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.

Key words: regulation, reporting frequency, innovation, managerial myopia, externality.

JEL classification: G14, G18, M41, M45.

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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.

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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 (see He and Tian (2018) for a recent review). To stimulate innovation, governments typically implement policies providing protection of intellectual property rights. The economic consequences of these policies have been widely documented (e.g., 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, Hegde, Penas, and Seamans 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. Different from 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 multi-stage 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, resulting 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 U.S. 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 these studies in two important ways. First, unlike conventional investments (e.g., 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 (i.e., R&D) can have an immediate one-to-one negative effect on pre-tax 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, based on patent information. 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]

[1] Under current U.S. Generally Accepted Accounting Principles, research and development costs are expensed immediately. Under International Financial Reporting Standards, development costs can be capitalized as intangible assets.
[2] For example, using the same setting of brokerage closures and mergers to identify changes in analyst coverage, research documents drastically different effects of analyst coverage on capital expenditures and corporate innovation. While Derrien and Kecskes (2013) find that more analyst coverage leads to more capital expenditures (by reducing information asymmetry and the cost of capital), He and Tian (2013) find that more analyst coverage leads to a reduction in innovation (by imposing short-term pressure).

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 sub-optimally 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 (e.g., 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.

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

Therefore, the net externality effect of frequent reporting is unclear *ex-ante*.

We use the financial reporting frequency change in the U.S. as our empirical setting. The U.S. Securities and Exchange Commission (SEC) required annual financial reporting of listed firms in 1934, increased the frequency to semi-annual reporting in 1955, and further increased it to quarterly reporting in 1970. We perform two event studies to gauge the overall effect of reporting frequency on firm value. If more frequent reporting causes a firm's managers to become more myopic, the value of the firm would fall. Using a three-day event window around the SEC announcement of mandatory quarterly reporting (on September 15, 1969), we find a significant negative effect of 1% on the market value for firms that reported semi-annually but no significant effect for firms that already reported quarterly.[4] These results suggest that quarterly reporting is net costly for these semi-annual reporters, which explains why they had not voluntarily reported this way previously.

While the mandate on quarterly reporting has been in effect for almost five decades, President Trump recently (on August 17, 2018) asked the SEC, via a tweet, to review quarterly reporting and reconsider semi-annual reporting for public companies.[5] Using a three-day event window around Trump's tweet, we find a significant positive effect of 0.6% on the market value for firms in which innovation matters a lot but a relatively weaker effect of 0.3% on the market value for other firms. The significant difference in market reactions (i.e., 0.3%) between these two types of firms alleviates the concern that the positive market reaction for innovative firms reflects other implications of Trump's tweet (e.g., less burdensome disclosure requirements or more business-friendly regulation). Our results suggest that the cost of

[4] Prior to 1970, many firms had already reported more frequently than required by the SEC, due to stock exchange listing requirements or pressure. As early as 1923, the NYSE 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 AMEX and other regional exchanges took similar actions in 1962. See more detailed descriptions in the work of Leftwich, Watts, and Zimmerman (1981) and Butler, Kraft, and Weiss (2007).

[5] On August 17, 2018, President Trump tweeted "Stop quarterly reporting & go to a six month system." See the *Wall Street Journal* article by Michaels, Rapoport and Maloney (2018) for details.

quarterly reporting (i.e., exacerbating managerial myopia) matters more for innovative firms.

These 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. Specifically, 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 patent quantity, quality, and economic value, respectively.

Our interim reporting frequency data are from the work of Butler, Kraft, and Weiss (2007) and Fu, Kraft, and Zhang (2012) and span the period 1951–1973. 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 impossible to study the relation between reporting frequency and innovation using more recent U.S. 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 due to stock exchange requirements or investor pressure. 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 of 1951–1973, our patent data (ended in 2010) are unlikely to suffer from the usual truncation problems the innovation literature has to deal with (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, relative to aggregate innovation by other entities, as well as the individual and total trends over the sample period 1951–1973. We observe an overall upward trend for the ratio, suggesting that public firms' contribution to aggregate innovation generally increases over time. However, we observe a temporary decrease of public firm innovation around 1970 (when the SEC mandate on quarterly reporting took effect). This evidence suggests that the net impact of frequent reporting (aggregating both 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, leading to an increase in aggregate innovation. We fully acknowledge that these trends, while interesting, can only be interpreted 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 on financial reporting frequency affects corporate innovation. We designate firms that experience an increase in 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 treatment group is treated by increases in reporting frequency. The peer group is not subject to increases in reporting frequency but 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. Specifically, the difference-in-differences estimators show that, compared with control firms, mandatory adopters (i.e., firms that increase their reporting frequency due to 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 (i.e., firms that increase their reporting frequency due to 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 firm 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 potential interest to regulators and policymakers 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 two event studies to gauge the overall effect of reporting frequency on firm 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 Stromberg 2011), CEO 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 et al. 2013, 2014; Galasso and Luo 2017), market conditions (Nanda and Rhodes-Kropf 2013), corporate venture capitalists (Chemmanur et al. 2014), mergers and acquisitions (Bena and Li 2014), firm boundaries (Seru 2014), investors' attitudes toward failure (Tian and Wang 2014), banking competition (Cornaggia et al. 2015), bank interventions (Gu, Mao, and Tian 2018), 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 (e.g., Biddle and Hilary 2006; Biddle, Hilary, and Verdi 2009).[6] A notable exception is the work of Zhong (2018), who documents that transparency enhances firm innovation in an international setting. We build on the theoretical work of 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'

[6] See also 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), Garcia Lara, Garcia Osma, and Penalva (2016); and Shroff (2017, 2019).

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 than the literature 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, President Trump's tweet, gives us the opportunity to demonstrate that the cost of quarterly reporting (i.e., exacerbating managerial myopia) outweighs its benefit (i.e., 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 the 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 (see Minnis and Shroff (2017) for a thorough review).

