Financial Reporting Frequency and Corporate Innovation

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Abstract

We examine how the regulation of financial reporting frequency affects corporate innovation. We use a difference-in-differences approach based on a sample of treatment firms that experience a change in their reporting frequency and matched industry peers and control firms whose reporting frequency remains unchanged. We find that higher reporting frequency significantly reduces treatment firms' innovation output but find no evidence that the net externality effect on industry peers is statistically significant. Together, our results are consistent with the hypothesis that frequent reporting induces managerial myopia and impedes corporate innovation.

1. Introduction

What drives corporate innovation, which is critical to both a nation's economic growth (Solow 1956, 1957; Romer 1990) and a firm's competitive advantage (Porter 1992)? A fast-growing literature tackles this question, exploring empirical links between corporate innovation and a variety of firm-, industry-, and market-level characteristics (for recent reviews, see He and Tian 2018, forthcoming).

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To stimulate innovation, governments typically implement policies providing protection of intellectual property rights. The economic consequences of these policies have been widely documented (for example, Lerner 2009). In addition, recent research has studied whether and how the incentives to innovate are influenced by public policies not directly targeted at innovation, such as health policy (Finkelstein 2004), labor laws (Acharya, Baghai, and Subramanian 2013, 2014), bankruptcy codes (Acharya and Subramanian 2009; Cerqueiro et al. 2017), and tort laws (Galasso and Luo 2017). In this paper, we examine whether and how the regulation of reporting frequency affects corporate innovation. We study the effects of a change in reporting frequency on both the treatment firms that experience such a change and their industry peers whose reporting frequency remains unchanged, as both effects are relevant from a regulator's perspective (Roychowdhury, Shroff, and Verdi 2019).

Motivating innovation is difficult for most firms. Unlike routine tasks that rely on well-known approaches, corporate innovation entails the exploration of unknown methods that typically have a high probability of failure, involve multistage investment, and take years to generate positive returns (Holmstrom 1989). Therefore, to effectively motivate innovation, managers must be protected from external short-term pressure, and short-term failures must be tolerated (Manso 2011). Yet more frequent financial reporting likely intensifies short-term pressure from capital markets and puts managers in a position in which their failure to meet short-term earnings targets is less tolerated. Therefore, more frequent financial reporting could induce managers to focus on short-term earnings rather than long-term firm value, which would result in less innovation.

This hypothesis is supported by both theoretical work (Gigler et al. 2014) and survey evidence (Graham, Harvey, and Rajgopal 2005) and is closely related to recent research on the relation between reporting frequency and capital expenditures. For example, Kraft, Vashishtha, and Venkatachalam (2018) find that firms listed on the US stock exchanges decrease their capital investment levels following a reporting-frequency increase. However, Nallareddy, Pozen, and Rajgopal (2017) and Kajüter, Klassmann, and Nienhaus (2019) find no such evidence in the United Kingdom and Singapore. Our focus on corporate innovation differentiates our work from those studies in two important ways. First, unlike conventional investments (for example, capital expenditures), which are initially capitalized and only gradually affect earnings via depreciation, corporate innovation is a long-term, risky, and idiosyncratic investment in intangible assets (Holmstrom 1989), and innovation expenditures (that is, research and development [R&D]) can have an immediate one-to-one negative effect on pretax earnings.[1] These features make innovation vulnerable to short-term pressure created by frequent reporting and well suited to testing theories of myopia. Second, we can measure both the quantity and quality of innovation output on the basis of patent infor-

[1] Under current US generally accepted accounting principles (GAAP), almost all research and development (R&D) costs are expensed immediately. Current International Financial Reporting Standards and old US GAAP allow some R&D costs to be capitalized as intangible assets.

mation. Note that the relation between reporting frequency and innovation cannot be readily inferred from the mixed evidence on capital expenditures, as research shows that the same economic factor can have opposite impacts on these two types of investments.[2]

Although the above discussion highlights that increased frequency of financial reporting can hinder corporate innovation, the literature also suggests that more frequent reporting could lead to greater innovation for at least two reasons. More frequent reporting can improve firms' access to financing by lowering their cost of equity (Fu, Kraft, and Zhang 2012). A lower cost of equity helps relax a firm's financial constraints and allows it to invest more in innovation, which requires a significant amount of investment in both tangible and intangible assets. In addition, more frequent reporting could improve monitoring from capital markets and help discipline managers, who may be reluctant to invest in long-term projects. Moral hazard models suggest that managers who are not properly disciplined shirk or invest suboptimally in short-term projects that generate quicker and more certain returns (Grossman and Hart 1988; Harris and Raviv 1988, 1989). Frequent financial reporting exposes managers to more intensive monitoring by a variety of capital market players (such as financial analysts, short sellers, and regulators) and motivates them to invest in long-term, value-enhancing projects.[3] Given these tensions in the literature, the existence, direction, and economic magnitude of the effect of financial reporting frequency on corporate innovation are unresolved empirical questions.

Frequent reporting can also generate significant externalities for peer firms. On one hand, frequent reporting potentially reduces industry-level information asymmetry and helps industry peers identify investment opportunities or reduce agency frictions (for example, Badertscher, Shroff, and White 2013; Shroff, Verdi, and Yu 2014; Shroff, Verdi, and Yost 2017; Arif and De Gorge 2019). This information spillover is likely to have a positive effect on industry peers' innovation. On the other hand, a firm's myopic behavior caused by frequent reporting can create short-term performance pressure on its industry peers and hinder their innovation. Therefore, the net externality effect of frequent reporting is ex ante unclear.

We use the change in financial reporting frequency in the United States as our empirical setting. The US Securities and Exchange Commission (SEC) required annual financial reporting of listed firms in 1934, increased the frequency to semiannual reporting in 1955, and further increased it to quarterly reporting in

[2] For example, research using the same setting of brokerage closures and mergers to identify changes in analysts' coverage documents drastically different effects of such coverage on capital expenditures and corporate innovation. While Derrien and Kecskés (2013) find that more coverage by analysts leads to more capital expenditures (by reducing information asymmetry and the cost of capital), He and Tian (2013) find that it leads to a reduction in innovation (by imposing short-term pressure).

[3] Consistent with this disciplinary role of reporting frequency, results in Balakrishnan and Ertan (2018) indicate that greater reporting frequency is associated with an improvement in the quality of loan portfolios in the banking industry.

1970. We perform two event studies to gauge the overall effect of reporting frequency on a firm's value. If more frequent reporting causes a firm's managers to become more myopic, the value of the firm will fall. Using a 3-day event window around the SEC announcement of mandatory quarterly reporting (September 15, 1969), we find a significant negative effect of 1 percent on the market value for firms that reported semiannually but no significant effect for firms that already reported quarterly.[4] These results suggest that quarterly reporting is net costly for semiannual reporters, which explains why they had not voluntarily reported this way previously.

While the mandate on quarterly reporting has been in effect for almost 5 decades, President Donald Trump recently (August 17, 2018) asked the SEC, via a tweet, to review quarterly reporting and reconsider semiannual reporting for public companies.[5] Using a 3-day event window around Trump's tweet, we find a significant positive effect of .6 percent on the market value of firms in which innovation matters a lot but a relatively weaker effect of .3 percent on the market value for other firms. The significant difference in market reactions between these two types of firms alleviates the concern that the positive market reaction for innovative firms reflects other implications of Trump's tweet (for example, less burdensome disclosure requirements or more business-friendly regulation). Our results suggest that the cost of quarterly reporting (namely, exacerbating managerial myopia) matters more for innovative firms.

The two event studies provide preliminary evidence consistent with our hypothesis that frequent reporting induces managerial myopia and is net costly to innovative firms. To more directly test the link between financial reporting frequency and managerial myopia, we use observable innovation output to gauge the success of long-term investment in innovation, which is typically hard to observe and measure. We construct three innovation-output measures: the number of patent applications a firm files in a year that are eventually granted, the number of non-self-citations the firm's patents receive in subsequent years, and the economic value of patents, based on stock market reactions to patent grants (computed according to the method of Kogan et al. [2017]). These three measures capture patents' quantity, quality, and economic value, respectively.

