			0.002	-0.001
			(0.552)	(-0.194)
*TREAT*AFTER(+3,+5)*			-0.025***	-0.012**
			(-3.935)	(-2.560)
Controls	YES	YES	YES	YES
Firm fixed effects	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES
Observations	12,350	10,517	12,350	10,517
R-squared	0.397	0.524	0.364	0.525

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*). All control variables are defined in the caption of Table 1. Panel B presents robustness to inclusion of industry-year fixed effects measured using the Fama-French 10 industry classification. Apart from coefficient estimates on alternative measures of investment opportunities in Panel A, coefficient estimates for all other control variables (all defined in the caption of Table 1) have been omitted for brevity. *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) *INVGROSS*	(2) *CAPEX*	(3) *INVGROSS*	(4) *CAPEX*
TREAT	0.019***	0.013***	0.030***	0.016***
	(3.913)	(3.384)	(5.426)	(3.989)
AFTER	0.002	0.000	0.004	0.001
	(0.619)	(0.198)	(1.307)	(0.333)
*TREAT*AFTER*	-0.015***	-0.009**	-0.023***	-0.011**
	(-2.811)	(-2.075)	(-4.045)	(-2.510)
SALESGROWTH	0.160***	0.044***		
	(15.160)	(6.981)		
Q_SIC			0.006	0.011**
			(0.896)	(2.047)
Other controls	YES	YES	YES	YES
Firm fixed effects	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES
Observations	12,350	10,517	12,350	10,517
R-squared	0.475	0.537	0.395	0.525

Table 4 (continued)

Panel B: Controlling for time varying industry shocks

	(1) INVGROSS	(2) CAPEX
TREAT	0.030***	0.016***
	(5.617)	(3.950)
AFTER	0.005	0.000
	(1.513)	(0.176)
*TREAT*AFTER*	-0.024***	-0.011***
	(-4.238)	(-2.590)
Controls	YES	YES
Firm fixed effects	YES	YES
Year fixed effects	YES	YES
Industry-Year fixed effects	YES	YES
Observations	12,350	10,517
R-squared	0.420	0.545

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. Coefficient estimates for all control variables (all defined in the caption of Table 1) have been omitted for brevity. *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	0.026**	0.020***
	(2.563)	(2.873)
AFTER	0.009	0.004
	(1.500)	(1.045)
*TREAT*AFTER*	-0.025***	-0.018***
	(-2.852)	(-2.925)
Controls	YES	YES
Firm fixed effects	YES	YES
Year fixed effects	YES	YES
Observations	5,605	5,030
R-squared	0.451	0.579

Table 6: Effect of Financial slack

This table presents evidence on how the decline in investments following reporting frequency increases depends on availability of financial slack prior to the increase in reporting frequency. We use three different approaches to identify firms with High (Low) financial slack: (i) firms with above (below) median cash balance, (ii) firms that pay (do not pay) common dividends, and (iii) firms with below (above) median value of financing constraints index from Kaplan and Zingales (1997). Coefficient estimates are obtained from a modified version of Eqn. (1) that allows coefficients on all covariates to vary across different levels of financial slack. *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*). Coefficient estimates for all control variables (all defined in the caption of Table 1) have been omitted for brevity. *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.

Slack measured using	Cash				Dividend Payment			
	INVGROSS		*CAPEX*		*INVGROSS*		*CAPEX*	
	(1) High slack	(2) Low Slack	(3) High slack	(4) Low Slack	(5) High slack	(6) Low Slack	(7) High slack	(8) Low Slack
TREAT	0.026***	0.032***	0.016***	0.016***	0.023***	0.031***	0.009**	0.028***
	(3.860)	(4.332)	(3.093)	(3.130)	(4.041)	(2.872)	(2.099)	(3.473)
AFTER	0.022***	-0.009**	0.010***	-0.007**	0.008**	-0.004	0.003	-0.004
	(4.818)	(-2.010)	(2.987)	(-1.968)	(2.117)	(-0.488)	(1.132)	(-0.703)
*TREAT*AFTER*	-0.021***	-0.026***	-0.011*	-0.012**	-0.015***	-0.032***	-0.003	-0.027***
	(-2.751)	(-3.378)	(-1.877)	(-2.085)	(-2.679)	(-2.713)	(-0.703)	(-2.764)
Test of differences in TREAT:								
High – Low	-0.006		-0.000		-0.008		-0.019**	
	(-0.587)		(-0.037)		(-0.651)		(-2.183)	
*Test of differences in TREAT*AFTER:*								
High – Low	0.005		0.000		0.017		0.024**	
	(0.506)		(0.058)		(1.318)		(2.284)	
Controls	YES		YES		YES		YES	
Firm and year fixed effects	YES		YES		YES		YES	
Observations	12,350		10,517		12,337		10,513	
R-squared	0.403		0.527		0.402		0.530	

Table 6 (Continued)

Slack measured using	Kaplan-Zingales Index			
	INVGROSS		*CAPEX*	
	(9)	(10)	(11)	(12)
	High slack	Low Slack	High slack	Low Slack
TREAT	0.016**	0.033***	0.004	0.023***
	(2.519)	(3.617)	(0.961)	(3.450)
AFTER	0.013***	-0.012**	0.005*	-0.007
	(3.025)	(-1.966)	(1.659)	(-1.448)
*TREAT*AFTER*	-0.006	-0.032***	0.001	-0.020***
	(-0.974)	(-3.304)	(0.129)	(-2.652)
Test of differences in TREAT:				
High – Low	-0.017		-0.019**	
	(-1.593)		(-2.462)	
*Test of differences in TREAT*AFTER:*				
High – Low	0.026**		0.021**	
	(2.291)		(2.401)	
Controls	YES		YES	
Firm and year fixed effects	YES		YES	
Observations	10,883		9,481	
R-squared	0.415		0.531	

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) asset turnover computed as sales scaled by lagged assets (*ASSETTURN*), (iii) 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 (iv) 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)	(2)	(3)	(4)
	ROA	*ASSETTURN*	*TFP*	*SALESGROWTH*
TREAT	-0.006	0.010**	-0.019	0.054***
	(-0.999)	(2.566)	(-1.055)	(3.944)
AFTER	-0.012***	-0.011	0.015	0.000
	(-2.874)	(-0.456)	(1.379)	(0.0132)
*TREAT*AFTER*	0.004	-0.069*	-0.003	-0.040***
	(0.625)	(-1.779)	(-0.168)	(-2.841)
Firm fixed effects	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES
Observations	12,314	12,350	10,127	12,350
R-squared	0.568	0.815	0.755	0.215

Table 8: 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. Observations with above (below) median level of timeliness are classified as firms with high (low) timeliness. Coefficient estimates are obtained from a modified version of Eqn. (1) that allows coefficients on covariates to vary across different levels of timeliness. *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*). Coefficient estimates for all control variables (all defined in the caption of Table 1) have been omitted for brevity. *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.

	INVGROSS		*CAPEX*	
	(1) High Timeliness	(2) Low Timeliness	(3) High Timeliness	(4) Low Timeliness
TREAT	0.020***	0.030***	0.004	0.021***
	(2.635)	(4.135)	(0.778)	(3.885)
AFTER	0.007	-0.001	0.002	-0.003
	(1.427)	(-0.269)	(0.639)	(-0.752)
*TREAT*AFTER*	-0.018**	-0.022***	-0.002	-0.017***
	(-2.242)	(-2.923)	(-0.389)	(-2.884)
Test of differences in TREAT:				
High – Low	-0.010		-0.017**	
	(-1.045)		(-2.385)	
*Test of differences in TREAT*AFTER:*				
High - Low	0.004		0.015*	
	(0.331)		(1.934)	
Controls	YES		YES	
Firm fixed effects	YES		YES	
Year fixed effects	YES		YES	
Observations	11,163		9,611	
R-squared	0.410		0.540	