Finally, our finding that more frequent reporting impedes corporate innovation is of interest to regulators and industry groups, who have recently debated on whether firms should be required to undertake more frequent interim financial reporting (e.g., Day 2003; European Commission 2004; Jopson 2006; Yiu 2009; Solomon 2011; Yahya 2016). The SEC is considering the pros and cons of replacing quarterly with semi-annual reporting (especially for smaller reporting companies).[7] 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.[8]

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 where 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 Panel A of Table 1.

[Insert Table 1 here.]

We gauge the market's reaction via CAR [0, 2], which is the cumulative abnormal return over the three-day window of [0, 2], with 0 being the event date. Its mean value is -1.0%, significant at the 10% level, for semiannual reporters (i.e., firms that reported semiannually before the announcement), while it averages 0.2%, statistically insignificant, for quarterly

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[7] https://www.sec.gov/news/speech/international-developments-higgins.html.

[8] 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.

reporters (i.e., firms that reported quarterly prior to the announcement). The difference between the two types of firms is significant at the 5% 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 President Trump's announcement on Twitter on August 17, 2018, which raised the possibility of dropping the quarterly reporting requirement. Panel B of Table 1 reports our results. We find that CAR [0, 2] is 0.6%, significant at the 1% level, for innovative firms (i.e., firms that have filed patents between 2005 and 2014),[9] while it is 0.3%, significant at the 1% level, for non-innovative firms (i.e., firms that have not filed patents between 2005 and 2014). The difference between the two types of firms is significant at the 5% level. Under the assumption that firms that file patents are the firms for which innovation matters, our results support the conjecture that the negative valuation impact of quarterly reporting is more severe for firms where innovation plays an important role.

Together, these two event studies provide preliminary evidence consistent with our hypothesis that frequent reporting induces managerial myopia and is net costly to innovative firms.

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 line "Total" represents the total number of patents filed in the year, divided by the total number of patents filed in 1973. From 1951 to 1965, the aggregate innovation in the United States increases steadily, reflecting the post-World War II prosperity and productivity gain.

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[9] Our patent data (collected from the U.S. 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, i.e., 2005–2014.

From 1965 to 1969, it shows a declining trend. From 1969 to 1973, it increases 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 through the lines labeled as "Public" and "Other," while the line "Relative" shows the number of patents filed by publicly listed firms divided by the number of patents filed by other entities.[10] We find that, relative to other entities, the number of patents attributable to publicly listed firms increases between 1951 and 1968, decreases between 1968 and 1971, and increases from 1971 to 1973. The decrease between 1968 and 1971 is consistent with the conjecture that the quarterly reporting requirement damps the innovation of publicly listed firms (aggregating both treatment effects and spillover effects). We, however, acknowledge that these trends, while interesting, can only be interpreted as being descriptive.

[Insert Figure 1 here.]

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 the work of Butler, Kraft, and Weiss (2007) and Fu, Kraft, and Zhang (2012), who hand-collect the data from *Moody's Industrial News Reports* covering the 1951–1973 period.[11] Reporting frequency is defined as one for annual reporters, two for semiannual reporters, three for firms reporting three times a year, and four for quarterly reporters. The following firms are excluded: firms not listed on either the NYSE or AMEX,

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

[11] 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.

firms lacking CRSP or Compustat data, and firms in industries with distinctive disclosure requirements (e.g., utilities; financial services, insurance, and real estate firms; and railroad and other transportation companies). We merge this dataset with the innovation data downloaded from http://iu.box.com/patents (see Kogan et al. (2017) for a detailed description of the data). Following the innovation literature (e.g., He and Tian 2013), we set the innovation proxies to zero 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 for the period 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 actual 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 firm shareholders. Our data for these innovation measures end in 2010. Since our sample period of 1951–1973 ends long before 2010, our patent variables are unlikely to suffer from the typical truncation problems the innovation literature needs to deal with.

Control variables include firm size, *LNMV*, measured by the natural logarithm of firm market capitalization; investment in innovation, *RD*, measured by R&D expenditures scaled by total assets;[12] 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 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 index based on annual sales;[13] growth opportunities, *Q*, measured by Tobin's Q; financial constraints, *HPINDEX*, a financial constraint measure based on firm size and age that is developed by Hadlock and Pierce (2010);[14] firm age, *LNAGE*, measured by the natural logarithm of one 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 (i.e., daily absolute stock return divided by the dollar trading volume measured in 1000s).

5.3.Descriptive statistics

Panel A of Table 2 reports the distribution of sample firms by reporting regimes. In the period of 1951–1954, when only annual reporting is required, 22.55% of our sample firms report semiannually, and 66.67% of them report quarterly. In the period of 1955–1969, when semiannual reporting starts to be compulsory, 86.61% of our sample firms report quarterly. In the period of 1970–1973, 96.60% of our sample firms report quarterly. These three periods account for 5.06%, 59.86%, and 35.08% of our sample, respectively. There are 1.68% of our sample observations that report three times a year because firms may decide to switch from semi-annual 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

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[12] Our results are largely unchanged when we control for cumulative R&D expenditures over the current year and the previous one, two, or three years.

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

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

setting for our investigation. In total, our full sample consists of 1,117 unique firms and 9,904 firm-year observations.[15]

[Insert Table 2 here.]

Panel B of Table 2 provides descriptive statistics for our full sample. *PAT* has a mean of 6.46 and a median of 0. The mean and median of *TCITE* are 46.26 and 0, respectively. *TSM* has a mean of 4.32, while its median is 0. *LNMV* has a mean of 3.83 and a median of 3.70. The mean and median of *RD* are 0.006 and 0, respectively. The mean of *ROA*, *PPE*, *LEV*, and *CAPEX* indicate that an average firm in our sample has a return on asset ratio of 14.8%, its *PPE* (net) is about 32% of its total assets, its leverage ratio is 22%, and its capital expenditure is about 6% of its total assets. The mean of *HERF* is 0.48, while the mean of *Q* is 1.7. The mean of *HPINDEX* is -2.37, and the median is -2.43. The mean of *LNAGE* is 1.46, while the average of *AMIHUD* is 0.015.