Our interim reporting-frequency data are from Butler, Kraft, and Weiss (2007) and Fu, Kraft, and Zhang (2012) and span the period 1951–73. This empirical setting has three desirable features. First, there is substantial cross-sectional and time-series variation in firms' reporting frequency over this period. It is impos-

[4] Prior to 1970, many firms already reported more frequently than required by the Securities and Exchange Commission, because of stock exchange listing requirements or pressure. As early as 1923, the New York Stock Exchange required newly listed firms to provide quarterly reports and pressured already listed firms to do the same, and in 1926, it asked all listed firms to commit to quarterly reporting. The American Stock Exchange and other regional exchanges took similar actions in 1962. See more detailed descriptions in Leftwich, Watts, and Zimmerman (1981) and Butler, Kraft, and Weiss (2007).

[5] On August 17, 2018, Trump tweeted, "Stop quarterly reporting & go to a six month system." See Michaels, Rapoport, and Maloney (2018) for details.

sible to study the relation between reporting frequency and innovation using more recent US data because almost all firms have followed the SEC's quarterly reporting requirement since 1970. Second, the SEC mandate affects only a subset of firms at a time, because some firms had already adopted more frequent reporting prior to the mandate because of stock exchange requirements or pressure from investors. This feature allows us to observe plausible counterfactuals: what level of innovation productivity would firms have achieved in the absence of a reporting-frequency change? The counterfactual is based on control firms with similar economic characteristics but that are not themselves subject to the reporting-frequency change. Thus, we can use a difference-in-differences approach to tighten identification. Third, for this early sample period (1951–73), our patent data (which end in 2010) are unlikely to suffer from the usual truncation problems with which the innovation literature has to contend (Lerner and Seru 2017).

As a first step, we provide descriptive evidence on the trends of aggregate innovation in the economy. We plot the ratio of aggregate innovation by public firms to aggregate innovation by other entities and the individual and total trends over the sample period. We observe an overall upward trend for the ratio, which suggests that public firms' contribution to aggregate innovation generally increases over time. However, we observe a temporary decrease of public firms' innovation around 1970 (when the SEC mandate on quarterly reporting took effect). This evidence suggests that the net impact of frequent reporting (aggregating treatment effects and spillover effects on peer firms) on total innovation is negative. Interestingly, this temporary decrease in aggregate innovation by public firms is more than offset by an increase in aggregate innovation by other entities, which leads to an increase in aggregate innovation. We fully acknowledge that these trends, while interesting, can be interpreted only as descriptive. Thus, we turn to firm-level analyses to strengthen empirical identification and provide tighter evidence.

We use a difference-in-differences approach to examine how regulation of financial reporting frequency affects corporate innovation. We designate firms that increase their reporting frequency as treatment firms. We then use propensity-score matching to identify peer firms in the same industry and control firms in other industries (both with similar economic characteristics but whose reporting frequency remains unchanged). The peer group is not subject to increases in reporting frequency but is affected by the externality effect of increased reporting by firms in the treatment group. The control group is affected by neither increases in reporting frequency nor the externality effect.

In the difference-in-differences tests, we examine the effects of increases in reporting frequency on the innovation output of the treatment firms and their industry peers relative to the control firms. We find a significant reduction in innovation output for the treatment firms relative to the control firms. The difference-in-differences estimators show that, compared with control firms, mandatory adopters (firms that increase their reporting frequency because of the SEC's requirement or exchange requirement) experience a decrease of 1.87

patents, 19.58 non-self-citations, and $1.76 million worth of patent value after the mandatory switch. We find similar results for voluntary adopters (firms that increase their reporting frequency because of the demand of investors). These results suggest that frequent reporting induces managerial myopia and hinders innovation for the treatment firms. For the matched industry peers, we find a significant increase in innovation output, but this increase is not statistically different from that of the control firms. Our inferences are unchanged when we perform a difference-in-differences regression analysis with the matched sample or the full sample.

Overall, our evidence suggests that higher reporting frequency imposes short-term pressure on firms' managers and impedes innovation, and we do not find evidence that the net externality effect on industry peers is statistically significant. These results could be of interest to regulators and policy makers in evaluating the costs and benefits of the quarterly reporting mandate.

The rest of the paper is organized as follows. Section 2 discusses the related literature and our contribution. Section 3 presents the event studies to gauge the overall effect of reporting frequency on firms' value. Section 4 provides some descriptive evidence on the trends of aggregate innovation in the economy. Section 5 describes the sample selection, variable measurement, and summary statistics. Section 6 presents the main difference-in-differences results, and Section 7 concludes.

2. Related Literature

Our contributions to the literature are threefold. First, our study adds a new angle to the literature on corporate innovation by identifying an important accounting practice, financial reporting frequency, as a crucial determinant of innovation. Studies have found that managerial incentives of investing in innovation are affected by various firm, industry, and market characteristics, including product market competition (Aghion et al. 2005), private-equity ownership (Lerner, Sorensen, and Strömberg 2011), chief executive officers' overconfidence (Hirshleifer, Low, and Teoh 2012), institutional ownership (Aghion, Van Reenen, and Zingales 2013), financial analysts (He and Tian 2013), laws (Acharya and Subramanian 2009; Achara, Baghai, and Subramanian 2013, 2014; Galasso and Luo 2017), market conditions (Nanda and Rhodes-Kropf 2013), corporate venture capitalists (Chemmanur, Loutskina, and Tian 2014), mergers and acquisitions (Bena and Li 2014), firms' boundaries (Seru 2014), investors' attitudes toward failure (Tian and Wang 2014), banking competition (Cornaggia et al. 2015), bank interventions (Gu, Mao, and Tian 2017), and external financial dependence (Acharya and Xu 2017). While this line of work highlights many determinants of corporate innovation, the role of accounting practices has largely been ignored. Research in accounting typically focuses on the effect of a firm's financial reporting quality on its capital investment (for example, Biddle and Hilary 2006; Biddle, Hilary, and Verdi 2009; Francis and Martin 2010; Bushman, Piotroski, and Smith

2011; Badertscher, Shroff, and White 2013; Balakrishnan, Core, and Verdi 2014; Goodman et al. 2014; Shroff, Verdi, and Yu 2014; Balakrishnan, Watts, and Zuo 2016; García Lara, García Osma, and Penalva 2016; Shroff 2017, forthcoming). A notable exception is Zhong (2018), who documents that transparency enhances firms' innovation in an international setting. We build on the theoretical work in Gigler et al. (2014) and provide empirical evidence that the frequency of financial reporting has a substantial effect on corporate innovation by exacerbating managerial myopia.

Second, our work contributes to the literature on financial reporting frequency. Research on the frequency of financial reporting largely focuses on its effects on firms' information environments, such as the information content of annual reports (McNichols and Manegold 1983), earnings timeliness (Alford et al. 1993; Butler, Kraft, and Weiss 2007), and the cost of equity (Fu, Kraft, and Zhang 2012; Verdi 2012). Recent studies begin to examine the effects of frequent financial reporting on managerial decisions, such as investments in fixed assets (Nallareddy, Pozen, and Rajgopal 2017; Kraft, Vashishtha, and Venkatachalam 2018; Kajüter, Klassmann, and Nienhaus 2019), real activities manipulations (Ernstberger et al. 2017), cash holdings (Downar, Ernstberger, and Link 2018), and banks' loan portfolio quality (Balakrishnan and Ertan 2018). Given the mixed evidence in the literature, Roychowdhury, Shroff, and Verdi (2019) conclude that whether an increase in reporting frequency decreases managers' investment horizon and induces myopia or whether it increases transparency and serves a disciplinary role remains an open question. Our study sheds light on this important question by focusing on a firm's innovation, which is critical to a country's competitive advantages and long-term growth.