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 innovation for those reporting firms. Second, frequent reporting improves firms' access to financing and monitoring from capital markets and thus enhances firm innovation. Third, frequent reporting 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

[15] Due to some of our sample firms switching their reporting frequency during the sample period, the total number of unique firms in our full sample does not equal to the sum of the number of unique firms across different frequency.

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 treatment, peer, and control groups of firms using propensity-score matching. Specifically, we first use the full sample to run an ordered probit model to estimate the propensity score related to the change of reporting frequency (see Appendix B). We then use the predicted probabilities, or propensity scores, to perform a nearest-neighbor propensity-score matching to identify peer firms 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 (based on Fama-French 48 industries) as treatment firms because externalities are most likely to occur among industry peers. Control firms are from other industries.

Table 3 reports the distributions of treatment firms (firms that increase their frequency) used in the tests. Panel A reports the distribution according to the change in reporting frequency. In the period of 1951–1954, 57 firms increase their reporting frequency to the semiannual level, 20 firms to three times a year, and 89 firms to the quarterly level. In the period of 1955–1969, 252 firms increase to the semiannual level, 157 firms to three times a year, and 426 firms increase to the quarterly level. In the period of 1970–1973, the number of firms increasing to the semiannual level, the three-times-a-year level, and the quarterly level is 9, 13 and 52, respectively. Our findings are largely consistent with those of Kraft et al. (2018).[16]

[16] 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.

[Insert Table 3 here.]

Panel B 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. Specifically, 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 increased the frequency to the quarterly level after 1967. The switch is deemed as a result of exchange requirement if the firm is listed on the AMEX and increased its frequency to the quarterly level starting one year before and up to two years after 1962 (the year in which the AMEX started urging existing firms and requiring newly listed firms to switch to quarterly reporting). During our sample period, there was no change in NYSE's listing rules regarding reporting frequency. We assume that, if firms are not required by either the SEC or the stock exchange to switch their reporting frequency, the reporting frequency switches are due to the demand of investors. Overall, there are 366 switches as a result of the SEC regulation, 133 switches as a result of exchange requirements, and 576 switches as a result of investor demand.

Our matched sample includes firm-year observations for treatment, peer, control firms over a six-year window centered on the year of the switch in the reporting frequency (Fang, Tian, and Tice 2014). Out of the 1,075 treatment firms with reporting frequency increases, 491 firms engage in some patenting activities during the sample period.[17]

Following Kraft et al. (2018), we classify firms that increase their reporting frequency due 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

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

of both mandatory and voluntary adopters suggests that the cost-benefit tradeoff varies across firms. Firms voluntarily adopt more frequent reporting when the benefit of doing so (e.g., lower cost of equity) outweighs the cost (e.g., reduced innovation), while the opposite is likely true for mandatory adopters.

We depict in Figure 2 the values of the three innovation measures for the six years surrounding the mandatory increase in reporting frequency for the treatment, peer, and control groups. Year 0 (omitted from the figures) is the year of the switch. Panels A to C show that the two lines representing innovation output for the treatment group and control group trend closely in parallel in the three 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 innovation output for the peer group and control group trend closely in parallel over the six-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.

[Insert Figure 2 here.]

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 to 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).[18] Lastly, the difference-in-differences approach controls for unobserved constant differences between the treatment (or peer) group and the control group.

Based on 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 4 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 results for mandatory increases in Panel A and voluntary increases in Panel B. In both panels of Table 4, row (1) reports the change in innovation activities for treatment firms after the switch in financial reporting frequency. Specifically, 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 patent counts/citation counts/economic values over the three years preceding the switch in reporting frequency from the patent counts/citation counts/economic values over the three years following the switch in reporting frequency for each treatment firm. The differences are then averaged over the treatment group. Similarly, we calculate the average change in patent counts/citation counts/economic values for the peer and control groups and report them in rows (2) and (3). In rows (4) to (6), we report the mean difference-in-differences (DiD) estimators and the corresponding two-tailed *t*-statistics, testing the null hypothesis that the estimators are zero.

[Insert Table 4 here.]

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[18] A caveat is that our sample does not include firms that choose to delist in the presence of additional disclosure mandates (e.g., Bushee and Leuz 2005).

Panel A of Table 4 reports the results for mandatory increases in reporting frequency. We find that these 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 in row (4) 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 economic value for the treatment firms relative to the control firms. We find similar treatment effects when using peer firms as the benchmark group (see row (6)).

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 of peer firms. On the other hand, it elevates the short-termism of treatment firms and, through peer pressure, imposes similar changes on peer firms, resulting in lower innovation. Results in row (5) 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% confidence intervals based on the estimates in row (5), which are [-1.76, 0.45] for patents, [-14.06, 17.57] for non-self-citations, and [-0.95, 1.43] for the economic value. Thus, the largest externality effect that can be ruled out at the 95% level appears to be relatively big and comparable to the average treatment effects estimated in row (4), though we observe both large positive and negative externalities.

Panel B of Table 4 reports the results for voluntary increases in reporting frequency. We find that the innovation output of the treatment firms decreases after the switches (row (1)). We find all three innovation measures increase significantly after the switch for peer firms and, to a less extent, for control firms (rows (2) and (3)). 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.[19]

6.3. Difference-in-differences regression analyses

In this subsection, 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 six-year window centered on the year of the switch in the reporting frequency, and estimate the following model:

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

The dependent variable is one of the three innovation output measures (*PAT*, *TCITE*, and *TSM*). *TREAT* is a dummy variable that 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

[19] The largest externality effect that can be ruled out at the 95% level still appears to be relatively big. The 95% confidence intervals based on the estimates in row (5) are [-1.73, 1.42] for patents, [-5.59, 14.79] for non-self-citations, and [-1.91, 1.43] for the economic value.

a dummy variable that equals one if a firm-year observation is from the second year before the switch in reporting frequency (year −2) and zero otherwise; $BEFORE^1$ is a dummy variable that equals one if a firm-year observation is from the year before the switch in reporting frequency (year −1) 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 (year 1) 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 (year 2) 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 (year 3) and zero otherwise. Also, we include the firm and year fixed effects.