Moreover, we provide richer evidence on the economic consequences of reporting frequency in several ways. First, we conduct an event study to show a negative market reaction to the quarterly reporting mandate, which is consistent with firms incurring a firm-specific net cost and explains why firms do not voluntarily increase their reporting frequency. Second, an unexpected recent event—that is, Trump's tweet—gives us the opportunity to demonstrate that the cost of quarterly reporting (namely, exacerbating managerial myopia) outweighs its benefit (namely, lowering the cost of equity), especially for innovative firms. Third, we complement our firm-level analyses with descriptive evidence on the trends of aggregate innovation in the economy. Fourth, we conduct separate analyses for matched industry peers and assess on the externality of mandatory quarterly reporting. Understanding this spillover effect is important since one of the primary justifications for mandatory disclosure is externalities (for a thorough review, see Minnis and Shroff 2017).

Finally, our finding that more frequent reporting impedes corporate innovation is of interest to regulators and industry groups, who recently debated whether firms should be required to undertake more frequent interim financial reporting (for example, Day 2003; Directive 2004/109/EC, On the Harmonisation of Transparency Requirements, O.J. (L. 390) 38–57; Jopson 2006; Yiu 2009;

Davidoff Solomon 2011; Yahya 2016). The SEC is considering the pros and cons of replacing quarterly with semiannual reporting (especially for smaller reporting companies; Higgins [2016]). The United Kingdom started requiring firms to provide quarterly interim management statements in 2007 but ended the requirement in 2014. To the extent that firms today face greater short-term pressure than in the past (Hersh 2016; Dimon and Buffett 2018; Stoll 2018), our results represent a lower-bound estimate of the impact of frequent financial reporting on corporate innovation.[6]

3. Event Studies

We argue that more frequent reporting causes a firm's managers to become more myopic. If our argument is true, we would expect a drop in the value of a firm when it is required to report more frequently. Furthermore, we expect the effect to be more pronounced for firms in which innovation plays an important role. We use event studies to test our expectations. The first event is the SEC's announcement of the quarterly reporting requirement on September 15, 1969. Our related results are reported in Table 1.

We gauge the market's reaction via CAR [0, 2], which is the cumulative abnormal return over the 3-day window of [0, 2], with 0 being the event date. Its mean value is negative and significant for semiannual reporters (firms that reported semiannually before the announcement), while it is statistically insignificant for quarterly reporters (firms that reported quarterly prior to the announcement). The difference between the two types of firms is significant at the 5 percent level. This negative market reaction to the quarterly reporting mandate is consistent with firms incurring a firm-specific net cost and explains why firms might not have chosen to voluntarily increase reporting frequency.

The second event is Trump's announcement on Twitter on August 17, 2018, which raised the possibility of dropping the quarterly reporting requirement. Table 2 reports our results. We find that CAR [0, 2] is positive and significant for innovative firms (firms that filed patents between 2005 and 2014),[7] while it is weaker for noninnovative firms (firms that did not file patents between 2005 and 2014). The difference between the two types of firms is significant at the 5 percent level. Under the assumption that firms that file patents are those for which innovation matters, our results support the conjecture that the negative-valuation impact of quarterly reporting is more severe for firms for which innovation plays an important role. Together, these event studies provide preliminary evidence consistent with our hypothesis that frequent reporting induces managerial myopia and is net costly to innovative firms.

[6] Given that the R&D expensing rules are different outside the United States, assessing the generalizability of our findings in an international setting is an interesting avenue for future research.

[7] Our patent data (collected from the US Patent Office) end in 2014. To gauge a firm's innovativeness, we look at the most recent 10 years of a firm's patent-filing history, namely, 2005–14.

Table 1

Event Study: The Securities and Exchange Commission Announcement

	Semiannual Reporters	Quarterly Reporters	Difference
Mean CAR [0, 2]	−.010+	.002	−.012*
	(−1.75)	(1.21)	(−2.23)
N	80	908	

Note. Values are the 3-day cumulative abnormal stock returns (CAR) around the Securities and Exchange Commission (SEC) announcement of its quarterly reporting requirement for SEC-listed firms, with *t*-statistics in parentheses. The sample includes all firms with nonmissing stock returns and reporting frequency.
+ $p < .10$.
* $p < .05$.

Table 2

Event Study: The President's Tweet

	Innovative Firms	Noninnovative Firms	Difference
Mean CAR [0, 2]	.006**	.003**	.003*
	(5.46)	(8.01)	(2.29)
N	1,023	6,397	

Note. Values are the 3-day cumulative abnormal stock returns around the president's tweet about dropping the quarterly reporting requirement, with *t*-statistics in parentheses. The sample includes all listed firms with nonmissing stock returns on the announcement date.
* $p < .05$.
** $p < .01$.

4. Descriptive Evidence on Aggregate Trends

We hypothesize that high reporting frequency curbs innovation. As a first step, we provide descriptive evidence on the trends of aggregate innovation in the economy in Figure 1. The total curve represents all patents filed in the year divided by the patents filed in 1973. From 1951 to 1965, the aggregate innovation in the United States increased steadily, reflecting the post–World War II prosperity and productivity gains. From 1965 to 1969, it showed a declining trend. From 1969 to 1973, it increased again. Aggregate innovation is likely affected by geopolitics, macroeconomic conditions, and technological advances in addition to reporting frequency.

Figure 1 also reports the proportion of patents generated by publicly listed firms and other entities, while the relative curve indicates the number of patents filed by publicly listed firms divided by the patents filed by other entities.[8] We find that, relative to other entities, the number of patents attributable to publicly listed firms increased between 1951 and 1968, decreased between 1968 and 1971,

[8] Other entities include private firms, universities, governments, and even individuals. Most of the patents are filed by firms.

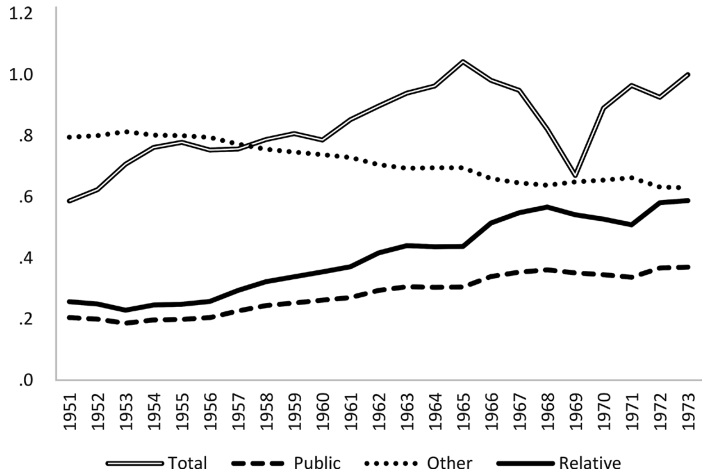

Figure 1. Trends in aggregate innovation

and increased from 1971 to 1973. The decrease between 1968 and 1971 is consistent with the conjecture that the quarterly-reporting requirement dampens the innovation of publicly listed firms (aggregating both treatment effects and spillover effects). We, however, acknowledge that these trends, while interesting, can be interpreted only as being descriptive.

5. Sample Selection, Variable Measurement, and Descriptive Statistics

5.1. Sample Selection

To strengthen empirical identification and provide tighter evidence, we turn to firm-level analyses. Our sample is drawn from Butler, Kraft, and Weiss (2007) and Fu, Kraft, and Zhang (2012), for which the data were hand collected from *Moody's Industrial News Reports* covering the 1951–73 period.[9] Reporting frequency is defined as 1 for annual reporters, 2 for semiannual reporters, 3 for firms reporting three times a year, and 4 for quarterly reporters. The following firms are excluded: firms not listed on either the New York Stock Exchange (NYSE) or the American Stock Exchange (AMEX), firms lacking Center for Research in Security Prices or Compustat data, and firms in industries with distinctive disclosure requirements (for example, utilities; financial services, insurance, and real estate firms; and railroad and other transportation companies). We merge this data set with innovation data (see Kogan et al. [2017] for a detailed description of the data).[10] Following the innovation literature (for example, He and Tian 2013), we

[9] See Butler, Kraft, and Weiss (2007) and Fu, Kraft, and Zhang (2012) for more details on the data sources and composition of the original reporting-frequency samples.