The key coefficient estimates are β_1 to 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).

[Insert Table 5 here.]

We report the regression results from estimating equation (1) in Table 5. Results related to the dependent variable *PAT*, *TCITE*, and *TSM* are reported in columns (1), (2), and (3). Panel A reports the results for mandatory increases in reporting frequency. β_1, β_2, β_6, and β_7 are all statistically insignificant in columns (1) to (3), suggesting that the parallel-trend assumption is not violated. β_3, β_4, and β_5 are negative and significant in eight out of nine specifications, consistent with our hypothesis that more frequent reporting leads to less corporate innovation for the treatment firms. β_8, β_9, and β_{10} are all statistically insignificant.

Panel B reports the results for voluntary increases in reporting frequency. Our results resemble those reported in Panel A. In all three columns, β_1, β_2, β_6, and β_7 are all statistically insignificant, suggesting that the parallel-trend assumption is not violated; β_3, β_4, and β_5 are negative and significant in eight out of nine specifications, suggesting 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 (e.g., 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 six-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 (i.e., 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 1951–1973 is not feasible as most industries included in our sample are "treated" over this period.[20] In this section, we use the full sample and a generalized different-in-differences estimator that exploits the staggered nature of the treatment effects as a robustness check. Specifically, we use firm-year observations for the full sample and estimate the following model:

[20] An industry is "treated" when at least one firm in this industry switches reporting frequency.

$$INNOV = \alpha + \beta_1 QUARTERLY \times POST\_Q + \beta_2 SEMIANNUAL \times POST\_S + CONTROLS$$
$$+ \textit{Firm Fixed Effects} + \textit{Year Fixed Effects} + \varepsilon. \quad (2)$$

The dependent variable is one of the three innovation output measures (*PAT*, *TCITE*, and *TSM*). *QUARTERLY* is a dummy variable that 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 the 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 patents with smaller economic value, in the years following the reporting frequency change to the quarterly (or semiannual) level.

[Insert Table 6 here.]

We report the regression results from estimating equation (2) in Table 6. Results related to the dependent variable *PAT*, *TCITE*, and *TSM* are reported in columns (1), (2), and (3). β_1 is negative and significant in all three specifications, consistent with our hypothesis that more frequent reporting leads to less corporate innovation for the treatment firms. The negative but statistically insignificant β_2 suggests that switching from annual reporting to semiannual reporting is not particularly costly to treatment firms. But this latter result should be interpreted with caution given the limited number of treatment firms that switch to semiannual reporting over the sample period (see Table 3).

To ensure that the treatment effects of quarterly reporting documented in Table 6 are not driven by differential pre-trends, we add leads (i.e., $BEFORE^2$ and $BEFORE^1$) and lags (i.e., $AFTER^0$, $AFTER^1$, $AFTER^2$, $AFTER^3$, and $AFTER^{4+}$) as interaction terms to the model as in Autor (2003). We also add industry-specific linear trends in the specification.[21] Table 7 reports the results. In all three columns, the coefficient estimates on the lead variables are all statistically insignificant, suggesting that the parallel-trend assumption is not violated; the coefficient estimates on the lag variables are negative and significant in 12 out of 15 specifications, suggesting that compared with control firms, treatment firms experience a drop in innovation output.

[Insert Table 7 here.]

Overall, these findings are consistent with our matched sample results and suggest that increases in reporting frequency lead to drops in innovation output.

7. Conclusion

We provide empirical evidence on the effect of financial reporting frequency regulation on corporate innovation. Based on two events—the SEC announcement of the quarterly reporting requirement and President Trump's tweet on reconsidering semi-annual 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 firm innovation around 1970 (when the SEC mandate on quarterly reporting became effective). Using a difference-in-differences design, we find that firms experiencing an increase in reporting frequency exhibit

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

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 firm managers and hence impedes innovation. Our evidence shows the real consequences of interim reporting frequency and has important policy implications for regulators and firms.

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Appendix A. Definition of variables

Variable definitions

Variable	Definition
Measures of innovation	
PAT	Firm *i*'s total number of patents filed in year *t*.
TCITE	Firm *i*'s total number of non-self-citations received on the firm's patents filed in year *t*.
TSM	The total economic value of firm *i*'s patents (based on stock market reactions to patent grants) filed in year *t*, expressed in 1982 dollar values (in million), respectively.
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 book value of total assets (AT) measured at the end of fiscal year *t* and set to zero if missing.
ROA	Operating income before depreciation (OIBDP) divided by book value of total assets (AT), measured at the end of fiscal year *t*. A missing value is replaced by the industry-year median.
PPE	Property, plant, and equipment (net) (PPENT) divided by book value of total 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 book value of total assets (AT) measured at the end of fiscal year *t*.
CAPEX	Capital expenditures (CAPXV) scaled by the book value of total assets (AT) measured at the end of fiscal year *t*.
HERF	Herfindahl index of four-digit SIC 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 book value of assets (AT) minus book value of equity (CEQ) minus balance sheet deferred taxes (set to zero if missing) (TXDB) divided by book value of assets (AT).
HPINDEX	$-0.737\times\log(ASSETS) + 0.043\times\log(ASSETS)^2 - 0.040\times AGE$, where *ASSETS* is the book value of total assets (AT), and *AGE* is the number of years the firm has been on Compustat with a non-missing stock price. In calculating this index, *ASSETS* is replaced with $4.5 billion and *AGE* with thirty-seven years if the actual values exceed these thresholds.
LNAGE	Natural logarithm of one 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, i.e., daily absolute stock return divided by the dollar trading volume (measured in 1000s).
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 any change in reporting frequency and zero otherwise. Peer firms are

	matched to treatment firms based on 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 any change in reporting frequency are matched to treatment firms based on 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 right before the switch year (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 switch year (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.