[10] To download the innovation data, see Indiana University, Patent Data (http://iu.box.com/patents).

set the innovation proxies to 0 for firms without available patent or citation information. Our results are quantitatively similar if we drop the observations with missing innovation proxies. Our sample consists of 9,904 firm-year observations from 1951 to 1973.

5.2. Innovation Measures and Control Variables

We construct three measures to capture a firm's innovation output. The first is the number of patent applications a firm files in a year that are eventually granted (PAT). We use a patent's application year, instead of its grant year, because the application year arguably better captures the timing of innovation (Griliches, Pakes, and Hall 1988). A limitation of this measure is that it does not distinguish major innovations from marginal advances. To further gauge a patent's impact, we employ two other measures of corporate innovation output: the number of non-self-citations the firm's patents receive in subsequent years (TCITE) and the economic value of patents (TSM) based on stock market reactions to patent grants. The difference between these two measures is that the former mainly captures scientific impact, while the latter represents market value to a firm's shareholders. Our data for these innovation measures end in 2010. Since our sample period ends long before 2010, our patent variables are unlikely to suffer from the typical truncation problems the innovation literature must address.

Control variables include firm size, LNMV, measured by the natural logarithm of the firm's market capitalization; investment in innovation, RD, measured by R&D expenditures scaled by total assets;[11] profitability, ROA, measured by return on assets; asset tangibility, PPE, measured by net property, plant, and equipment scaled by total assets; leverage, LEV, measured by the ratio of total debt to total assets; investment in fixed assets, CAPEX, measured by capital expenditures scaled by total assets; product market competition, HERF, measured by the Herfindahl-Hirschman index based on annual sales;[12] growth opportunities, *Q*, measured by Tobin's *q*; financial constraints, HPINDEX, a measure based on the firm's size and age that is developed by Hadlock and Pierce (2010);[13] the firm's age, LNAGE, measured by the natural logarithm of 1 plus the number of years the firm is listed on Compustat; and stock illiquidity, AMIHUD, measured by the yearly median of the Amihud (2002) price-impact measure (daily absolute stock return divided by the trading volume measured in thousands of dollars).

[11] Our results are largely unchanged when we control for cumulative R&D expenditures over the current year and the previous 1, 2, or 3 years.

[12] We also include in our regressions the squared Herfindahl-Hirschman index, HERF_SQR, to account for the nonlinear effect of product market competition (Aghion et al. 2005).

[13] We do not use the more current measures of financial constraint based on textual analyses of 10-K filings (for example, Hoberg and Maksimovic 2015; Buehlmaier and Whited 2018), because such measures are not available for our sample period.

Table 3
Distribution of the Sample by Reporting Frequency, 1951–73

	N	Annual	Semiannual	Triannual	Quarterly	Total
1951–54	501	7.78	22.55	2.99	66.67	5.06
1955–69	5,929	1.08	10.47	1.84	86.61	59.86
1970–73	3,474	.55	1.64	1.21	96.60	35.08
Total	9,904	1.23	7.99	1.68	89.11	100.00
Firms	1,117	58	189	128	1,089	

Table 4
Descriptive Statistics

	Mean	SD	Quarter 1	Median	Quarter 3
PAT	6.464	15.483	.000	.000	4.000
TCITE	46.262	108.416	.000	.000	28.000
TSM	4.319	14.053	.000	.000	.821
LNMV	3.830	1.641	2.623	3.697	4.972
RD	.006	.015	.000	.000	.000
ROA	.148	.084	.100	.144	.195
PPE	.319	.170	.198	.294	.413
LEV	.216	.157	.088	.205	.319
CAPEX	.063	.053	.026	.052	.085
HERF	.483	.313	.225	.385	.727
Q	1.703	1.042	1.055	1.422	1.967
HPINDEX	−2.365	.693	−2.887	−2.430	−1.943
LNAGE	1.458	.955	.693	1.609	2.197
AMIHUD	.015	.032	.000	.004	.014

Note. $N = 9{,}904$ firm-years for 1951–73.

5.3. Descriptive Statistics

Table 3 reports the distribution of sample firms by reporting regime. During 1951–54, only annual reporting was required. In 1955–69, semiannual reporting began to be compulsory. In 1970–73, most of our sample firms reported quarterly. In our sample, a few firms report three times a year because they may decide to switch from semiannual to quarterly reporting in the middle of the fiscal year. Our sample period provides both cross-sectional and time-series variation in reporting frequency and provides an ideal setting for our investigation. Our full sample consists of 1,117 firms and 9,904 firm-year observations.[14] Table 4 provides descriptive statistics for the full sample.

6. Difference-in-Differences Analyses

6.1. Treatment, Peer, and Control Groups

We argue that financial reporting frequency affects economy-wide innovation in various ways: First, frequent reporting induces managerial myopia and hinders

[14] Because some of our sample firms switched their reporting frequency during the sample period, the number of firms in our full sample does not equal the sum of firms across reporting frequencies.

innovation for reporting firms. Second, it improves firms' access to financing and monitoring from capital markets and thus enhances their innovation. Third, it potentially reduces industry-level information asymmetry, and this information spillover is likely to have a positive effect on industry peers' innovation. Fourth, a firm's myopic behavior caused by frequent reporting can create short-term performance pressure on its industry peers and hinder their innovation. To empirically assess the treatment and externality effects of frequent reporting on innovation, we divide the full sample into three groups: the treatment group (treated by increases in reporting frequency and possibly also by the externality effect from other firms in this group), the peer group (not subject to increases in reporting frequency but affected by the externality effect of increased reporting by firms in the treatment group), and the control group (affected by neither increases in reporting frequency nor the externality effect).

We construct the three groups of firms using propensity-score matching. We first use the full sample to run an ordered probit model to estimate the propensity score related to the change in reporting frequency (see Table A1). We then use the predicted probabilities, or propensity scores, to perform nearest-neighbor propensity-score matching to identify peer and control firms. By construction, peer firms and control firms have characteristics similar to treatment firms, but their reporting frequency remains unchanged. We require peer firms to be in the same industry (on the basis of the Fama-French 48 industries) as treatment firms because externalities are most likely to occur among industry peers. Control firms are from other industries.

Table 5 reports the distributions of treatment firms according to the change in reporting frequency. Our findings are largely consistent with those of Kraft, Vashishtha, and Venkatachalam (2018).[15] Table 6 presents the distribution of treatment firms according to the reason for the change in reporting frequency. Firms may switch their reporting frequency because of the SEC's regulation, the stock exchange's requirements, or demand from investors. We conclude that the switch is due to the SEC's regulation if the firm increased the frequency to the semiannual level starting in 1955 or to the quarterly level after 1967. The switch is deemed a result of an exchange requirement if the firm is listed on AMEX and increased its frequency to the quarterly level starting 1 year before and up to 2 years after 1962 (the year in which AMEX started urging existing firms and requiring newly listed firms to switch to quarterly reporting). During our sample period, there was no change in the NYSE's listing rules regarding reporting frequency. We assume that if firms are not required by the SEC or the stock exchange to switch their reporting frequency, the switches are due to demands from investors.

Our matched sample includes firm-year observations for the three groups of firms over a 6-year window centered on the year of the change in reporting frequency (Fang, Tian, and Tice 2014). Of the 1,075 treatment firms with reporting-

[15] There are very few cases in which firms temporarily decrease their reporting frequency. We find few effects of these temporary reporting changes on firms' innovation output.