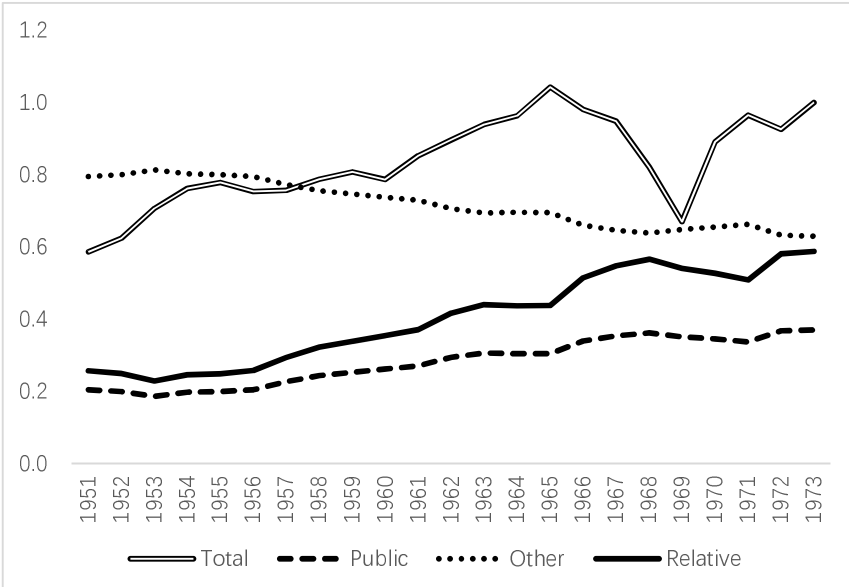
This appendix describes the calculation of variables used in the main analyses of this study.

Appendix B. Propensity score regression

Dependent Variable	*CHANGE*
LNMV	-0.012
	(-0.45)
RD	-3.789**
	(-2.10)
ROA	-0.202
	(-0.72)
PPE	-0.196
	(-1.16)
LEV	-0.154
	(-0.92)
CAPEX	1.325***
	(2.95)
HINDEX	0.295
	(0.83)
HINDEX_SQR	-0.166
	(-0.55)
Q	0.024
	(0.99)
HPINDEX	0.198***
	(4.00)
LNAGE	-0.288***
	(-9.71)
AMIHUD	0.383
	(0.70)
PAT_GROWTH	0.005
	(1.50)
TCITE_GROWTH	0.001
	(1.21)
TSM_GROWTH	0.001
	(0.29)
Pseudo R-squared	0.079
Observations	9,904

This table reports the parameter estimates from a probit model used to estimate propensity scores for firm *i*'s change in reporting frequency in year *t*. The dependent variable *CHANGE* is a dummy variable with the value of 1 for increases in reporting frequency and 0 for no change in reporting frequency in year *t*. The two-tailed test *z*-statistics in parentheses are based on standard errors clustered by firm. The sample contains 9,904 firm-year observations from 1951 to 1973. The innovation growth variables, i.e., the growth in the number of patents (*PAT_CHG*), the growth in the number of non-self-citations a firm's patents receive (*TCITE_CHG*), and the growth in the value of a firm's patents (*TSM_CHG*), are computed over prior three-year periods. All other variables are defined in Appendix A. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

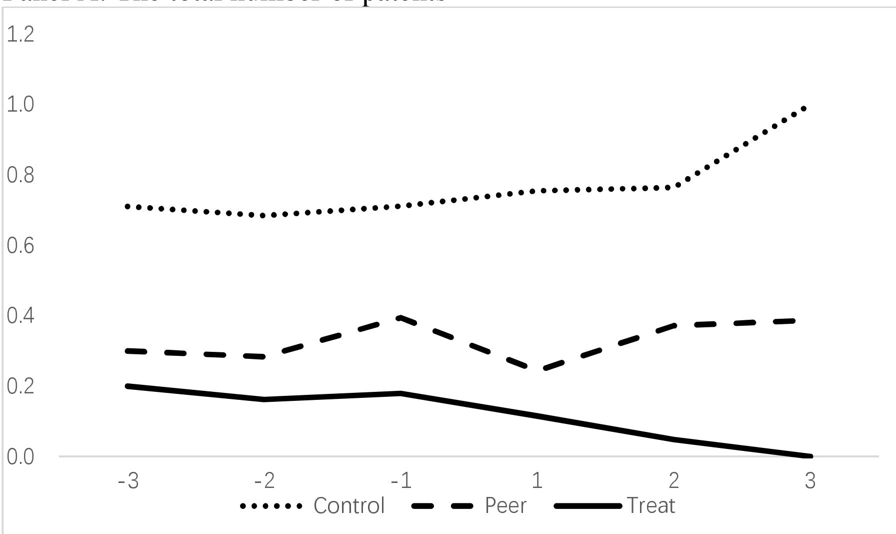
Figure 1. Trends of aggregate innovation