Table 5
Time-Series Distribution of the Sample by Increase in Reporting Frequency

	To Semiannual	To Triannual	To Quarterly	Total
1951–54	57	20	89	166
1955–69	252	157	426	835
1970–73	9	13	52	74
Total	318	190	567	1,075
Firms with nonzero patents	132	85	274	491

Table 6
Distribution of the Sample by Reason for Switching

	Securities and Exchange Commission Regulation	Stock Exchange Requirement	Investors' Demand	Total
1951–54	0	0	166	166
1955–69	305	133	397	835
1970–73	61	0	13	74
Total	366	133	576	1,075
Firms with nonzero patents	144	51	296	491

frequency increases, 491 firms engage in some patenting activities during the sample period.[16]

Following Kraft, Vashishtha, and Venkatachalam (2018), we classify firms that increase their reporting frequency owing to the SEC's requirement or exchange requirement as mandatory increasers and all others as voluntary increasers. In total, 499 firms experience a mandatory increase in reporting frequency, and 576 firms experience a voluntary increase in reporting frequency. The existence of both mandatory and voluntary adopters suggests that the cost-benefit trade-off varies across firms. Firms voluntarily adopt more frequent reporting when the benefit of doing so (such as a lower cost of equity) outweighs the cost (such as reduced innovation), while the opposite is likely true for mandatory adopters.

We show in Figure 2 the values of the three innovation measures for the 6 years surrounding the mandatory increase in reporting frequency for the three groups of firms. Year 0 (omitted from Figure 2) is the year of the switch. The values of the innovation proxies are adjusted by sample averages in each year and standardized to range between 0 and 1.

Figure 2 shows that the two lines representing innovation output for the treatment group and control group trend closely in parallel in the 3 years leading up to the mandatory increase in reporting frequency. After the increase, the two lines start to diverge: innovation output increases slightly for the control firms, and it drops substantially for the treatment firms. The two lines representing in-

[16] For expositional simplicity, the number of treatment firms here refers to the number of unique treatments (not the number of unique firms).

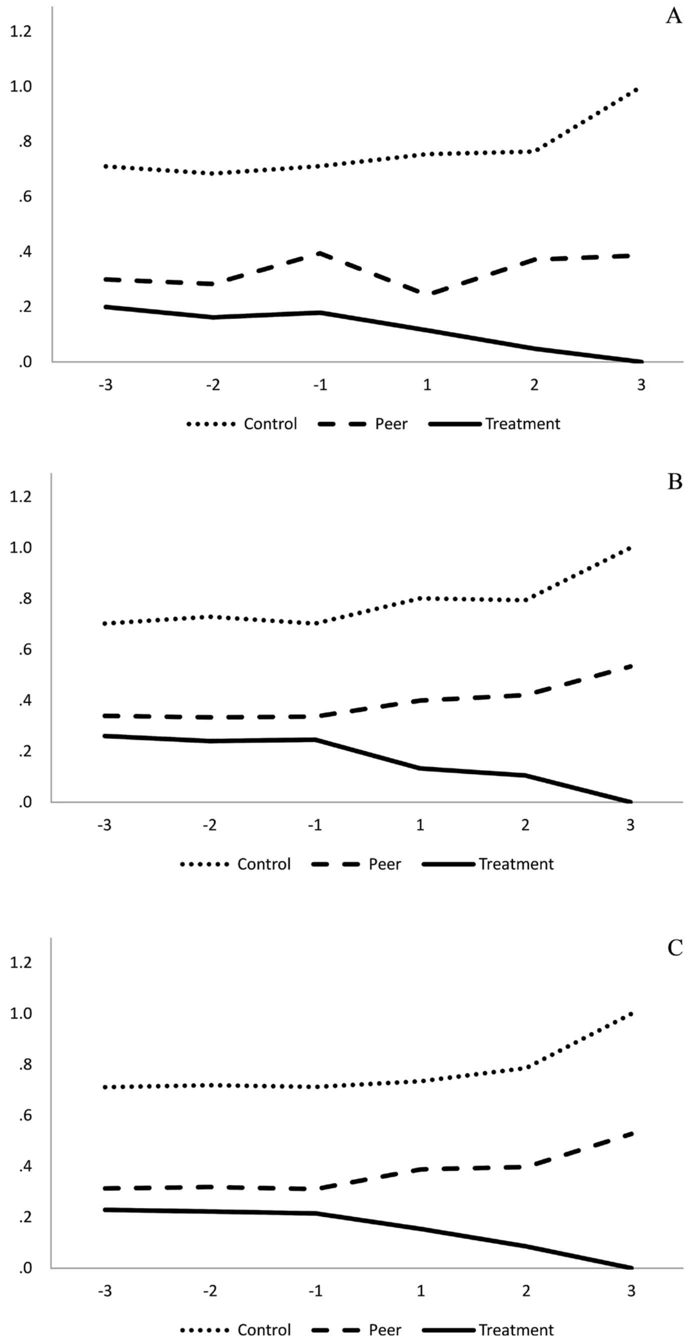

Figure 2. Trends in innovation proxies around the mandatory increase in reporting frequency (*A*) for all patents, (*B*) for non-self-citations, and (*C*) by the economic value of patents.

novation output for the peer group and control group trend closely in parallel over the 6-year window. If we use peer firms as the benchmark, we reach the same conclusion that increases in reporting frequency reduce innovation output of treatment firms. Figure 2 offers visual evidence in support of the parallel-trend assumption underlying the difference-in-differences analysis. It also shows that a mandatory increase in reporting frequency results in a lower level of innovation output for the treatment group, but the net externality effect on the peer group seems limited.

6.2. *Simple Difference-in-Differences Tests*

We use a difference-in-differences approach and compare the innovation output of treatment firms or their industry peers with that of comparable control firms. The difference-in-differences approach has three key advantages. First, it alleviates the concern that the time-series trend, rather than a change in reporting frequency, drives the change in innovation output. Second, we can conduct tests for firms that change their reporting frequency as a result of the SEC or stock exchange mandate (rather than a firm's choice).[17] Finally, the difference-in-differences approach controls for unobserved constant differences between the treatment (or peer) group and the control group.

Using the matched sample, we first conduct univariate tests to obtain the difference-in-differences estimators. We adjust the innovation proxies by the average values for each year to remove aggregate time trends. Table 7 presents the results. We separately examine mandatory and voluntary changes, because mandatory changes in reporting frequency are unlikely driven by an individual firm's choice and hence provide better identification. We report the average change in the number of patents (PAT), the average change in the number of non-self-citations (TCITE), and the average change in the economic value of patents (TSM). These measures are computed by first subtracting these values over the 3 years preceding the switch in reporting frequency from the corresponding values over the 3 years following the switch in reporting frequency for each treatment, peer, or control group. The differences are then averaged over the respective group. We then report the mean difference-in-differences estimators and the corresponding two-tailed *t*-statistics, testing the null hypothesis that the estimators are zero.

For mandatory increases in reporting frequency, we find that treatment firms experience a significant decrease in innovation output, consistent with our hypothesis that more frequent reporting leads to less corporate innovation; by contrast, the peer firms experience a significant increase in innovation output, and there is no significant change for control firms. The difference-in-differences estimators suggest that, on average, a mandatory increase in reporting frequency results in a decrease of 1.87 patents, 19.58 non-self-citations, and $1.76 million in

[17] A caveat is that our sample does not include firms that chose to delist in the presence of additional disclosure mandates (for example, Bushee and Leuz 2005).