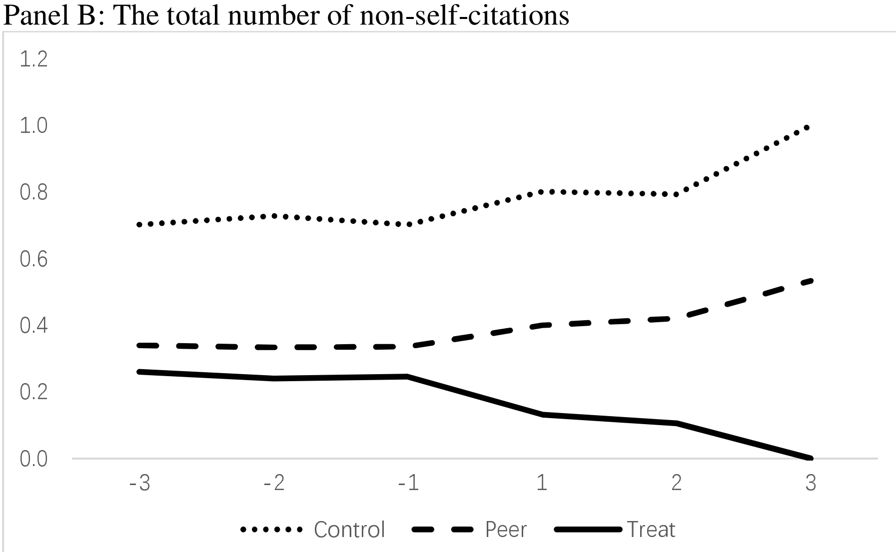
Note: Patent data are obtained from Kogan et al. (2017). "Total" is calculated as the total number of patents by all entities in a year divided by the total number of patents in 1973. "Public" ("Other") refers to the total number of patents by public firms (other entities) relative to the total number of patents by all entities in a year. "Relative" is calculated as the ratio of the total number of patents by public firms ("Public") to the total number of patents by other entities ("Other").

Figure 2. Trends of innovation proxies around the mandatory increase in reporting frequency

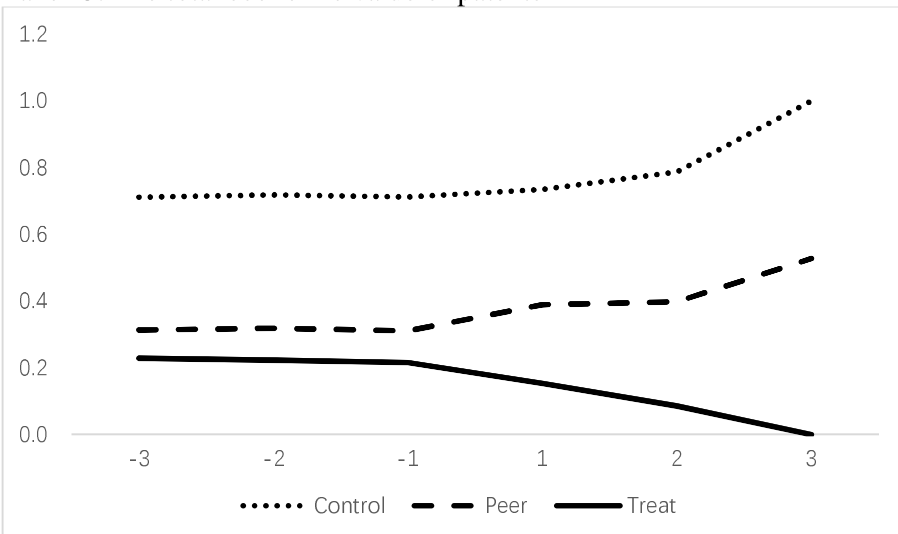
Panel A: The total number of patents



Panel B: The total number of non-self-citations



Panel C: The total economic value of patents



Note: "Treat" refers to treatment firms that experience an increase in reporting frequency. "Peer" refers to peer firms that do not experience any change in reporting frequency and are matched to treatment firms based on the closest propensity score and (Fama-French 48) industry. "Control" refers to control firms that are matched to treatment firms based on the closest propensity score but from industries that have never experienced any change in reporting frequency. The values of all innovation proxies are adjusted by sample averages in each year and standardized to range between zero and one.

Table 1
Event studies

Panel A: SEC announcement on quarterly reporting requirement for all listed firms on September 15, 1969

CAR [0, 2]	Semiannual reporters	Quarterly reporters	Difference
Mean	-0.010*	0.002	-0.012**
	(-1.75)	(1.21)	(-2.23)
Observations	80	908	

Panel B: President Trump's tweet on dropping quarterly reporting requirement on August 17, 2018

CAR [0, 2]	Innovative firms	Non-innovative firms	Difference
Mean	0.006***	0.003***	0.003**
	(5.46)	(8.01)	(2.29)
Observations	1,023	6,397	

This table reports the 3-day cumulative abnormal stock returns around two events. Panel A reports the results on the SEC announcement of quarterly reporting requirement for all listed firms on September 15, 1969. Its sample includes all firms with non-missing stock returns and reporting frequency. Panel B reports the results on President Trump's tweet on dropping quarterly reporting requirement on August 17, 2018. Its sample includes all listed firms with non-missing stock returns on the announcement date. Innovative firms (non-innovative firms) are defined as those with (without) patents filed in the period between 2005 and 2014. CAR [0, 2] is the cumulative market-adjusted abnormal returns during the three-day window starting from the announcement date to two days after the announcement date. The *t*-statistics are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 2
Descriptive statistics

Panel A: Sample distribution

Period	N	Freq=1(%)	Freq=2(%)	Freq=3(%)	Freq=4(%)	Total (%)
1951-1954	501	7.78	22.55	2.99	66.67	5.06
1955-1969	5,929	1.08	10.47	1.84	86.61	59.86
1970-1973	3,474	0.55	1.64	1.21	96.60	35.08
Total	9,904	1.23	7.99	1.68	89.11	100.00
No. of unique firms	1,117	58	189	128	1,089	

Panel B: Descriptive statistics of the sample

Variable	N	Mean	Std Dev	Q1	Median	Q3
PAT	9,904	6.464	15.483	0.000	0.000	4.000
TCITE	9,904	46.262	108.416	0.000	0.000	28.000
TSM	9,904	4.319	14.053	0.000	0.000	0.821
LNMV	9,904	3.830	1.641	2.623	3.697	4.972
RD	9,904	0.006	0.015	0.000	0.000	0.000
ROA	9,904	0.148	0.084	0.100	0.144	0.195
PPE	9,904	0.319	0.170	0.198	0.294	0.413
LEV	9,904	0.216	0.157	0.088	0.205	0.319
CAPEX	9,904	0.063	0.053	0.026	0.052	0.085
HERF	9,904	0.483	0.313	0.225	0.385	0.727
Q	9,904	1.703	1.042	1.055	1.422	1.967
HPINDEX	9,904	-2.365	0.693	-2.887	-2.430	-1.943
LNAGE	9,904	1.458	0.955	0.693	1.609	2.197
AMIHUD	9,904	0.015	0.032	0.000	0.004	0.014

This table reports the sample distribution (in Panel A) and the descriptive statistics for the sample (in Panel B). The sample contains 9,904 firm-year observations from 1951 to 1973. The variables are defined in Appendix A.

Table 3
Distribution of the sample for the difference-in-differences tests

Panel A: Distribution across frequency

Period	Increase to semiannual	Increase to three times	Increase to quarterly	Total
1951-1954	57	20	89	166
1955-1969	252	157	426	835
1970-1973	9	13	52	74
Total	318	190	567	1,075
No. of firms with non-zero patent	132	85	274	491

Panel B: Distribution across switch reason

Period	SEC requirement	Exchange requirement	Investor demand	Total
1951-1954	0	0	166	166
1955-1969	305	133	397	835
1970-1973	61	0	13	74
Total	366	133	576	1,075
No. of firms with non-zero patent	144	51	296	491

This table reports the distribution of the sample for the difference-in-differences tests. Panel A reports the time-series distribution of treatment firms experiencing an increase in reporting frequency across reporting frequency and Panel B presents such distribution across switch reason. The distribution of treatment firms with a non-zero patent is reported at the end of each panel.

Table 4
Simple difference-in-differences test

Variable	*PAT*	*TCITE*	*TSM*
	(1)	(2)	(3)
Panel A：Mandatory increases in reporting frequency (N=499)			
(1) Mean *TREAT* difference	-1.071***	-12.404***	-1.020***
(After – Before)	(-10.71)	(-22.59)	(-20.46)
(2) Mean *PEER* difference	0.137	8.935***	0.981***
(After – Before)	(0.48)	(3.33)	(3.66)
(3) Mean *CONTROL* difference	0.794	7.179	0.739
(After – Before)	(1.65)	(0.96)	(1.54)
(4) Mean DiD estimator	-1.865***	-19.583**	-1.758***
(Δ*TREAT* – Δ*CONTROL*)	(-3.79)	(-2.61)	(-3.64)
(5) Mean DiD estimator	-0.657	1.756	0.242
(Δ*PEER* – Δ*CONTROL*)	(-1.18)	(0.22)	(0.40)
(6) Mean DiD estimator	-1.208***	-21.339***	-2.000***
(Δ*TREAT* – Δ*PEER*)	(-4.01)	(-7.78)	(-7.33)
Panel B：Voluntary increases in reporting frequency (N=576)			
(1) Mean *TREAT* difference	-0.738***	-5.688***	-0.809***
(After – Before)	(-4.08)	(-7.27)	(-7.64)
(2) Mean *PEER* difference	1.260***	11.159***	1.207**
(After – Before)	(5.00)	(4.74)	(5.33)
(3) Mean *CONTROL* difference	1.417*	6.561	1.444*
(After – Before)	(1.88)	(1.31)	(1.77)
(4) Mean DiD estimator	-2.155***	-12.249**	-2.253***
(Δ*TREAT* – Δ*CONTROL*)	(-2.78)	(-2.42)	(-2.74)
(5) Mean DiD estimator	-0.157	4.598	-0.237
(Δ*PEER* – Δ*CONTROL*)	(-0.20)	(0.89)	(-0.28)
(6) Mean DiD estimator	-1.998***	-16.848***	-2.016***
(Δ*TREAT* – Δ*PEER*)	(-6.44)	(-6.79)	(-8.06)

This table provides the univariate difference-in-differences test results. Panel A (B) presents the results for mandatory (voluntary) increases in reporting frequency. *PAT* (*TCITE*, or *TSM*) is the sum of firm *i*'s number of patents (number of citations, or value of patents), adjusted by sample averages in each year, over the three-year window before or after the frequency switch. *TREAT*, *PEER*, and *CONTROL* refer to firms experiencing increases in reporting frequency, the matched peer firms from the same industry, and the matched control firms from other industries. All variables are defined in Appendix A. The *t*-statistics are given in parentheses below the mean differences in innovation outcomes. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 5
Difference-in-differences regression analyses