Table 7

Simple Difference-in-Differences Test for Increases in Reporting Frequency

	PAT	TCITE	TSM
Mandatory increases ($N = 499$):			
Treatment difference (After − Before)	−1.071**	−12.404**	−1.020**
	(−10.71)	(−22.59)	(−20.46)
Peer difference (After − Before)	.137	8.935**	.981**
	(.48)	(3.33)	(3.66)
Control difference (After − Before)	.794	7.179	.739
	(1.65)	(.96)	(1.54)
Difference-in-differences estimator:			
ΔTreatment − ΔControl	−1.865**	−19.583*	−1.758**
	(−3.79)	(−2.61)	(−3.64)
ΔPeer − ΔControl	−.657	1.756	.242
	(−1.18)	(.22)	(.40)
ΔTreatment − ΔPeer	−1.208**	−21.339**	−2.000**
	(−4.01)	(−7.78)	(−7.33)
Voluntary increases ($N = 576$):			
Treatment difference (After − Before)	−.738**	−5.688**	−.809**
	(−4.08)	(−7.27)	(−7.64)
Peer difference (After − Before)	1.260**	11.159**	1.207*
	(5.00)	(4.74)	(5.33)
Control difference (After − Before)	1.417+	6.561	1.444+
	(1.88)	(1.31)	(1.77)
Difference-in-differences estimator:			
ΔTreatment − ΔControl	−2.155**	−12.249*	−2.253**
	(−2.78)	(−2.42)	(−2.74)
ΔPeer − ΔControl	−.157	4.598	−.237
	(−.20)	(.89)	(−.28)
ΔTreatment − ΔPeer	−1.998**	−16.848**	−2.016**
	(−6.44)	(−6.79)	(−8.06)

Note. Results are mean differences in innovation outcomes from univariate difference-in-differences tests, with t-statistics in parentheses.

+ $p < .10$.
* $p < .05$.
** $p < .01$.

economic value for the treatment firms relative to the control firms. We find similar treatment effects when using peer firms as the benchmark group.

We also assess whether increases in reporting frequency affect peer firms in the industry. Externalities influence peer firms in two ways. On one hand, a mandatory increase in reporting frequency of treatment firms reduces industry-level information asymmetry and thereby encourages innovation by peer firms. On the other hand, it elevates the short-termism of treatment firms and, through peer pressure, imposes similar changes on peer firms, which results in less innovation. Results for the difference between peer and control firms show that, on average, the net effect of externalities is statistically insignificant. This insignificant result should be interpreted with caution as it may reflect that our tests on externalities lack statistical power, and our setting is not conducive for detecting externality effects. To further examine the economic significance (or size) of the externality effect for peer firms relative to control firms, we construct the 95 percent confidence intervals on the basis of those estimates, which are [−1.76, .45] for patents, [−14.06, 17.57] for non-self-citations, and [−.95, 1.43] for the economic value. Thus, the largest externality effect that can be ruled out at the 95 percent level appears to be relatively big and comparable to the average treatment effects estimated for the difference between treatment and control firms, though we observe both large positive and negative externalities.

For voluntary increases in reporting frequency, we find that the innovation output of the treatment firms decreases after the switches. We find all three innovation measures increase significantly after the switch for peer firms and, to a lesser extent, for control firms. The magnitude of the difference-in-differences estimators suggests that, on average, a voluntary increase in reporting frequency results in a decrease of 2.15 patents, 12.25 non-self-citations, and $2.25 million in economic value of patents compared with the control firms. Our results are similar when we use peer firms as the benchmark. The net effect of externality is statistically insignificant.[18]

6.3. *Difference-in-Differences Regression Analyses*

In this section, we use the matched sample to conduct difference-in-differences regression analyses to obtain our main results. Specifically, following Fang, Tian, and Tice (2014), we use firm-year observations for treatment, peer, and control firms over a 6-year window centered on the year of the switch in the reporting frequency and estimate the following model:

$$\begin{aligned}\text{INNOV} = \alpha &+ \beta_1 \text{Treat} \times \text{Before}^2 + \beta_2 \text{Treat} \times \text{Before}^1 + \beta_3 \text{Treat} \times \text{After}^1 \\ &+ \beta_4 \text{Treat} \times \text{After}^2 + \beta_5 \text{Treat} \times \text{After}^3 + \beta_6 \text{Peer} \times \text{Before}^2 \\ &+ \beta_7 \text{Peer} \times \text{Before}^1 + \beta_8 \text{Peer} \times \text{After}^1 + \beta_9 \text{Peer} \times \text{After}^2 \\ &+ \beta_{10} \text{Peer} \times \text{After}^3 + \beta_{11} \text{Before}^2 + \beta_{12} \text{Before}^1 \\ &+ \beta_{13} \text{After}^1 + \beta_{14} \text{After}^2 + \beta_{15} \text{After}^3 \\ &+ \text{Firm Fixed Effects} + \text{Year Fixed Effects} + \varepsilon.\end{aligned} \tag{1}$$

The dependent variable is one of the three innovation-output measures (PAT, TCITE, and TSM). The dummy variable Treat equals one for treatment firms and zero otherwise; Peer is a dummy variable that equals one for industry peers and zero otherwise; Before^2 is a dummy variable that equals one if a firm-year observation is from the second year before the switch in reporting frequency and zero otherwise; Before^1 is a dummy variable that equals one if a firm-year observation

[18] The largest externality effect that can be ruled out at the 95 percent level still appears to be relatively big for these voluntary switches. The 95 percent confidence intervals based on the estimates for the difference between peer and control firms are [−1.73, 1.42] for patents, [−5.59, 14.79] for non-self-citations, and [−1.91, 1.43] for the economic value.

is from the year before the switch in reporting frequency and zero otherwise; After[1] is a dummy variable that equals one if a firm-year observation is from the year immediately after the reporting-frequency switch and zero otherwise; After[2] is a dummy variable that equals one if a firm-year observation is from the second year after the switch and zero otherwise; After[3] is a dummy variable that equals one if a firm-year observation is from the third year after the switch and zero otherwise. We also include the firm and year fixed effects.

The key coefficient estimates are β_1–β_{10}. A statistically insignificant coefficient estimate of β_1, β_2, β_6, or β_7 suggests that the parallel-trend assumption is not violated. Negative and significant coefficient estimates of β_3, β_4, or β_5 suggest that, compared with control firms, treatment firms generate a smaller number of patents, patents with fewer citations, and patents with smaller economic value in the years following the reporting-frequency change. Significant coefficient estimates of β_8, β_9, or β_{10} suggest that reporting-frequency increases generate a statistically significant externality on industry peers (relative to control firms).

We report the regression results from estimating equation (1) in Table 8. For mandatory increases in reporting frequency, β_1, β_2, β_6, and β_7 are all statistically insignificant, which suggests that the parallel-trend assumption is not violated. In eight of nine specifications, β_3, β_4, and β_5 are negative and significant, consistent with our hypothesis that more frequent reporting leads to less corporate innovation for the treatment firms, and β_8, β_9, and β_{10} are all statistically insignificant.

Our results for voluntary increases in reporting frequency are similar. For all three dependent variables, β_1, β_2, β_6, and β_7 are statistically insignificant, which suggests that the parallel-trend assumption is not violated; β_3, β_4, and β_5 are negative and significant in eight of nine specifications, which suggests that compared with control firms, treatment firms experience a drop in innovation output; β_8, β_9, and β_{10} are again all statistically insignificant. Overall, these findings are consistent with our univariate difference-in-differences estimator findings and suggest that increases in reporting frequency lead to drops in innovation output.

6.4. Full-Sample Analysis

Following prior research (for example, Fu, Kraft, and Zhang 2012; Fang, Tian, and Tice 2014; Kraft, Vashishtha, and Venkatachalam 2018), our previous analyses are based on matched samples over a 6-year window centered on the year of the switch in the reporting frequency. An advantage of this approach is that it allows us to identify the three groups of firms (treatment, peer, and control firms) around the relatively short event window and study both firm-level effects and spillover effects on industry peers. Separating the sample into the three groups over the full sample period (1951–73) is not feasible, as most industries included in our sample are treated over that period.[19] In this section, we use the full sample and a generalized difference-in-differences estimator that exploits the staggered

[19] An industry is treated when at least one firm in the industry switches reporting frequency.

Table 8

Difference-in-Differences Regression Analyses for Increases in Reporting Frequency

	PAT	TCITE	TSM
Mandatory increases ($N = 4{,}832$):			
Treat × Before^2	−.181	−4.927	−.190
	(−.31)	(−1.17)	(−.95)
Treat × Before^1	−.261	−1.893	−.156
	(−.34)	(−.49)	(−.34)
Treat × After^1	−1.315*	−18.857*	−.863*
	(−2.12)	(−1.97)	(−2.18)
Treat × After^2	−1.939+	−18.935	−1.701*
	(−1.82)	(−1.27)	(−2.13)
Treat × After^3	−3.554**	−33.455+	−3.262*
	(−2.81)	(−1.71)	(−2.19)
Peer × Before^2	.079	−3.540	−.058
	(.13)	(−.92)	(−.26)
Peer × Before^1	.824	−.842	−.040
	(1.01)	(−.20)	(−.09)
Peer × After^1	−.862	−3.260	.401
	(−1.34)	(−.40)	(.99)
Peer × After^2	.293	.297	.143
	(.28)	(.02)	(.19)
Peer × After^3	−.953	.387	.065
	(−.88)	(.02)	(.05)
Before^2	.076	5.859	.293
	(.13)	(1.41)	(1.54)
Before^1	1.141	4.277	.409
	(1.65)	(1.04)	(.83)
After^1	1.225+	21.307*	.894*
	(1.92)	(2.16)	(2.00)
After^2	2.045+	20.273	1.715*
	(1.96)	(1.32)	(2.14)
After^3	2.995*	30.554	3.162*
	(2.47)	(1.58)	(2.17)
Adjusted R^2	.725	.741	.748
Voluntary increases ($N = 5{,}668$):			
Treat × Before^2	.351	−9.197	−.220
	(.78)	(−1.50)	(−.73)
Treat × Before^1	−.047	−7.088	−.387
	(−.09)	(−1.19)	(−.89)
Treat × After^1	−2.375+	−16.084*	−2.178+
	(−1.94)	(−1.97)	(−1.90)
Treat × After^2	−2.835*	−18.906	−2.796+
	(−2.03)	(−1.43)	(−1.91)
Treat × After^3	−2.518*	−23.967+	−3.503*
	(−2.18)	(−1.88)	(−2.24)
Peer × Before^2	−.341	−9.496	−.356
	(−.88)	(−1.56)	(−1.22)
Peer × Before^1	−.087	−5.943	−.437
	(−.17)	(−.99)	(−1.00)

Table 8 (*Continued*)

	PAT	TCITE	TSM
Peer × After[1]	−1.375	.468	−1.396
	(−1.13)	(.05)	(−1.22)
Peer × After[2]	−.140	−1.010	−.648
	(−.10)	(−.08)	(−.44)
Peer × After[3]	−.061	.840	.104
	(−.05)	(.07)	(.06)
Before[2]	.358	9.825+	.365
	(.95)	(1.69)	(1.32)
Before[1]	.810*	8.159	.656+
	(1.96)	(1.41)	(1.73)
After[1]	2.256+	16.893*	1.817+
	(1.90)	(2.34)	(1.70)
After[2]	2.715*	19.890	2.464+
	(2.04)	(1.64)	(1.85)
After[3]	2.351*	24.666*	3.041*
	(2.22)	(2.15)	(2.16)
Adjusted R^2	.580	.633	.453

Note. Results are estimates of the innovation dynamics of treatment and control firms 3 years before and after the change in reporting frequency. Firm and year fixed effects are included in all regressions. The two-tailed test t-statistics in parentheses are based on standard errors clustered by firm.
$^{+}p < .10$.
$^{*}p < .05$.
$^{**}p < .01$.

nature of the treatment effects as a robustness check. We use firm-year observations for the full sample and estimate the following model:

$$\begin{aligned}\text{INNOV} = \alpha &+ \beta_1 \text{Quarterly} \times \text{Post\_Q} + \beta_2 \text{Semiannual} \times \text{Post\_S} + \text{Controls} \\ &+ \text{Firm Fixed Effects} + \text{Year Fixed Effects} + \varepsilon. \end{aligned} \quad (2)$$

The dependent variable is one of the three innovation-output measures: PAT, TCITE, and TSM. The dummy variable Quarterly equals one for treatment firms that increase reporting frequency to the quarterly level and zero otherwise; Semiannual is a dummy variable that equals one for treatment firms that increase reporting frequency to the semiannual level and zero otherwise; Post_Q is a dummy variable that equals one if a firm-year observation is from a year after the reporting-frequency switch to the quarterly level and zero otherwise; Post_S is a dummy variable that equals one if a firm-year observation is from a year after the reporting-frequency switch to the semiannual level and zero otherwise. We include the standard set of control variables as in Fang, Tian, and Tice (2014) and firm and year fixed effects.

The key coefficient estimates are β_1 and β_2. A negative and significant coefficient estimate of β_1 (or β_2) suggests that, compared with control firms, treatment firms generate a smaller number of patents, patents with fewer citations, and pat-

Table 9
Difference-in-Differences Analyses for the Full Sample

	PAT	TCITE	TSM
Quarterly × Post_Q	−5.438**	−25.541**	−5.151**
	(−6.35)	(−5.31)	(−5.24)
Semiannual × Post_S	−2.187	−7.815	−1.194
	(−1.40)	(−.86)	(−1.31)
LNMV	1.979**	18.465**	3.675**
	(3.66)	(4.85)	(5.06)
RD	156.208**	1,033.758**	154.557**
	(4.84)	(5.59)	(4.21)
ROA	−5.268+	−40.125*	−8.815*
	(−1.77)	(−2.17)	(−2.37)
PPE	2.234	15.626	4.506
	(.85)	(.84)	(1.29)
LEV	1.300	6.841	.017
	(.72)	(.62)	(.01)
CAPEX	−13.560**	−79.925**	−16.439**
	(−4.05)	(−3.44)	(−3.48)
HERF	.289	2.377	.611
	(.04)	(.05)	(.08)
HERF_SQR	.604	6.607	1.566
	(.11)	(.18)	(.25)
Q	−.709*	−3.369+	−.894*
	(−2.18)	(−1.77)	(−1.98)
HPINDEX	.693*	1.244	1.273**
	(1.93)	(.50)	(2.71)
LNAGE	−2.289**	−11.413**	−3.490**
	(−3.16)	(−2.59)	(−4.30)
AMIHUD	7.851**	40.680**	12.730**
	(4.08)	(3.22)	(5.66)
Adjusted R^2	.729	.761	.643

Note. Results are estimates of the impact of reporting frequency on innovation. Firm and year fixed effects are included in all regressions. The two-tailed test t-statistics in parentheses are based on standard errors clustered by firm. $N = 9{,}904$.
$^{+}p < .10$.
$^{*}p < .05$.
$^{**}p < .01$.

ents with smaller economic value in the years following the reporting-frequency change to the quarterly (or semiannual) level.

We report the regression results from estimating equation (2) in Table 9. The results for β_1 related to the dependent variables are negative and significant, consistent with our hypothesis that more frequent reporting leads to less corporate innovation for the treatment firms. The negative but statistically insignificant results for β_2 suggest that switching from annual reporting to semiannual reporting is not particularly costly to treatment firms. But this result should be interpreted with caution given the limited number of treatment firms that switched to semiannual reporting over the sample period (see Table 5).

Table 10

Dynamic Changes in Innovation Output for the Full Sample

	PAT	TCITE	TSM
Quarterly × Before[2]	1.199	−1.415	.040
	(1.46)	(−.31)	(.06)
Quarterly × Before[1]	.531	−1.919	−.567
	(.60)	(−.36)	(−.72)
Quarterly × After[0]	−1.750*	−5.262	−1.723*
	(−2.04)	(−1.10)	(−1.97)
Quarterly × After[1]	−1.687+	−6.384	−1.772+
	(−1.91)	(−1.32)	(−1.94)
Quarterly × After[2]	−1.733+	−7.541	−1.870+
	(−1.88)	(−1.37)	(−1.88)
Quarterly × After[3]	−2.916**	−9.703+	−2.529**
	(−3.15)	(−1.71)	(−2.62)
Quarterly × After[4+]	−3.647**	−17.009*	−4.179**
	(−3.10)	(−2.35)	(−3.12)
Adjusted R^2	.761	.788	.697

Note. Results are estimates of the innovation dynamics surrounding the change in the frequency of reporting. All regressions include control variables, Semiannual interacted with leads and lags, firm and year fixed effects, and industry-specific linear trends. The two-tailed test t-statistics in parentheses are based on standard errors clustered by firm. $N = 9{,}904$.

$^{+}\ p < .10$.

$^{*}\ p < .05$.

$^{**}\ p < .01$.

To ensure that the treatment effects of quarterly reporting documented in Table 9 are not driven by differential pretrends, we add leads and lags as interaction terms to the model, as in Autor (2003). We also add industry-specific linear trends.[20] Table 10 reports the results. The coefficient estimates on the lead variables are all statistically insignificant, which suggests that the parallel-trend assumption is not violated; the coefficient estimates on the lag variables are negative and significant in 12 of 15 specifications, which suggests that, compared with control firms, treatment firms experience a drop in innovation output. Overall, these findings are consistent with our matched-sample results and suggest that increases in reporting frequency lead to decreases in innovation output.

7. Conclusion

We provide empirical evidence on the effect of the regulation of financial reporting frequency on corporate innovation. Based on two events—the SEC announcement of the quarterly-reporting requirement and Trump's tweet about reconsidering semiannual reporting—our analyses suggest that frequent reporting induces managerial myopia and is net costly to innovative firms. We also observe a temporary decrease of public firms' innovation around 1970, when the

[20] We do not add firm-specific linear trends because doing so significantly reduces the power of the test because of the limited number of firm-year observations (relative to the number of firms).

SEC mandate on quarterly reporting became effective. Using a difference-in-differences design, we find that firms experiencing an increase in reporting frequency exhibit a lower level of innovation output relative to control firms. We find no evidence that the net externality effect on industry peers is statistically significant. Overall, our results suggest that higher reporting frequency imposes short-term pressure on firms' managers and hence impedes innovation. Our evidence shows the real consequences of interim reporting frequency and has important policy implications for regulators and firms.

Appendix

Definitions of the Variables

A1. Measures of Innovation

PAT. Number of patents filed by firm *i* in year *t*

TCITE. Number of non-self-citations received by firm *i* on its patents filed in year *t*

TSM. Economic value of firm *i*'s patents (based on stock market reactions to patent grants) filed in year *t*, in millions of 1982 dollars

A2. Other Variables

LNMV. Natural logarithm of firm *i*'s market value of equity (PRCC_C × CSHO) measured at the end of fiscal year *t*

RD. Research and development expenditures (XRD) divided by the book value of assets (AT) measured at the end of fiscal year *t* and set to 0 if missing

ROA. Operating income before depreciation (OIBDP) divided by the book value of assets (AT), measured at the end of fiscal year *t;* missing values are replaced by the industry-year median

PPE. Property, plant, and equipment (net, PPENT) divided by the book value of assets (AT) measured at the end of fiscal year *t*

LEV. Firm *i*'s leverage ratio, defined as the book value of debt (DLTT + DLC) divided by the book value of assets (AT) measured at the end of fiscal year *t*

CAPEX. Capital expenditures (CAPXV) scaled by the book value of assets (AT) measured at the end of fiscal year *t*

HERF. Herfindahl-Hirschman index of four-digit Standard Industrial Classification industry *j* to which firm *i* belongs, measured at the end of fiscal year *t*

HERF_SQR. The square of HERF

Q. Firm *i*'s market-to-book ratio during fiscal year *t*, calculated as the market value of equity (PRCC_C × CSHO) plus the book value of assets (AT) minus the book value of equity (CEQ) minus balance-sheet deferred taxes (set to 0 if missing; TXDB) divided by the book value of assets (AT)

HPINDEX. The value of $-.737 \times \log(\text{Assets}) + .043 \times \log(\text{Assets})^2 - .040 \times$ Age, where Assets is the book value of assets (AT) and Age is the number of years the firm has been on Compustat with a nonmissing stock price; in calculating this index, Assets is replaced with \$4.5 billion and Age is replaced with 37 years if the actual values exceed these thresholds

LNAGE. Natural logarithm of 1 plus firm *i*'s age, approximated by the number of years listed on Compustat

AMIHUD. The yearly median of the Amihud (2002) price-impact measure, that is, daily absolute stock return divided by the trading volume (in thousands of dollars)

Treat. A dummy variable that equals one for treatment firms that experience an increase in reporting frequency and zero otherwise

Peer. A dummy variable that equals one for peer firms that do not experience a change in reporting frequency and zero otherwise; peer firms are matched to treatment firms on the basis of the closest propensity score and Fama-French 48 industry

Control. A dummy variable that equals one for control firms that do not experience any change in reporting frequency and zero otherwise; control firms from industries that have never experienced a change in reporting frequency are matched to treatment firms on the basis of the closest propensity score

$Before^{2}$. A dummy variable that equals one if a firm-year observation is from the second year before the frequency change (year −2) and zero otherwise

$Before^{1}$. A dummy variable that equals one if a firm-year observation is from the year before the frequency change (year −1) and zero otherwise

$After^{0}$. A dummy variable that equals one if a firm-year observation is from the year of the frequency change (year 0) and zero otherwise

$After^{1}$. A dummy variable that equals one if a firm-year observation is from the first year after the frequency change (year 1) and zero otherwise

$After^{2}$. A dummy variable that equals one if a firm-year observation is from the second year after the frequency change (year 2) and zero otherwise

$After^{3}$. A dummy variable that equals one if a firm-year observation is from the third year after the frequency change (year 3) and zero otherwise

$After^{4+}$. A dummy variable that equals one if a firm-year observation is from the fourth year or later after the frequency change (year 4+) and zero otherwise

Quarterly. A dummy variable that equals one for treatment firms that increase reporting frequency to the quarterly level over the sample period and zero otherwise

Semiannual. A dummy variable that equals one for treatment firms that increase reporting frequency to the semiannual level over the sample period and zero otherwise

Post_Q. A dummy variable that equals one if a firm-year observation is from a year after the reporting-frequency switch to the quarterly level and zero otherwise

Post_S. A dummy variable that equals one if a firm-year observation is from a year after the reporting-frequency switch to the semiannual level and zero otherwise

Table A1
Propensity-Score Regression

	Change		Change
LNMV	−.012	*Q*	.024
	(−.45)		(.99)
RD	−3.789*	HPINDEX	.198**
	(−2.10)		(4.00)
ROA	−.202	LNAGE	−.288**
	(−.72)		(−9.71)
PPE	−.196	AMIHUD	.383
	(−1.16)		(.70)
LEV	−.154	PAT_Growth	.005
	(−.92)		(1.50)
CAPEX	1.325**	TCITE_Growth	.001
	(2.95)		(1.21)
HERF	.295	TSM_Growth	.001
	(.83)		(.29)
HERF_SQR	−.166	Pseudo R^2	.079
	(−.55)		

Note. Results are parameter estimates from a probit model used to estimate propensity scores for firm *i*'s change in reporting frequency in year *t* for 1951–73. The dependent variable Change is a dummy that equals one for increases in reporting frequency and zero for no change in year *t*. The two-tailed test *z*-statistics in parentheses are based on standard errors clustered by firm. The innovation growth variables are computed over prior 3-year periods. $N = 9{,}904$ firm-years.

* $p < .05$.

** $p < .01$.

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