Dependent variable	*PAT*	*TCITE*	*TSM*
	(1)	(2)	(3)
Panel A：Mandatory increases in reporting frequency			
TREAT×BEFORE^{2}	-0.181	-4.927	-0.190
	(-0.31)	(-1.17)	(-0.95)
TREAT×BEFORE^{1}	-0.261	-1.893	-0.156
	(-0.34)	(-0.49)	(-0.34)
TREAT×AFTER^{1}	-1.315**	-18.857**	-0.863**
	(-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}	0.079	-3.540	-0.058
	(0.13)	(-0.92)	(-0.26)
PEER×BEFORE^{1}	0.824	-0.842	-0.040
	(1.01)	(-0.20)	(-0.09)
PEER×AFTER^{1}	-0.862	-3.260	0.401
	(-1.34)	(-0.40)	(0.99)
PEER×AFTER^{2}	0.293	0.297	0.143
	(0.28)	(0.02)	(0.19)
PEER×AFTER^{3}	-0.953	0.387	0.065
	(-0.88)	(0.02)	(0.05)
BEFORE^{2}	0.076	5.859	0.293
	(0.13)	(1.41)	(1.54)
BEFORE^{1}	1.141	4.277	0.409
	(1.65)	(1.04)	(0.83)
AFTER^{1}	1.225*	21.307**	0.894**
	(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)
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Adjusted R-squared	0.725	0.741	0.748
Observations	4,832	4,832	4,832
Panel B：Voluntary increases in reporting frequency			
TREAT×BEFORE^{2}	0.351	-9.197	-0.220
	(0.78)	(-1.50)	(-0.73)
TREAT×BEFORE^{1}	-0.047	-7.088	-0.387
	(-0.09)	(-1.19)	(-0.89)
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	-0.341	-9.496	-0.356
	(-0.88)	(-1.56)	(-1.22)
PEER×BEFORE^1	-0.087	-5.943	-0.437
	(-0.17)	(-0.99)	(-1.00)
PEER×AFTER^1	-1.375	0.468	-1.396
	(-1.13)	(0.05)	(-1.22)
PEER×AFTER^2	-0.140	-1.010	-0.648
	(-0.10)	(-0.08)	(-0.44)
PEER×AFTER^3	-0.061	0.840	0.104
	(-0.05)	(0.07)	(0.06)
BEFORE^2	0.358	9.825*	0.365
	(0.95)	(1.69)	(1.32)
BEFORE^1	0.810**	8.159	0.656*
	(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)
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Adjusted R-squared	0.580	0.633	0.453
Observations	5,668	5,668	5,668

This table reports regression estimates of the innovation dynamics of treatment and control firms surrounding the frequency switch, i.e., three years before and after the frequency change. Panel A (B) presents the results for mandatory (voluntary) increases in reporting frequency. The dependent variable is innovation output measured by *PAT*, *TCITE*, or *TSM* in a given year. *TREAT* and *PEER* refer to treatment firms that experience increases in reporting frequency and the matched peer firms from the same industry. Firm and year fixed effects are included in all regressions. All variables are defined in Appendix A. The two-tailed test *t*-statistics in parentheses are based on standard errors clustered by firm. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 6
Difference-in-differences analyses based on the full sample

Dependent variable	*PAT*	*TCITE*	*TSM*
	(1)	(2)	(3)
QUARTERLY	-5.438***	-25.541***	-5.151***
×*POST*	(-6.35)	(-5.31)	(-5.24)
SEMIANNUAL	-2.187	-7.815	-1.194
×*POST*	(-1.40)	(-0.86)	(-1.31)
LNMV	1.979***	18.465***	3.675***
	(3.66)	(4.85)	(5.06)
RD	156.208***	1033.758***	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
	(0.85)	(0.84)	(1.29)
LEV	1.300	6.841	0.017
	(0.72)	(0.62)	(0.01)
CAPEX	-13.560***	-79.925***	-16.439***
	(-4.05)	(-3.44)	(-3.48)
HERF	0.289	2.377	0.611
	(0.04)	(0.05)	(0.08)
HERF_SQR	0.604	6.607	1.566
	(0.11)	(0.18)	(0.25)
Q	-0.709**	-3.369*	-0.894**
	(-2.18)	(-1.77)	(-1.98)
HPINDEX	0.693*	1.244	1.273***
	(1.93)	(0.50)	(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)
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Adjusted R-squared	0.729	0.761	0.643
Observations	9,904	9,904	9,904

This table reports regression estimates of the impact of reporting frequency on innovation based on the full sample. The dependent variable is innovation output measured by *PAT*, *TCITE*, or *TSM* in a given year. *QUARTERLY* (*SEMIANNUAL*) refers to treatment firms that increase reporting frequency to the quarterly (semiannual) level. Firm and year fixed effects are included in all regressions. All variables are defined in Appendix A. The two-tailed test *t*-statistics in parentheses are based on standard errors clustered by firm. ***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively.

Table 7
Dynamic changes in innovation output based on the full sample

Dependent variable	*PAT*	*TCITE*	*TSM*
	(1)	(2)	(3)
QUARTERLY	1.199	-1.415	0.040
$\times$*BEFORE*^{2}	(1.46)	(-0.31)	(0.06)
QUARTERLY	0.531	-1.919	-0.567
$\times$*BEFORE*^{1}	(0.60)	(-0.36)	(-0.72)
QUARTERLY	-1.750**	-5.262	-1.723**
$\times$*AFTER*^{0}	(-2.04)	(-1.10)	(-1.97)
QUARTERLY	-1.687*	-6.384	-1.772*
$\times$*AFTER*^{1}	(-1.91)	(-1.32)	(-1.94)
QUARTERLY	-1.733*	-7.541	-1.870*
$\times$*AFTER*^{2}	(-1.88)	(-1.37)	(-1.88)
QUARTERLY	-2.916***	-9.703*	-2.529***
$\times$*AFTER*^{3}	(-3.15)	(-1.71)	(-2.62)
QUARTERLY	-3.647***	-17.009**	-4.179***
$\times$*AFTER*$^{4+}$	(-3.10)	(-2.35)	(-3.12)
Controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry-specific trend	Yes	Yes	Yes
Adjusted R-squared	0.761	0.788	0.697
Observations	9,904	9,904	9,904

This table reports regression estimates of the innovation dynamics surrounding the frequency switch based on the full sample. The dependent variable is innovation output measured by *PAT*, *TCITE*, or *TSM* in a given year. *QUARTERLY* (*SEMIANNUAL*) refers to treatment firms that increase reporting frequency to the quarterly (semiannual) level. All regressions include the same set of control variables as in Table 6, *SEMIANNUAL* interacted with leads and lags, firm and year fixed effects, and industry-specific linear trends. All variables are defined in Appendix A. The two-tailed test *t*-statistics in parentheses are based on standard errors clustered by firm. ***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively.