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Unintended Real Effects of EDGAR: Evidence from Corporate Innovation

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ABSTRACT: We study the real effects on innovation of a transformative change in corporate disclosure dissemination, the implementation of the SEC's EDGAR system. On the one hand, increased disclosure dissemination can lower firms' cost of capital, thereby stimulating innovative activity. On the other hand, increased dissemination can exacerbate proprietary disclosure costs, reducing firms' incentives to innovate. We show that treated firms reduce innovation investment following EDGAR's implementation. In contrast, EDGAR reporting firms' innovation investment cuts are met with an increase in innovation investment by their technology rivals. Consistent with an increase in proprietary costs, EDGAR-filers disclose less about their innovation activities. We also find evidence of a redistribution of innovative activity from public to private firms not subject to EDGAR disclosure requirements. Overall, our results are consistent with increased disclosure dissemination crowding out investment in innovative projects, whose returns negatively depend on information spillovers.

JEL Classifications: D23; L86; M40; M41; O30; O31; O32; O34.

Keywords: corporate innovation; research and development; patents; EDGAR financial reporting; disclosure processing costs; proprietary costs; dissemination costs.

I. INTRODUCTION

The advent of the SEC's Electronic Data Gathering, Analysis, and Retrieval (EDGAR) system in 1993 transformed the way in which corporate disclosures are collected, processed, and disseminated. Prior literature exploiting the EDGAR implementation as a large shock to disclosure dissemination finds that investors' information environments improve after EDGAR (e.g., Gao and Huang 2020; Chang, Ljungqvist, and Tseng 2023; Lai, Lin, and Ma 2024). In this study, we focus on an economically important consequence of EDGAR, namely its effect on corporate innovation. This potential effect is imperative to study because of both the critical role that innovation plays

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in cultivating economic growth (Solow 1957; Bernstein 2015) and innovation's theoretical relation to proprietary disclosure costs (Glaeser and Lang 2023).

Whether and how EDGAR affects corporate innovation is *ex ante* unclear. On the one hand, increased disclosure dissemination via EDGAR may stimulate increased financial visibility, thereby expanding a firm's investor base (Merton 1987). This could, in turn, result in a lower cost of capital and increased innovation investment. Specifically, the improved dissemination of corporate information following the introduction of EDGAR could enable financially constrained firms to attract capital by increasing the visibility of their positive net present value (NPV) innovative projects to investors, thereby alleviating adverse selection issues in securities issuance. Consistent with this idea, previous research finds that the EDGAR implementation leads to an increase in traditional investment (i.e., capital expenditures) (Goldstein, Yang, and Zuo 2023) and a lower cost of capital (Lai et al. 2024).

On the other hand, EDGAR makes it easier for competitors to assimilate corporate information, given that financial disclosures are an important source of competitive intelligence (Guo, Lev, and Zhou 2004; Brown and Tucker 2011). As EDGAR improves the dissemination of corporate disclosures, it may increase the spillover of information to competitors, which can exacerbate the proprietary costs of disclosure. First, easier access to information on segment sales and profitability could intensify the threat of new entrants for incumbent firms (e.g., Dedman and Lennox 2009; Bernard, Burgstahler, and Kaya 2018; Berger, Choi, and Tomar 2024). Second, the increased dissemination of financial disclosures related to significant business developments and material risks (Aghamolla and Thakor 2022), as well as the level of R&D expenditures and acquired R&D, may inform competitors about peers' innovative strategies (Breuer, Leuz, and Vanhaverbeke 2022). This increased information dissemination could prompt competitors to seek additional external information (e.g., from trade fairs or patent disclosures) to aid in imitating firms' innovative activities (Breuer et al. 2022; Kim and Valentine 2023). Because of both the threat of entry and the threat of imitation, firms may expect to capture a smaller portion of the returns to innovation (Aghion and Howitt 1992; Bloom, Schankerman, and Van Reenen 2013), leading them to invest less in innovation.

The implementation of EDGAR is well suited for empirical analysis for several reasons. First, the EDGAR system revolutionized the way financial statement users acquired and processed corporate disclosures (e.g., Chang, Hsiao, Ljungqvist, and Tseng 2022; Chang et al. 2023). Second, the EDGAR implementation did not *mandate* changes in the content, quantity, quality, or timing of financial reporting disclosures. Rather, it enhanced the dissemination of existing mandatory disclosures to a wider audience of potential competitors and lowered the cost of accessing such disclosures, thus differentiating EDGAR from many prior regulatory reforms. Third, the staggered implementation of EDGAR allows researchers to construct plausible counterfactuals of how firm and peer innovation would have evolved had the EDGAR-induced shifts in disclosure dissemination not occurred, reducing endogeneity concerns due to omitted variables or reverse causality.

Our study focuses on a sample of innovative firms with positive R&D expenses to assess the impact of EDGAR's implementation on innovation investment.[1] Our first key result is that the implementation of EDGAR led to a significant permanent decrease in innovation investment. Specifically, R&D spending over total assets decreased by 0.22 percentage points for EDGAR filers compared to firms yet to be required to file through the EDGAR platform, corresponding to an 11 percent decrease in R&D investment relative to its pre-EDGAR mean. The observed decline in R&D investment contrasts with Goldstein et al. (2023) findings that the EDGAR implementation increased capital expenditures, which are subject to a lower expected degree of knowledge spillover.[2] Our result is derived from estimates of average treatment effects on the treated (ATTs) using stacked difference-in-differences (DiD) specifications that include firm×cohort and quarter×cohort fixed effects (Gormley and Matsa 2011; Cengiz, Dube, Lindner, and Zipperer 2019; Baker, Larcker, and Wang 2022). We also find that the decline in innovation is robust to using patent applications as a measure of innovation output (rather than innovation input) and to modifying our empirical specification to account for the potential confounding (spill-in) effects from treated firms (e.g., Berg, Reisinger, and Streitz 2021; Huber 2023).

In our next set of tests, we attempt to determine the mechanism through which innovation declines following the implementation of EDGAR. We start by examining whether EDGAR increases competitor learning. We demonstrate that the adverse effects of EDGAR on innovation investment are 25–60 percent more severe for firms expected to have greater knowledge spillovers *ex post* (e.g., innovation leaders and firms that rely on secrecy to protect their inventions). Indeed, the impact of knowledge spillovers on the investment in innovation of such firms should be more substantial,

[1] We measure innovation investment by the ratio of R&D to total assets because the timing of R&D expenditures is closest to the initiation of innovation activity (Faleye, Kovacs, and Venkateswaran 2014).

[2] Innovation investment often results in assets (e.g., intangibles) that lack excludability, making it harder for firms to exert property rights on those assets (Haskel and Westlake 2017; Crouzet, Eberly, Eisfeldt, and Papanikolaou 2022).

as knowledge tends to propagate from firms at the forefront of technology to others (Akcigit and Ates 2023) and imitation costs are lower when disclosed information pertains to unpatented technologies.

In addition, we provide direct evidence of significant knowledge spillovers to the technology rivals of treated firms. Using spillover measures similar to Bloom et al. (2013), we demonstrate that the R&D expenditures of a treated firm's average technology rival increase by about 8 percent after the introduction of EDGAR, compared to their pre-EDGAR levels. Moreover, we show that these spillovers were pronounced for treated firms that were innovation leaders or that relied on secrecy to protect their inventions prior to the EDGAR implementation, further cementing the importance of the competitor learning channel.

Next, we consider whether the observed decline in innovation investment after the implementation of EDGAR is due to an improvement in innovation efficiency, where firms may achieve the same or better economic returns with less spending on innovation (e.g., Zhong 2018). For instance, easy and timely access to peer disclosures post-EDGAR may offer insights into technological efficiencies and cost-effective methods of producing analogous technologies. In a contemporaneous paper, Chawla (2023) documents lower R&D expenditures following EDGAR (as we do) but also documents higher patent citations and interprets such findings as consistent with improved innovation efficiency.[3] To distinguish between the competitor learning and innovation efficiency explanations, we examine whether firms' sales, market share, and pre-R&D profitability change following the implementation of EDGAR. We find that treated firms incur significantly lower sales and have smaller market shares after EDGAR.[4] Given these findings, the decrease in innovation investment appears more consistent with the competitor learning channel than with an increase in innovation efficiency.

As an extension, we examine whether firms alter the R&D narrative disclosures in their financial statements after the EDGAR implementation. Our tests show that firms curtail their R&D narrative disclosures after EDGAR was implemented. The findings could be interpreted as consistent with less R&D activity among treated firms or as an attempt by firms to obscure details about R&D activities to minimize knowledge spillovers to competitors.

We conclude by exploring EDGAR's aggregate and distributional innovation effects among public and private firms, although these analyses are more descriptive in nature due to limited data availability. First, we study the patent filings of both public and private firms and how they evolve around the EDGAR implementation. Second, we analyze industry-level R&D investment by both public and private entities during the EDGAR adoption period. Although we find that public firms' patent filings decline relative to private firms after the EDGAR implementation, we find suggestive but inconclusive evidence of negative aggregate effects. Collectively, when coupled with our main results, these tests provide consistent evidence of the negative effect of the EDGAR implementation on public firm innovation with a redistribution of innovative activity from public to private firms not directly subject to EDGAR disclosure requirements.

We contribute to the extant literature in two ways. First, our study adds to the literature on EDGAR's economic consequences. Although existing studies document several important benefits to investors and firms from the EDGAR implementation (e.g., Gao and Huang 2020; Chang et al. 2023; Lai et al. 2024), we are one of the first papers to document an important societal cost of the regulation, namely a reduction in corporate innovation. The effects are complex as we also document significant positive spillovers across companies, particularly technology rivals. We also complement the nascent literature on the real effects of EDGAR (e.g., Bird, Karolyi, Ruchti, and Truong 2021; Goldstein et al. 2023; Liu and Zhang 2023). Although we focus on innovation, our results contextualize some of the previous findings in these papers (e.g., lower R&D investment contributes to previously documented increased profitability post-EDGAR).

Second, our paper provides new evidence on the mixed literature regarding the publicness of financial statement (or technical) information and its effects on innovation (e.g., Acharya and Xu 2017; Brown and Martinsson 2019; Fetter, Steck, Timmins, and Wrenn 2020; Kim and Valentine 2021; Hegde, Herkenhoff, and Zhu 2023; Berger et al. 2024). By focusing on EDGAR, we can isolate the innovation-related effects of a major disclosure dissemination technology independent from new disclosure mandates (e.g., Dambra, Field, and Gustafson 2015; Fu, Kraft, Tian, Zhang, and Zuo 2020) or nonfinancial reporting regimes that are directly tied to innovative activity or patent protection (e.g., Kim and Valentine 2021). Our evidence that firm-level innovation expenses decline in the U.S. for newly reporting EDGAR firms complements Breuer et al. (2022), who find that mandating European firms to publicly disclose their financial statements reduces the number of innovative firms in an industry, but not aggregate R&D spending. An important difference between our studies is that Breuer et al. (2022) find a reallocation of innovation from small to large firms due to the

[3] We additionally document in our study that the average quality of innovation as measured by patent originality, generality, and tail citations increases significantly after a firm starts filing through EDGAR. As we discuss in Section VI, these results are in fact consistent with the competitor learning explanation.

[4] In unreported tests, we confirm Goldstein et al. (2023) finding that EDGAR leads to higher profitability (ROA), a result which we find is driven by the decline in R&D that we document herein. This is because the decline in R&D mechanically improves (short-term) profitability.

mandatory financial statement disclosures, whereas our analysis indicates that the implementation of EDGAR shifts innovation from large to small firms.[5]

II. ECONOMIC MECHANISMS

Although EDGAR is a transformational regulatory technology for the dissemination and processing of corporate disclosures, its *net* effect on corporate innovation is *ex ante* unclear.[6] This tension arises due to several countervailing forces. On the one hand, if the EDGAR implementation reduces information search costs, this could broaden a firm's investor base and reduce its cost of capital (Merton 1987; Dambra, Schonberger, and Wasley 2023). Prior research indeed documents that the broader information dissemination associated with the EDGAR introduction leads to a decrease in the cost of equity, and an increase in both the level of equity financing and corporate investment (Goldstein et al. 2023; Lai et al. 2024). Relating capital markets access to innovation, Acharya and Xu (2017) find that public firms in external finance dependent industries spend more on R&D and have better patent portfolios than their private counterparts. Brown and Martinsson (2019) further document that the availability of firm-specific information to corporate outsiders results in significantly higher rates of R&D and patenting. The effects are pronounced in industries that rely on external equity, suggesting that the reduction in information costs associated with arm's-length financing boosts corporate innovation. Altogether, these findings suggest that improved corporate information dissemination following EDGAR could allow constrained firms reliant on external finance to attract capital by making their positive NPV innovative projects more visible to investors and by reducing adverse selection in the issuance of securities.

EDGAR may also lead to the more efficient use of capital and higher innovative efficiency. Specifically, the wider dissemination of verifiable financial information on firm value and managerial actions via EDGAR may assist managers in better assessing returns on prospective projects, maintaining discipline during implementation, and ensuring that R&D capital is directed toward its optimal uses. For instance, Zhong (2018) argues that increased financial reporting transparency disciplines managers to ensure that R&D capital is allocated efficiently within a firm. Christensen, Floyd, Liu, and Maffett (2017) find that improved dissemination of mine safety information in SEC filings leads to an improvement in mine productivity, even though such information is already disclosed on a mine regulator's website. Separately, information on other firms' innovative activities acquired via improved dissemination can help firms identify worthwhile activities and avoid duplicative innovation (Chawla 2023; Furman, Nagler, and Watzinger 2021; Hegde et al. 2023). Higher innovative efficiency due to EDGAR would suggest that treated companies may be able to achieve the same or even higher innovative output using a lower level of R&D investment.

On the other hand, increased dissemination of financial disclosures may exacerbate the proprietary costs of such disclosures. Financial disclosures are an important source of competitive intelligence (Previts, Bricker, Robinson, and Young 1994; Rogers and Grant 1997; Leder 2003; Brown and Tucker 2011; Lehavy, Merkley, and Li 2011; Merkley 2014), which firms may use to calibrate investment decisions (Bernard, Blackburne, and Thornock 2020). Notably, facilitated dissemination via EDGAR allows competitors to glean information about their rivals at a lower cost (e.g., Blankespoor, deHaan, and Marinovic 2020). As result, firms' incentives to innovate decrease because the disclosing firms are likely to capture less of the rents from their inventions (Aghion and Howitt 1992; Bloom et al. 2013). In line with the higher proprietary costs of enhanced disclosure, Breuer et al. (2022) studies the European Union (EU) Accounting Directives regulation and an enforcement reform in Germany and documents that forcing firms to publicly disclose their financial statements reduces the total number of innovating firms in the industry but provides positive innovation spillovers to larger firms. In the context of the American Inventors Protection Act (AIPA), Kim and Valentine (2021) show that more quickly disseminated patent disclosures led to a decrease in innovation for firms with longer patent publication lags. Altogether, whether EDGAR hampers or enhances corporate innovation by amplifying the dissemination of financial reporting is an open empirical question.[7]

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5 Specifically, focusing on European regulations and an enforcement reform in Germany, Breuer et al. (2022) find that mandatory *disclosure* of financial statements decreases the number of firms engaged in innovation, particularly smaller firms, due to the proprietary costs imposed by such regulations. Aggregate innovation spending becomes concentrated among larger firms benefiting from information spillovers. In contrast, using the implementation of the SEC's EDGAR system in the U.S., our paper focuses on the corporate innovation impact of disclosure *dissemination* (holding mandated disclosure content, quantity, quality, and timing constant), whereby we document a negative innovation investment effect across EDGAR-treated firms. In contrast to Breuer et al. (2022), we find that innovative firm responses appear concentrated among larger firms.

6 In line with an ambiguous net impact, recent research examining the economy-wide consequences of reporting regulations reveals that mandatory reporting does not unequivocally enhance or hinder industry-level productivity growth (e.g., Breuer 2021).

7 The increased publicness of financial disclosures may also exacerbate agency frictions, as outside pressure induces managers to reduce innovation to either lower risk or achieve earnings expectations (e.g., Fu et al. 2020; Dambra and Gustafson 2021; Kraft, Vashishtha, and Venkatachalam 2018). However, we consider this theoretical prediction less compelling in our setting, as informational asymmetry gives rise to managerial myopia, and extant research suggests that EDGAR reduces information asymmetry (i.e., Goldstein et al. 2023; Lai et al. 2024). Indeed, we find no evidence that treated firms become more likely to just meet or beat analyst expectations (e.g., Bhojraj, Hribar, Picconi, and McInnis 2009) following the implementation of EDGAR. See Online Appendix, Table OA.1.

III. INSTITUTIONAL BACKGROUND, EMPIRICAL DESIGN, AND DATA

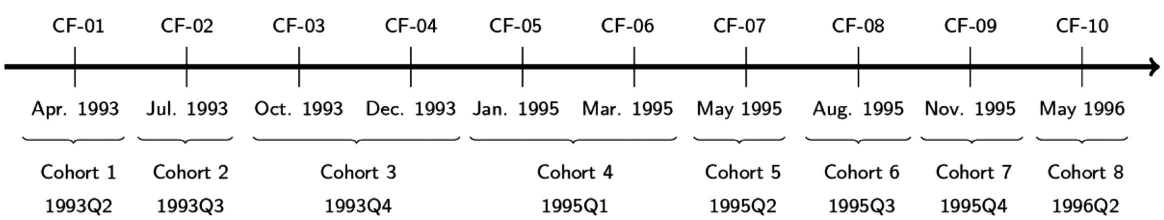
Institutional Background

The SEC introduced EDGAR in the second quarter of 1993 to make regulatory filings available electronically and increase public access to public companies' financial reporting. Initially, EDGAR's electronic filings were immediately accessible to electronic data stream subscribers and accessible overnight to electronic tape subscribers. In the first quarter of 1994, the data became freely accessible through the internet, first provided by New York University until the first quarter of 1996, and then by the SEC's online EDGAR platform since the second quarter of 1996 (Kambil and Ginsburg 1998).

Firms were subject to EDGAR according to a phase-in schedule that was random, conditional on firm size (Chang et al. 2023). The releases listed firms, their SEC Central Index Keys (CIKs), and one of ten groups (CF 01 to CF 10) determining the EDGAR reporting start date.[8] Compliance with phase-in groups was very high, as only 3 percent of firms participate in a phase-in group other than the group that they were assigned to by the SEC (Gao and Huang 2020).[9] Firms in the first group were required to become EDGAR filers in April 1993, whereas firms in the last group became EDGAR filers in May 1996. The time gap between the start dates of each group ranged from three to six months. Since most of our data are at the quarterly frequency, we aggregate the ten phase-in groups into eight cohorts based on the quarter a firm was required to file through EDGAR. Figure 1 maps the ten groups to our quarterly cohorts.

EDGAR centralized information access, boosting information disclosure dissemination and lowering information search costs. Before EDGAR, accessing firm filings was a time-consuming and costly process (Rider 2000; Gao and Huang 2020; Chang et al. 2023). Public companies were required to send multiple hard copies of their filings to the SEC through mail, courier services, or personal delivery. To access those filings, investors could visit one of the SEC's public reference rooms where companies' paper filings were deposited and accessible by the public after SEC examination. Due to the limitation of paper copies available, usually one or two copies per location, and the fact that only one person could inspect them at a time, disseminating the information to a wider audience was challenging.[10] Adding to the predicament, the maintenance of such a massive collection of paper filings proved difficult, resulting in files being misplaced or even stolen. To avoid these problems, financial statement users sometimes obtained electronic copies of corporate filings via commercial intermediaries (e.g., Asthana and Balsam 2001; Badertscher, Shroff, and White 2013). Prior research suggests that commercial access was "quite costly" and used only by the largest institutional investors (Chang et al. 2023).[11]

FIGURE 1
EDGAR Implementation Timeline



This figure shows a timeline of the staggered implementation of EDGAR.

8 Our use of *ex ante* phase-in schedules mitigates any potential effects of initial public offerings (IPOs) on our results (Bernstein 2015). To address this issue more directly, we show that our results are robust to excluding companies that were not publicly listed as of 1990Q2, the first quarter of our estimation sample. See Online Appendix, Table OA.2.

9 The SEC could approve or reject a firm's request to join a phase-in group different from the one assigned in the schedule. Due to the potential endogeneity of a firm's decision to alter the implementation timing, we follow Gao and Huang (2020) and rely on the prespecified timing rather than the actual implementation timing. To the extent that the actual implementation date of a firm is different from that specified in the phase-in schedule, this difference will lead to misclassifications in our coding and bias against finding significant results.

10 See also Noble (1982) and *The New York Times* (1994).

11 Investors who owned shares on the record date could wait to receive a copy of the company's annual report in the mail (Chang et al. 2023). Our private conversations with practitioners additionally indicate that such annual reports were more broadly available to the public through companies' public relations departments. Annual reports, however, are usually less comprehensive and detailed than 10-Ks (e.g., Asthana and Balsam 2001; Chang et al. 2023), particularly regarding information on various risks and discussions of operations.

Private conversations with practitioners reinforced some of the difficulties encountered in acquiring financial reports prior to EDGAR, including the significant delays associated with information acquisition in some cases. Moreover, they indicated that the processing of financial reports was challenging for companies before EDGAR. At larger firms, the financial statements of competitors were converted into internal databases. The manual transposition of this information was time-consuming and resulted in expositional errors, which made the information less useful. At smaller firms, competitor financial statements were stored in drawers or within the finance department and were not easily accessible to the different teams, as they were needed. One practitioner noted that financial statement analysis was incredibly time-consuming—information could not be compared on a timely basis and even revenue comparisons were difficult. On the other hand, after EDGAR, electronic filings allowed individuals to easily process information using features like search functions to swiftly locate specific details within electronic documents.

Empirical Design

A ubiquitous challenge in studies centered around changes in firms' information environments is that such changes tend to be endogenous.[12] Our study uses the staggered implementation of EDGAR to overcome this challenge. Most of our sample firms serve as both treatment and control firms. For instance, when firms in Cohort 1 transitioned from non-EDGAR filers to EDGAR filers in 1993Q2, the firms in Cohorts 2 through 8 acted as control groups. Similarly, when firms in Cohort 2 became subject to EDGAR in 1993Q3, the firms in Cohorts 3 through 8 served as control groups. This staggered approach helps alleviate concerns that the phase-in schedule may coincide with other firm-level events that could simultaneously impact corporate innovation and information search costs of corporate outsiders. In other words, for an omitted variable to explain our findings, it would need to affect specific groups of companies at distinct time points as specified in the phase-in schedule. Additionally, it is extremely unlikely that the phase-in schedule was designed by the SEC to anticipate changes in corporate innovation (up to three years in the future), which also renders reverse-causality explanations implausible.

We are careful in the implementation of our staggered setup. A growing econometrics literature highlights issues with using the traditional difference-in-differences estimator in staggered settings with varying treatment timing, particularly when all units are eventually treated. The ATTs estimated by the traditional approach can be biased because it is a weighted average of different treatment effects, including effects from newly treated units compared to already treated units whose treatment status does not change (Goodman-Bacon 2021). To address this, we implement a "stacked regression" approach (Gormley and Matsa 2011; Cengiz et al. 2019; Deshpande and Li 2019; Baker et al. 2022) that estimates ATTs within each EDGAR implementation cohort. Other studies within the EDGAR literature have used the stacked regression DiD approach as well (e.g., Chang et al. 2022, 2023; Lai et al. 2024).

The stacked regression approach obtains clean counterfactuals by stacking cohort-specific datasets to avoid contamination from comparing newly treated firms to those already treated. Specifically, each dataset is a firm-quarter panel comprised of treated firms in the cohort and those not yet treated, where the period of treatment is at most 11 quarters after the cohort's EDGAR start date.[13] This creates a "clean 2 × 2" dataset for each cohort (Baker et al. 2022). For each clean 2 × 2 dataset, we create a dataset-specific identifying variable (e.g., cohort *j*). The event-specific datasets are then "stacked" together.[14] The baseline stacked regression specification is:

$$Y_{it} = \beta_1 Treated_i^j \times Post\ EDGAR_t^j + \alpha_{ij} + \lambda_{tj} + \epsilon_{it} \quad (1)$$

where *i* denotes firms, *j* cohorts, and *t* quarters. Y_{it} measures innovation investment, output, quality, or narrative disclosure. The innovation investment of a firm is measured as its R&D expenses scaled by total assets (and multiplied by 100).[15,16]

[12] For example, IPOs are commonly used as information shocks, but the decision to go public depends on firm characteristics and economic conditions (Ibbotson and Jaffe 1975; Ritter 1984; Pagano, Panetta, and Zingales 1998).

[13] The treatment window length for a cohort equals the difference between the quarter in which the last cohort is treated and the quarter in which the given cohort is treated. The maximum window length is therefore 11 quarters, the difference between 1996Q1 (the quarter before the last cohort is treated) and 1993Q2 (when the first cohort is treated).

[14] Since this approach requires the inclusion of firms not yet treated as the control group in each cohort, it can only estimate ATTs for the first seven cohorts as all firms are treated by the time Cohort 8 is included in EDGAR.

[15] Ortiz, Peter, Urzúa, and Volpin (2023) find that M&A activity increases with higher mandatory financial disclosure. In our context, this raises the concern that the ratio of R&D to total assets may decrease post EDGAR if firms become larger due to intensified growth through acquisitions. In Online Appendix, Table OA.3, we show that our results are robust to eliminating the effect of changing total assets by either using logarithm-transformed R&D expenses or using the ratio of R&D spending to total assets, while holding firms' assets at pre-EDGAR levels. In scaling R&D spending by total assets, we follow a significant body of prior literature (e.g., Brown, Fazzari, and Petersen 2009; Faleye et al. 2014; Koh and Reeb 2015; Brown and Martinsson 2019; Dambra and Gustafson 2021; Chen, Kim, Yang, and Zhang 2023 among others). Our results are also robust using sales instead of total assets as a scaler for R&D spending. See Online Appendix, Table OA.4.

[16] In some of our tests, we also measure the innovation investment by technology rivals of a firm (i.e., innovation investment spillovers), defined as the average R&D expenses by technology rivals as identified in Bloom et al. (2013), scaled by the total assets of the focal firm (see Section V for further details).

Innovation output is measured as the number of patents filed in a quarter, or as the number of patents filed in a quarter scaled by the firm's total assets. Innovation quality is measured by the scaled originality and generality of a firm's patents as in Trajtenberg, Jaffe, and Henderson (1997) or alternatively as the percentage of a firm's patents that are in the top 10 percent of citations among all patents applied for in a year as in Celik and Tian (2023) (see Section VI for further details). Narrative disclosure is measured by counting the proportion of words in a firm's filing containing the list of R&D keywords in Merkley (2014).

Since the estimations are performed using a stacked dataset across cohorts, the $Treated_i^j$ and $Post\ EDGAR_t^j$ indicator variables are cohort-specific. $Treated_i^j \times Post\ EDGAR_t^j$ equals 1 for treated firms in cohort *j* from the quarter cohort *j* files through EDGAR until the first calendar quarter of 1996.[17] In our tests, the coefficient of interest in Equation (1) is β_1. To the extent that EDGAR mitigates asymmetric information regarding treated firms' innovation projects and lowers their costs of capital (i.e., Merton 1987), we would expect β_1 to be positive. Alternatively, to the extent that EDGAR lowers firms' returns on innovation (i.e., Aghion and Howitt 1992), we would expect β_1 to be negative.[18]

We include firm-cohort (α_{ij}) and quarter-cohort (λ_{tj}) fixed effects, which control for firm- and time-invariant factors that may affect innovation investment and outcomes.[19] They also restrict the ATTs to be estimated within each cohort. Following Gao and Huang (2020), we do not include time-varying covariates in these regressions as they can partially capture treatment effects (i.e., mediate the relation between EDGAR adoption and innovation) and confound inference. We emphasize, however, that our results are robust to the inclusion of time-varying covariates (e.g., logarithm total assets, Tobin's q, book leverage, and cash as a proportion of total assets) as shown in Online Appendix, Table OA.6. We use two-dimensional clustering, at the firm and treatment status (within the different stacks of data of our stacked regression design) levels.

Chang et al. (2022, 2023) make two additional empirical observations in the context of EDGAR that merit consideration. First, the SEC assigned public firms to the EDGAR phase-in waves randomly conditional on firm size (i.e., the assignment is conditionally random), which suggests that accounting for differences in firm size across cohorts may impact inferences. Second, for research designs that control for firm size using matching (i.e., controlling for firm size nonparametrically), the last three EDGAR cohorts lack clean controls because of "bunching toward the end of the SEC's phase-in schedule." We show our results are robust to controlling for firm size both parametrically and through a matching approach, whereby in the latter approach we only include firms from Cohorts 1–5.[20]

Although not specific to the context of EDGAR, but important given the economic channels discussed in Section II (particularly, knowledge spillovers to technology competitors), another potential concern with the specification in Equation (1) is that cross-firm spillovers may lead to a bias in estimating the EDGAR implementation treatment effect. This concern arises from the fact that the treatment status of a particular firm can influence outcomes at other firms (e.g., Berg et al. 2021; Huber 2023). Our results are again robust to accounting for cross-firm spillovers by including the average treatment status of technologically proximate firms (to the focal firm in our regression specifications) and its interaction with our *Post EDGAR* indicator.

We lastly contextualize the relative strengths and weaknesses of our study's design and the corresponding implications for the interpretation of our results. Our design uses relatively short windows to capture variation in information dissemination around the EDGAR implementation. A strength of the design is that these relatively short windows reduce concerns about confounding factors. The short-window design, however, would be better suited for fast-moving outcome variables, unlike innovation, which may not change quickly or fully over short periods of time (e.g., Roychowdhury, Shroff, and Verdi 2019). Accordingly, our study may underestimate EDGAR's effect on innovation and cannot provide conclusive evidence on its long-run effects.

Sample Construction

We link the CIKs in the phase-in schedules discussed in Section III to Compustat and obtain an initial sample of 8,256 firms. We then merge quarterly accounting data from Compustat and exclude financial services (SIC codes 6000–6999) and

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[17] Following Gao and Huang (2020) and Lai et al. (2024), our *Post EDGAR* $\times$ *Treated* indicator is defined as equal to 1 if the firm-quarter is subject to mandatory filing through EDGAR. Our results are robust to following Chang et al. (2022, 2023) and recoding the treatment quarters for Cohorts 1–3 to be 1994Q1 instead of 1993Q2, 1993Q3, and 1993Q4, respectively. See Online Appendix, Table OA.5.

[18] A positive effect of EDGAR on innovation efficiency may be consistent with either a positive or a negative β_1. We examine whether EDGAR improves innovation efficiency in Section V.

[19] Given the inclusion of firm-cohort and quarter-cohort fixed effects, the individual $Treated_i^j$ and $Post\ EDGAR_t^j$ terms are omitted from Equation (1).

[20] More generally, our results are robust to excluding any single cohort. See Online Appendix, Table OA.7. We also find more pronounced results for the earlier cohorts. First, this observation is consistent with the notion of more knowledge spillovers from "first movers" as well as early cohorts including larger firms, which are more likely to be innovation leaders. As we show in Section V, innovation leaders respond to the EDGAR implementation more strongly. Second, this observation is also driven by the shorter windows of observation post EDGAR for later cohorts combined with slow-moving corporate innovation, which we discuss at the end of Section III.

TABLE 1

Summary Statistics

	n (1)	Mean (2)	Std. Dev. (3)	25th Pctl. (4)	Median (5)	75th Pctl. (6)
Book leverage	18,615	0.20	0.21	0.03	0.16	0.30
Cash/total assets	18,596	0.18	0.22	0.02	0.09	0.27
Fraction of tail citations	18,615	3.62	14.25	0.00	0.00	0.11
Ln(Assets)	18,615	4.37	2.22	2.79	4.04	5.71
Ln(Sales)	18,249	3.02	2.42	1.42	2.92	4.54
Market share	18,551	0.17	0.30	0.00	0.02	0.17
Narrative R&D disclosure quantity (Entire filing)	8,766	0.12	0.16	0.00	0.05	0.16
Narrative R&D disclosure quantity (MD&A)	8,766	0.13	0.20	0.00	0.05	0.21
Number of patents	18,615	188.76	858.85	0.00	0.00	3.00
Pre-R&D ROA	15,975	0.04	0.05	0.02	0.04	0.06
R&D/total assets (%)	18,615	2.01	3.77	0.00	0.81	2.75
Scaled number of patents	18,615	3.26	33.65	0.00	0.00	0.01
SG&A/total assets (%)	18,615	7.34	6.03	3.16	6.30	10.01
Tobin's Q	18,615	2.18	2.03	1.05	1.45	2.40
Scaled generality	5,393	2.14	15.06	0.02	0.13	0.88
Scaled originality	5,383	2.38	21.74	0.03	0.18	1.08
Secrecy	18,615	0.23	0.42	0.00	0.00	0.00

This table reports summary statistics for the main variables used in the analysis during the pre-EDGAR period. The sample is a panel of firm-quarter observations of 1,732 innovative firms from 1990Q2 to 1993Q1.
Appendix A contains detailed variable definitions.

utilities (SIC codes 4900–4999) firms, which results in a sample of 3,265 firms from 1985Q1 to 2005Q4. Finally, we restrict our sample to innovative firms, defined as firms with positive R&D investment between 1985Q1 and 2005Q4.[21] This yields a final sample of 1,732 firms. Missing quarterly R&D values are set to 0.[22] All continuous variables are Winsorized at the 1 percent level.

Table 1 presents descriptive statistics for the 1,732 innovative firms during the three-year pre-EDGAR period, from 1990Q2 to 1993Q1. During that period, sample firms filed 189 patents and spent 2 percent of their assets on R&D, on average. The medians of each variable are 0 and 0.81 percent, respectively, indicating that the distribution of firm innovation is right skewed. We also report a correlation matrix and summary statistics for treated and control firms within each EDGAR implementation cohort separately in Online Appendix, Table OA.11. Given that the phase-in schedules were randomized conditional on firm size, it is not surprising that there is a size difference between treated and control firms in the first two cohorts. For instance, the average of the logarithm of assets is 8.4 for treated firms in Cohort 1 and 4.2 for control firms. Size differences become much smaller in the rest of the cohorts. We emphasize that there is no conceptual or theoretical reason to expect that these size differences across cohorts drive our results. We also empirically show this is not the case in our subsequent analyses.

IV. HOW DID EDGAR AFFECT INNOVATION INVESTMENT?

We begin by estimating the impact of EDGAR on firm innovation investment using Equation (1). The results presented in Table 2, column (1) indicate that EDGAR led to a 0.22 percentage point decrease in R&D over assets,

[21] Our results are robust to alternative definitions of innovative firms based exclusively on the pre-EDGAR period. In Online Appendix, Table OA.8, we report results where we restrict our sample to innovative firms, defined as firms with positive R&D investment between 1985Q1 and 1992Q2.

[22] One limitation of using reported R&D information is that it potentially conflates disclosure and real decisions. Koh and Reeb (2015) note that about 10 percent of firms that report missing R&D file and receive patents (with patent filings analogous to the bottom 90–95 percent of the positive R&D population). In Online Appendix, Table OA.9, we show that our results are robust to excluding missing R&D observations. We further show that our results are similar when we deploy alternative innovation outcome measures, such as patents in Table 6 and selling, general, and administrative expenses in Online Appendix, Table OA.10.

TABLE 2

Innovation around the Implementation of EDGAR

	R&D/total assets (%)					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Treated × Post EDGAR	−0.221***	−0.359***	−0.221***	−0.239***	−0.368***	−0.232***
	(2.914)	(3.416)	(3.448)	(3.080)	(3.506)	(3.520)
Ln(Assets) × Post EDGAR		0.030***			0.029***	
		(2.851)			(2.772)	
Treated rivals × Post EDGAR				0.008***	0.007**	0.006*
				(2.620)	(1.964)	(1.950)
Fixed effects						
Firm × cohort	Yes	Yes	Yes	Yes	Yes	Yes
Quarter × cohort	Yes	Yes	Yes	Yes	Yes	Yes
Observations	127,313	126,254	47,595	127,313	126,254	47,595
Adjusted R^2	0.60	0.62	0.62	0.60	0.60	0.62

***, **, * Indicate statistical significance at the 1, 5, and 10 percent levels, respectively.
The table reports average treatment effect (ATT) estimates of the staggered implementation of EDGAR on firm innovation investment using stacked difference-in-differences regressions (Gormley and Matsa 2011; Cengiz et al. 2019; Deshpande and Li 2019; Baker et al. 2022). The sample is a panel of firm-cohort-quarter observations of 1,732 innovative firms from 1990Q2 to 1996Q1. *R&D/total assets (%)* is research and development expenses divided by total assets and multiplied by 100. *Treated × Post EDGAR* is an indicator variable that equals 1 from the quarter a firm's cohort is required to file electronically in EDGAR until the end of the sample and 0 otherwise. *Ln(Assets)* is measured as the three-year average from 1990Q2 to 1993Q1. *Treated rivals* is the weighted average number of a firm's treated technology rivals within a cohort. The weights are proportional to the technology distance between firms, calculated as in Bloom et al. (2013). In columns (3) and (6), treated and control firms are balanced using nearest neighbor matching on the logarithm of assets in the pre-EDGAR period. All specifications control for firm × cohort and quarter × cohort fixed effects. t-statistics based on standard errors clustered at the firm and treatment status × cohort levels are shown in parentheses below the coefficient estimates.
Appendix A contains detailed variable definitions.

corresponding to an 11 percent decrease relative to the pre-EDGAR mean of the ratio, suggesting that the EDGAR implementation led to a reduction in corporate innovation investment. The reduction in innovation investment indicates that the increase in either proprietary disclosure costs or innovation efficiency overwhelmed the positive effect of lower cost of capital resulting from the EDGAR implementation (Goldstein et al. 2023; Lai et al. 2024).

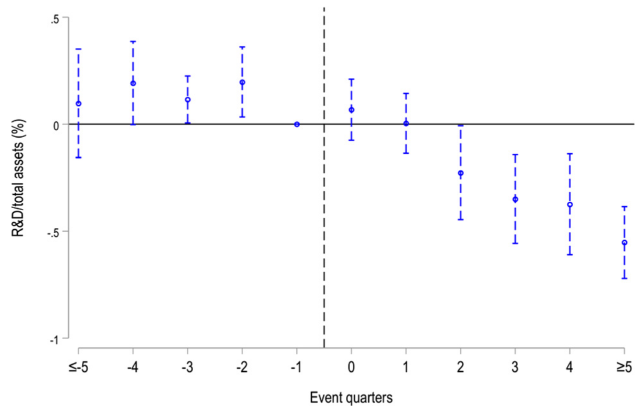
Figure 2, Panel A shows an event-study graph of the specification from Table 2, column (1), with the variable *Treated* interacted with indicator variables for event-quarters −4 to 4, excluding event quarter 0, and jointly for event quarters −11 to −5 and 5 to 11. We set the immediate pre-implementation observation as the base quarter in our parallel trends figure (e.g., Chang, Dambra, Schonberger, and Suk 2023). Although the estimated interaction terms are slightly positive and significant in two of the pre-EDGAR quarters, they are jointly insignificant across all the pre-EDGAR quarters (p-value of 0.112), suggesting that differences between treated and control firms are small in the pre-implementation period. This lends support to the parallel trends assumption implicit in our DiD approach (i.e., the absence of diverging trends in the post-treatment period had EDGAR not been implemented). Specifically, the absence of diverging pre-trends provides comfort that, absent the EDGAR adoption, treated and control firms would have invested in R&D similarly. Furthermore, our findings here alleviate concerns of strategic investment by treated firms, such as increasing R&D investment in the period running up to the EDGAR implementation.

Figure 2, Panel A also shows that the post-EDGAR innovation effects are not immediate but appear to materialize within two to three quarters after the EDGAR implementation, with the magnitude of the effect increasing with time elapsed after EDGAR. This observation is consistent with our discussion in Section III about the relative strengths and weaknesses of our empirical design specifically noting that innovation is slow moving in nature. The fact that the innovation effects become statistically significant within two to three quarters, however, also suggests that our results are not driven exclusively by the long end of our treatment windows.[23]

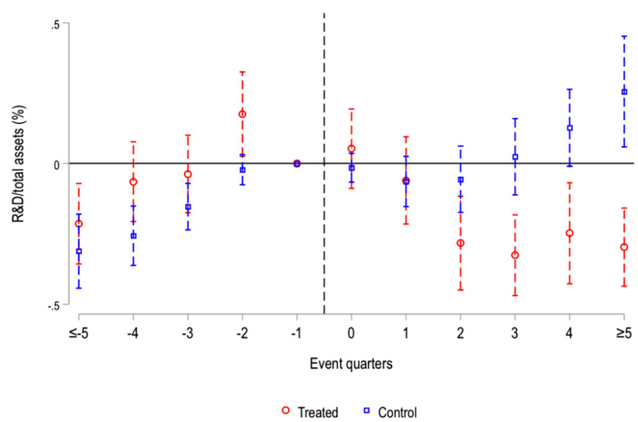
[23] In Table OA.12 of the Online Appendix, we replicate our main results by (1) restricting our maximum treatment window to eight quarters, (2) restricting our treatment window to exactly eight quarters (which restricts our sample to the first three cohorts), and (3) setting the treatment window to be exactly eight quarters, but dropping the first two treatment quarters. Our results remain robust to these alternative specifications.

FIGURE 2
The Evolution of Innovation Investment

Panel A: Dynamic Treatment Effects



Panel B: Treatment Effects Decomposition



Panel A plots the β coefficient estimates and 95 percent confidence intervals from the following regression: $R\&D/total\ assets_{it} = \sum_{t=-s}^{S} \gamma_t Event\ quarter_t^j + \sum_{t=-S}^{S} \beta_t Treated_i^j \times Event\ quarter_t^j + \alpha_{ij} + \epsilon_{it}$, where i indexes firms, j EDGAR cohorts, and t quarters. The β coefficients capture the average treatment effects across the EDGAR implementation cohorts each event quarter between $s = -4$ and $s = 4$ and jointly for event quarters $-11 \leq s \leq -5$ and $5 \leq s \leq 11$. $Treated_i^j$ is an indicator variable that equals 1 if firm i in cohort j is treated and 0 otherwise. Panel B decomposes the evolution of innovation investment among treated and control firms by plotting the γ coefficients of the following regressions separately for each of those groups: $R\&D/total\ assets_{it} = \sum_{t=-s}^{S} \gamma_t Event\ quarter_t^j + \alpha_{ij} + \epsilon_{it}$. In both panels, $Event\ quarter_t^j$ is a vector of event-time indicator variables for the period around cohort j's requirement to file electronically through EDGAR. α_{ij} denotes firm×cohort fixed effects. In Panel A, standard errors are clustered at the firm and treatment status × cohort levels. In Panel B, standard errors are clustered at the firm level. The dashed vertical line represents the mid-point between the pre- and post-treatment period.
(The full-color version is available online.)

Figure 2, Panel B plots the impact of EDGAR on treated and control firms' innovation investment separately. This decomposition reveals that EDGAR's impact on innovation is driven by two forces: a decrease in innovation investment by treated firms and an increase by control firms. Innovation spillovers are a crucial component of our competitor learning explanation for the decrease in innovation investment around the introduction of EDGAR. Although Panel B suggests that the innovation related effects of the EDGAR implementation are more related to proprietary costs than alternative explanations, we delve deeper into innovation spillovers in Section V.

Although the specification in Table 2, column (1) controls for differences in time-invariant characteristics between treated and control firms, one may still be concerned about size differences between treated and control firms in the first two cohorts. For instance, it may be the case that innovation investment at treated and control firms evolved differently around the introduction of EDGAR because of differences in *ex ante* firm size. We address this potential issue in two ways. First, in column (2), we augment Equation (1) by including an interaction between firm size measured as the three-year average logarithm total assets before the EDGAR introduction and *Post EDGAR* as a control.[24] Second, in column (3), we match treatment and control firms on total assets (specifically, for each firm that switches from being a nonfiler to an EDGAR filer in a quarter, we identify a nonswitching firm that has statistically similar size) excluding the last three cohorts from our estimation per our discussion in Section III. In both cases, the estimated coefficients on *Treated* × *Post EDGAR* remain directionally similar and statistically significant, indicating that *ex ante* differences in firm size are unlikely to explain our results. Our results are similarly robust to accounting for additional *ex ante* characteristics–see Online Appendix, Table OA.6.[25]

To further cement the idea that the parallel trends assumption is likely to hold in our setting, we conclude this section with a falsification test using a period preceding the actual EDGAR implementation. We find insignificant changes in innovation investment during this period, solidifying the evidence that our estimated treatment effects are not spurious. To conserve space, we report these results in Online Appendix, Table OA.14.

V. ECONOMIC MECHANISMS AND ALTERNATIVE EXPLANATIONS

Our results in Section IV suggest that a company's inclusion in EDGAR led to a significant decline in innovation investment. In this section, we attempt to isolate the economic mechanism through which EDGAR reduces corporate innovation investment.

Competitor Learning

The competitor learning channel (i.e., proprietary cost of disclosure) posits that the economic returns from innovation are significantly influenced by knowledge spillovers. To the extent that EDGAR made it easier for competitors to learn about incumbent firms' proprietary R&D strategies, it could have either facilitated imitation among technology rivals or promoted entry into a technology space by reducing uncertainty about profitable investment opportunities (Dedman and Lennox 2009). An implication of this explanation is that EDGAR's effect on innovation investment is likely to be greater for firms susceptible to larger knowledge spillovers. To test this prediction, we interact the *Treated* × *Post EDGAR* indicator with different measures of the expected potential knowledge spillovers following EDGAR's introduction. We first consider a firm to be more susceptible to knowledge spillovers if it is an innovation leader or produces high-quality innovations. The diffusion of knowledge resulting from EDGAR should benefit competitors more when it pertains to a firm at the forefront of technology, or when knowledge diffuses from the "best" to the "rest" (Akcigit and Ates 2023). We therefore define two indicator variables: *Innovation leader* and *Innovation quality*, which take a value of 1 if a firm has an above-median average share of patents granted or patent citations, respectively, within a technology class in the three years before a cohort's inclusion in EDGAR and 0 otherwise.

Firms may safeguard their innovations using formal intellectual property such as patents and trademarks, or choose to rely on secrecy (Hall, Helmers, Rogers, and Sena 2014). Using secrecy effectively increases competitors' imitation costs. Therefore, we consider a firm to be more vulnerable to knowledge spillovers if it relies on secrecy to safeguard its innovation, as the *ex post* diffusion of innovation-relevant information should lead to lower imitation costs. We define *Secrecy* as an indicator variable that equals 1 if a firm is in an industry that tends to rely on trade secrets to protect their innovations (Cohen, Nelson, and Walsh 2000), and 0 otherwise.

Table 3 presents results of regressions analogous to those in column (1) of Table 2 but including interactions between *Treated* × *Post EDGAR* and Innovation leader, Innovation quality, and Secrecy (we include all appropriate double interactions throughout but omit to report their coefficients for brevity). In all instances, the coefficient estimates for the interaction terms are negative and statistically significant at conventional levels. Furthermore, these estimates suggest that the negative impact of EDGAR on innovation investment is roughly 50 percent greater for firms that were *ex ante* innovation leaders, 60 percent higher for firms producing high-quality innovations, and 25 percent more severe for firms relying on secrecy to protect their inventions. These findings support the learning-based explanation for the reduction in innovation investment.

Finally, we test the learning hypothesis more directly by estimating the innovation spillovers that accrue to a firm's technology competitors after EDGAR. We follow a methodology similar to Bloom et al. (2013) to estimate these

[24] We use the three-year average to avoid short-term fluctuations in firm size.

[25] Breuer et al. (2022) document that reporting regulation has significant distributional effects on innovation across characteristics such as firm size. Motivated by this finding, we explore EDGAR's distributional effects on innovation across firm size as well as several additional firm characteristics —leverage, value, and cash. We find that larger firms' innovation investment declines significantly more post-EDGAR relative to smaller firms. In contrast, we do not find significant differential effects across the other firm dimensions. See Online Appendix, Table OA.13.

TABLE 3

Treatment Effect Heterogeneity by Innovation Leadership, Quality, and Secrecy

	R&D/total assets (%)		
	(1)	**(2)**	**(3)**
Treated × Post EDGAR	−0.243***	−0.224***	−0.205***
	(2.923)	(3.105)	(2.872)
Treated × Post EDGAR × Innovation leader	−0.121***		
	(2.978)		
Treated × Post EDGAR × Innovation quality		−0.135**	
		(2.479)	
Treated × Post EDGAR × Innovation secrecy			−0.052**
			(2.451)
Fixed effects			
Firm × cohort	Yes	Yes	Yes
Quarter × cohort	Yes	Yes	Yes
Observations	127,313	127,313	127,313
Adjusted R^2	0.60	0.60	0.60

***, **, * Indicate statistical significance at the 1, 5, and 10 percent levels, respectively.
The table reports interactions between average treatment effect (ATT) estimates of the staggered implementation of EDGAR on firm innovation using stacked difference-in-differences regressions (Gormley and Matsa 2011; Cengiz et al. 2019; Deshpande and Li 2019; Baker et al. 2022) and *ex ante* measures of innovation leadership, quality, and secrecy. The sample is a panel of firm-cohort-quarter observations of 1,732 innovative firms from 1990Q2 to 1996Q1. *R&D/total assets (%)* is research and development expenses divided by total assets and multiplied by 100. *Treated × Post EDGAR* is an indicator variable that equals 1 from the quarter a firm's cohort is required to file electronically in EDGAR until the end of the sample and 0 otherwise. *Innovation leader* is an indicator variable that equals 1 for firms with an above-median average share of patents granted within a technology class in the three years before a cohort's inclusion in EDGAR and 0 otherwise. *Innovation quality* is an indicator variable that equals 1 for firms with an above-median average share of patent citations within a technology class in the three years before a cohort's inclusion in EDGAR and 0 otherwise. *Innovation secrecy* is an indicator variable that equals 1 for firms in industries that rely on trade secrets to protect their innovations and 0 otherwise. All columns control for firm × cohort and quarter × cohort fixed effects. Interaction terms of *Innovation leader*, *Innovation quality*, and *Innovation secrecy* with *Post EDGAR* are included but omitted from the table for brevity. Interaction terms of *Innovation leader*, *Innovation quality*, and *Innovation secrecy* with *Treated* are subsumed by the firm × cohort fixed effects. t-statistics based on standard errors clustered at the firm and treatment status × cohort levels are shown in parentheses below the coefficient estimates.
Appendix A contains detailed variable definitions.

spillovers. First, we compute the average share of patents firms hold in each of the 426 United States Patent and Trademark Office (USPTO) technology classes from 1970Q1 to 1999Q4 and create the vector $T_i = (T_{i1}, T_{i2}, \ldots, T_{i426})$. Here, $T_{i\tau}$ represents the average share of patents of firm i in technology class τ over the given period. We then determine the technological distance between a pair of firms i and j, as described in Jaffe (1986), by computing the matrix $\text{Tech}_{ij} = [(T_i T_j')]/[(T_i T_i')^{\frac{1}{2}}(T_j T_j')^{\frac{1}{2}}]$. Finally, we estimate spillovers as Tech $\text{spillovers}_{ij} = \sum_{i \neq j} W_{ij} R\&D_{jt}$, where $R\&D_{jt}$ is the R&D investment of firm j in quarter t and $W_{it} = \text{Tech}_{ij} / \sum_i \text{Tech}_{ij}$.[26] We scale the spillover measure by the total assets of the focal EDGAR-treated firm. Table 4, Panel A presents results of regression specifications that estimate the impact of EDGAR on technology spillovers.

The positive significant coefficient of *Treated × Post EDGAR* in column (1) suggests that a firm's inclusion in EDGAR led to substantial R&D spillovers to its technology competitors. On average, the R&D investments of technology rivals increased by 8 percent relative to their pre-EDGAR average. This effect is economically large and provides evidence that spillovers to technology rivals constitute an important channel through which the implementation of EDGAR resulted in innovation investment reductions.

Table 4, Panel A also investigates the heterogeneity of the spillover effects based on innovation leadership, quality, and the use of secrecy as in Table 3. We interact the *Treated × Post EDGAR* indicator with the Innovation leader, Innovation quality, and Secrecy indicators defined previously. The results in columns (2) to (4) show that the spillover effects are concentrated among the competitors of firms that were innovation leaders, produced highly cited patents, or

[26] Since our focus is on understanding the impact of EDGAR on a firm's rivals' innovation, we multiply the technological distance weights by the R&D of rival firms instead of the R&D stock used in Bloom et al. (2013).

TABLE 4

Knowledge Spillovers

Panel A: Spillovers to All Firms

	Average rival R&D/focal firm assets			
	(1)	**(2)**	**(3)**	**(4)**
Treated × Post EDGAR	0.423***	0.703***	0.714***	0.347***
	(9.598)	(3.590)	(6.972)	(2.972)
Treated × Post EDGAR × Innovation leader		0.312**		
		(2.234)		
Treated × Post EDGAR × Innovation quality			0.484***	
			(6.081)	
Treated × Post EDGAR × Innovation secrecy				0.324**
				(2.069)
Fixed effects				
Firm × cohort	Yes	Yes	Yes	Yes
Quarter × cohort	Yes	Yes	Yes	Yes
Observations	127,313	127,313	127,313	127,313
Adjusted R^2	0.88	0.88	0.88	0.88

Panel B: Spillovers to Treated Firms

	Average rival R&D/focal firm assets			
	(1)	**(2)**	**(3)**	**(4)**
Treated × Post EDGAR	0.170***	0.051	0.071	0.144**
	(3.194)	(1.098)	(1.577)	(2.547)
Treated × Post EDGAR × Innovation leader		0.198***		
		(3.046)		
Treated × Post EDGAR × Innovation quality			0.262***	
			(2.667)	
Treated × Post EDGAR × Innovation secrecy				0.099**
				(2.102)
Fixed effects				
Firm × cohort	Yes	Yes	Yes	Yes
Quarter × cohort	Yes	Yes	Yes	Yes
Observations	127,313	127,313	127,313	127,313
Adjusted R^2	0.72	0.72	0.72	0.72

Panel C: Spillovers to Control Firms

	Average rival R&D/focal firm assets			
	(1)	**(2)**	**(3)**	**(4)**
Treated × Post EDGAR	0.315***	0.555***	0.433***	0.095
	(9.834)	(6.951)	(9.289)	(0.900)
Treated × Post EDGAR × Innovation leader		0.807***		
		(2.582)		
Treated × Post EDGAR × Innovation quality			0.568***	
			(2.574)	
Treated × Post EDGAR × Innovation secrecy				0.438***
				(4.541)

(continued on next page)

TABLE 4 (continued)

	Average rival R&D/focal firm assets			
	(1)	**(2)**	**(3)**	**(4)**
Fixed effects				
Firm × cohort	Yes	Yes	Yes	Yes
Quarter × cohort	Yes	Yes	Yes	Yes
Observations	127,313	127,313	127,313	127,313
Adjusted R^2	0.86	0.86	0.86	0.85

***, **, * Indicate statistical significance at the 1, 5, and 10 percent levels, respectively.
The table reports average treatment effect (ATT) estimates of the staggered implementation of EDGAR on innovation spillovers to technology rivals using stacked difference-in-differences regressions (Gormley and Matsa 2011; Cengiz et al. 2019; Deshpande and Li 2019; Baker et al. 2022). The sample is a panel of firm-cohort-quarter observations of 1,732 innovative firms from 1990Q2 to 1996Q1. *Average rival R&D/focal firm assets* is the average R&D expenditures of technology rivals divided by the total assets of focal firms. Technological distances are calculated as in Bloom et al. (2013). Panel A examines spillovers to all rival firms. Panels B and C examine spillovers to treated and control firms, respectively. *Treated* × *Post EDGAR* is an indicator variable that equals 1 from the quarter a firm's cohort is required to file electronically in EDGAR until the end of the sample and 0 otherwise. In columns (2) to (4), *Treated* × *Post EDGAR* is interacted with proxies for *ex ante* innovation leadership, quality, and information asymmetry, respectively. *Innovation leader* is an indicator variable that equals 1 for firms with an above-median average share of patents granted within a technology class in the three years before a cohort's inclusion in EDGAR and 0 otherwise. *Innovation quality* is an indicator variable that equals 1 for firms with an above-median average share of patent citations within a technology class in the three years before a cohort's inclusion in EDGAR and 0 otherwise. *Innovation secrecy* is an indicator variable that equals 1 for firms in industries that rely on trade secrets to protect their innovations and 0 otherwise. All columns control for firm × cohort and quarter×cohort fixed effects. Interaction terms of *Innovation leader*, *Innovation quality*, and *Innovation secrecy* with *Post EDGAR* are included but omitted from the table for brevity. Interaction terms of *Innovation leader*, *Innovation quality*, and *Innovation secrecy* with *Treated* are subsumed by the firm×cohort fixed effects. t-statistics based on standard errors clustered at the firm and treatment status×cohort levels are shown in parentheses below the coefficient estimates.
Appendix A contains detailed variable definitions.

used secrecy to protect their inventions. These results provide further support for the learning hypothesis and complement recent evidence that increases in public disclosures can stimulate competitor firms to enhance their innovative investment (e.g., Breuer et al. 2022; Kim and Valentine 2023).

Table 4, Panels B and C decomposes spillovers to technological rivals according to whether the rival companies are treated firms (Panel B), or alternatively, control firms (Panel C). The results across all specifications indicate that spillovers are particularly pronounced when the rivals are control firms—i.e., not subject to EDGAR. This suggests that EDGAR reduces incentives to invest in innovation opportunities arising from knowledge spillovers from competitors when the firms are subject to the heightened proprietary cost of disclosure themselves.

Having documented the *de facto* existence of significant R&D investment spillovers to technology rivals raises an important concern with our baseline estimates in Table 2, columns (1)–(3). Specifically, recent methodological papers (Berg et al. 2021; Huber 2023) suggest that spillovers may lead to complicated biases specifically because the treatment status of a given firm can affect the innovation outcomes at other firms. In the extreme, we may find the seemingly negative direct innovation effect on companies filing through EDGAR merely because their technological rivals in the control sample reap benefits from knowledge spillovers, not because EDGAR-treated firms invest less in innovation.

Consequently, an immediate prediction from Berg et al. (2021) and Huber (2023) for our setting would be that the firm-level R&D investment would depend not only on a firm's own EDGAR treatment status, but also on the fraction of treated firms in the group of technologically proximate firms. To check the robustness of our results in Section IV, we modify Equation (1) by including in our regression model the interaction of the average treatment status of the technology rivals of a focal firm in each EDGAR cohort, *Treated rivals*, with *Post EDGAR*. Table 2, columns (4)–(6) show that accounting for the "indirect" spillover effect has minimal impact on the "direct" effect of the EDGAR implementation. Reassuringly, we also find evidence of significant spillover effects as suggested by the positive significant coefficient on *Treated rivals* × *Post EDGAR* consistent with our earlier results in this section.[27]

[27] Heath, Ringgenberg, Samadi, and Werner (2023) study the multiple hypothesis testing problem that arises when researchers reuse natural experiments. Based on our review of the literature, we count approximately 15 unique outcome variables from published studies. Table AI, Panel B of Heath et al. (2023) suggests a critical t-statistic of around 2.90 as the minimum for our baseline results. We acknowledge that there is subjectivity in this critical value as our count of outcome variables increases or decreases based upon whether we: (1) require a stacked DiD, (2) include working papers, and (3) include or exclude highly correlated outcome variables, like liquidity and volume. Based on our main results reported in Table 2, the t-statistics of *Treated* × *Post EDGAR* range from 2.914 to 3.520 with an average t-statistic of 3.314. These t-statistics are above the critical value of 2.90, suggesting that our baseline results survive the Heath et al. (2023) correction for multiple hypothesis testing. In Table OA.15 of the Online Appendix, we also show that our innovation results are robust to controlling for various outcome variables studied in prior literature.

Innovation Efficiency

One alternative interpretation for our results is that they reflect improved innovation efficiency where firms may achieve the same or better economic returns even with less spending on innovation (Zhong 2018; Chawla 2023). For instance, access to information about other firms' innovative activities could assist firms in identifying valuable innovation opportunities and avoiding duplicative innovation efforts. To distinguish between these explanations, we explore measures reflecting economic returns from innovation.

For these analyses, we focus on company sales, market share, and pre-R&D profitability measures. We measure profitability prior to R&D because the reduction in R&D spending would mechanically boost short-term profitability. We expect to observe lower returns (reductions in sales, market share, and pre-R&D profitability) if EDGAR dissipates gains from innovation. In contrast, we expect to find the opposite if EDGAR enhances innovation efficiency.

Table 5 presents the results of regression specifications similar to those shown in Table 2, column (1), but using as dependent variables logarithm transformed sales, *Ln(Sales)*, percent industry share of sales, *Market share*, and pre-R&D expense return on assets, *Pre-R&D ROA*. The estimated coefficients of *Treated* × *Post EDGAR* are negative and significant when considering *Ln(Sales)* and *Market share*, and negative but insignificant when considering *Pre-R&D ROA*. We thus find some evidence that firms' economic returns from the EDGAR implementation decreased and no evidence that they increased. We also show in Section VI that firms' innovation output significantly declined after EDGAR. Altogether, we argue that the improved innovation efficiency explanation of EDGAR's negative effect on corporate innovation investment is unlikely. Although interesting in themselves, our results are also important for contextualization of findings in the prior literature (e.g., Goldstein et al. 2023; Chawla 2023). Specifically, the improved profitability that these studies document appears to be driven by the reduction in innovation investment.

VI. ADDITIONAL ANALYSES

In Sections IV and V, we demonstrate that a firm's inclusion in EDGAR had a significant impact on innovation investment, and that learning by competitors is a key mechanism behind the effects we document. Under the competitor learning channel, information spillovers to rivals reduce the incentive for incumbent firms to invest in innovation, as the expected returns from such investments are lower. One implication of the learning channel is that lower investment in innovation by EDGAR firms should decrease their innovation output. Another implication is that EDGAR firms may adjust the composition of their innovation projects toward those with higher expected returns. We test these predictions in the current section. In addition, we explore whether EDGAR alters firms' discussion of R&D activities in their more

TABLE 5

Economic Returns

	Ln(Sales)	*Market share*	*Pre-R&D ROA*
	(1)	**(2)**	**(3)**
Treated × *Post EDGAR*	−0.138***	−0.003**	−0.001
	(5.521)	(2.260)	(0.178)
Fixed effects			
Firm × cohort	Yes	Yes	Yes
Quarter × cohort	Yes	Yes	Yes
Observations	126,987	126,731	109,395
Adjusted R^2	0.97	0.93	0.56

***, **, * Indicate statistical significance at the 1, 5, and 10 percent levels, respectively.
The table reports average treatment effect (ATT) estimates of the staggered implementation of EDGAR on firm operating and financial performance using stacked difference-in-differences regressions (Gormley and Matsa 2011; Cengiz et al. 2019; Deshpande and Li 2019; Baker et al. 2022). The sample is a panel of firm-cohort-quarter observations of 1,732 innovative firms from 1990Q2 to 1996Q1. *Ln(Sales)* is the logarithm of sales. *Market share* is a firm's share of total sales within its four-digit SIC code each quarter. *Pre-R&D ROA* is operating income before depreciation and R&D expenses divided by total assets. *Treated* × *Post EDGAR* is an indicator variable that equals 1 from the quarter a firm's cohort is required to file electronically in EDGAR until the end of the sample and 0 otherwise. All specifications control for firm × cohort and quarter × cohort fixed effects. t-statistics based on standard errors clustered at the firm and treatment status × cohort levels are shown in parentheses below the coefficient estimates.
Appendix A contains detailed variable definitions.

easily accessible financial reports. Finally, we offer some evidence on EDGAR's aggregate innovation effects as well as its impact on the distribution of innovative activity between public and private firms.

Innovation Outcomes

The outcome of many R&D projects is a patent that protects the intellectual property rights of the innovation. Consequently, due to the lower innovation investment after EDGAR, we expect companies' patent applications to decline, which we examine next. We follow Santos Silva and Tenreyro (2006) and Cohn, Liu, and Wardlaw (2022) and use two alternative specifications to account for the right skewness of patent application distribution and the large proportion of zero observations. First, we estimate Poisson pseudo-maximum likelihood (PPML) regressions of the number of patent applications. The PPML estimator can accommodate count data restricted to non-negative values and produce unbiased estimates even when there is a large proportion of zeroes (Santos Silva and Tenreyro 2011). Second, using OLS, we scale the number of patent applications by total assets because the skewness in the number of patent applications is partly driven by firm size.

Table 6 presents results of the impact of EDGAR on the number of patent applications. The results in column (1) imply a decrease in patent applications of 38 percent ($100 \times (e^{-0.479} - 1)$) after the EDGAR implementation. The results reported in column (2), where the dependent variable is patent applications over total assets, suggest a 9 percent decrease in patent applications over assets relative to before EDGAR. Overall, these results indicate that EDGAR led to a large decrease in patent applications.

Innovation Quality

The competitor learning channel suggests that knowledge spillovers to rivals reduce treated firms' expected returns to innovation. This leads firms to adjust the composition of their innovation portfolios post-EDGAR, and as a result, improves the average quality of innovation projects given that they will reduce investment in marginal innovative projects. In other words, in terms of a project's NPV, a higher threshold for innovation quality would exist to make a project profitable.

We next examine the impact of EDGAR on innovation quality. We use three distinct proxies of forward-looking innovation quality. The first two proxies are patent originality (*Scaled originality*) and generality (*Scaled generality*), which we construct following Trajtenberg et al. (1997). *Scaled originality* is calculated as 1 minus the Herfindahl-Hirschman index (HHI) of the forward citations of other patents made by a firm's patents across technology classes

TABLE 6

Innovation Outcomes

	Number of patents	*Number of patents/total assets*
	(1)	**(2)**
Treated × Post EDGAR	−0.479***	−0.007***
	(3.190)	(8.401)
Fixed effects		
Firm × cohort	Yes	Yes
Quarter × cohort	Yes	Yes
Observations	81,483	127,313
Adjusted R^2	0.95	0.62

***, **, * Indicate statistical significance at the 1, 5, and 10 percent levels, respectively.
The table reports average treatment effect (ATT) estimates of the staggered implementation of EDGAR on firm innovation outcomes using stacked difference-in-differences regressions (Gormley and Matsa 2011; Cengiz et al. 2019; Deshpande and Li 2019; Baker et al. 2022). The sample is a panel of firm-cohort-quarter observations of 1,732 innovative firms from 1990Q2 to 1996Q1. *Number of patents* is the number of patents filed in a quarter. *Number of patents/total assets* is the number of patents filed in a quarter divided by total assets. *Treated × Post EDGAR* is an indicator variable that equals 1 from the quarter a firm's cohort is required to file electronically in EDGAR until the end of the sample and 0 otherwise. The coefficients are from a PPML estimation in column (1) and an Ordinary Least Squares estimation in column (2), respectively. All specifications control for firm × cohort and quarter × cohort fixed effects. t-statistics based on standard errors clustered at the firm and treatment status × cohort levels are shown in parentheses below the coefficient estimates.
Appendix A contains detailed variable definitions.

TABLE 7

Innovation Quality

	Scaled originality	***Scaled generality***	***Tail citations***
	(1)	**(2)**	**(3)**
Treated × Post EDGAR	0.689***	0.835***	1.439**
	(2.930)	(3.949)	(2.076)
Fixed effects			
Firm × cohort	Yes	Yes	Yes
Quarter × cohort	Yes	Yes	Yes
Observations	29,792	29,425	31,199
Adjusted R^2	0.73	0.84	0.43

***, **, * Indicate statistical significance at the 1, 5, and 10 percent levels, respectively.
The table reports average treatment effect (ATT) estimates of the staggered implementation of EDGAR on measures of firm innovation quality using stacked difference-in-differences regressions (Gormley and Matsa 2011; Cengiz et al. 2019; Deshpande and Li 2019; Baker et al. 2022). The sample is a panel of firm-cohort-quarter observations of 1,155 innovative firms based on patents granted from 1990Q2 to 1996Q1 with available forward citations data until 2010Q4. *Scaled originality* is the HHI of cited patents across technology classes scaled by the average originality of all patents granted within a technology class-year (Trajtenberg et al. 1997). *Scaled generality* is the HHI index of citing patents across technology classes scaled by the average generality of all patents granted within a technology class-year (Trajtenberg et al. 1997). *Tail citations* is the percentage of a firm's patents that are in the top 10 percent of citations among all patents applied for in a year. *Treated × Post EDGAR* is an indicator variable that equals 1 from the quarter a firm's cohort is required to file electronically in EDGAR until the end of the sample and 0 otherwise. All columns control for firm × cohort and quarter × cohort fixed effects. t-statistics based on standard errors clustered at the firm and treatment status × cohort levels are shown in parentheses below the coefficient estimates.
Appendix A contains detailed variable definitions.

scaled by the average originality of all patents granted within a technology class-year. *Scaled generality* is calculated as 1 minus the HHI of forward citations of a firm's patents made by other patents across technology classes scaled by the average generality of all patents granted within a technology class-year.[28] Our last proxy for innovation quality, *Tail citations*, measures the likelihood of a very high number of citations. We follow Celik and Tian (2023) to calculate the fraction of a firm's patents in the top 10 percent of all cited patents each year.

Table 7 presents the results from the staggered DiD estimation of the impact of a firm's inclusion in EDGAR on the innovation quality measures. The results indicate that EDGAR inclusion led to an increase in patent originality and generality by 29 percent and 39 percent, respectively, compared to pre-EDGAR means. Additionally, the fraction of a company's patents that are in the top 10 percent of most cited patents increased by 30 percent after the EDGAR implementation relative to the pre-EDGAR mean. The shift in the quality composition of a firm's patent portfolio toward higher quality patents with potentially higher returns further supports and reinforces our competitor learning explanation that projects need to be above a certain hurdle rate for EDGAR reporting firms to engage in innovation investment.[29,30]

R&D Narrative Disclosure

The significant decline in corporate innovation, coupled with significant positive spillovers to technology rivals, supports the idea of significant proprietary costs of corporate filings imposed by the EDGAR implementation. Apart from the real effects, another way for firms to respond to these higher proprietary costs is to reduce their disclosure of R&D activities after EDGAR.

It is important to note that we do not expect narrative disclosure reductions to fully shield firms filing through EDGAR from proprietary costs due to information spillovers to competitors. Managers are unable to avoid mandatory

[28] These measures capture different aspects of quality. Original patents are patents that are not concentrated in a narrow set of technologies, and thus more likely to represent breakthroughs. General patents, on the other hand, are impactful patents since they influence innovations in a variety of technologies.

[29] The number of observations in these tests is lower than our baseline tests for two reasons. First, the tests require that firms maintain a portfolio of filed patents both before and after the EDGAR shock. Second, for each of those patents, we require forward citations data until 2010, so that we can construct the innovation quality measures.

[30] These results are also consistent with EDGAR leading to increased innovation efficiency (Chawla 2023). However, our tests in Section V provide evidence counter to an improvement in investment efficiency.

TABLE 8

R&D Narrative Disclosure

	Narrative R&D disclosure quantity	
	MD&A Section	**Entire Filing**
	(1)	**(2)**
Treated × Post EDGAR	−0.065**	−0.114***
	(2.285)	(3.092)
Fixed effects		
Firm × cohort	Yes	Yes
Year × cohort	Yes	Yes
Observations	30,734	34,450
Adjusted R^2	0.16	0.17

***, **, * Indicate statistical significance at the 1, 5, and 10 percent levels, respectively.
The table reports average treatment effect (ATT) estimates of the staggered implementation of EDGAR on R&D disclosure using stacked difference-in-differences regressions (Gormley and Matsa 2011; Cengiz et al. 2019; Deshpande and Li 2019; Baker et al. 2022). The sample is a panel of firm-cohort-year observations of 1,732 innovative firms from 1990 to 1996. *Narrative R&D disclosure quantity* is the fraction of R&D-related words in a firm's filing, where R&D-related words are identified using the list in Appendix A of Merkley (2014). Column (1) and (2) present results where the fraction of R&D-related words is calculated using the text in the MD&A section and the entire filing, respectively. *Treated × Post EDGAR* is an indicator variable that equals 1 from the year a firm's cohort is required to file electronically in EDGAR until the end of the sample and 0 otherwise. All specifications control for firm × cohort and year × cohort fixed effects. t-statistics based on standard errors clustered at the firm and treatment status × cohort levels are shown in parentheses below the coefficient estimates.
Appendix A contains detailed variable definitions.

components of innovation disclosure, such as material product developments (e.g., Aghamolla and Thakor 2022), FASB-required disclosures of R&D via SFAS 2 and 68 (Merkley 2014), and SEC-required disclosures under Regulation S-K. In addition, firms are also expected to continue to balance between mitigating proprietary costs imposed by EDGAR and exacerbating information asymmetry by reducing narrative innovation disclosures (Glaeser 2018; Kim, Su, Wang, and Wu 2021). Although it is possible that EDGAR induces a reduction in narrative R&D disclosure to counter the effects of enhanced disclosure dissemination, we consider it unlikely that firms wind up in a corner solution of no narrative R&D disclosure that eliminates any incremental proprietary costs due to EDGAR.

We leverage the Compact Disclosure database to obtain corporate filings before and after the EDGAR implementation. It includes extracted text and tables from 10-Ks. We utilize all Compact Disclosure data from 1990 to 1996 and identify companies by their CUSIP numbers provided in the database. In our analyses, we quantify R&D disclosure both based on the company full filing, and just the MD&A section.[31] Similar to Merkley (2014), we measure corporate R&D disclosure quantity by using a computerized content analysis of 10-K filings. We start with Merkley's dictionary of common R&D keywords and phrases. We tokenize the text into words, removing stop words and common phrases in each company's filing, or alternatively just the respective MD&A section. We finally measure R&D disclosure, *Narrative R&D disclosure quantity*, as the percentage of disclosure text that the R&D keywords comprise. In contrast to our main sample, we note these measures are at the annual level given that they are based on 10-Ks.

Using specifications similar to Equation (1) at the annual frequency, we test whether the EDGAR implementation impacted R&D narrative disclosure. Our results in Table 8 suggest that the implementation of EDGAR significantly changed firm R&D disclosures. The coefficient on *Treated × Post EDGAR* is negative and statistically significant, suggesting that R&D disclosure decreases after the EDGAR implementation. These findings support the notion of higher propriety cost of disclosure in the post-EDGAR regime either because they reflect lower R&D activity, or alternatively, because firms cut R&D narrative to decrease disclosure informativeness.

[31] Prior research suggests that the MD&A section provides managers with the opportunity to convey their future expectations and strategic plans and allows for the most managerial discretion and flexibility in choosing the breadth and depth of topics discussed (Brown and Tucker 2011). Consistent with this argument, Cohen, Malloy, and Nguyen (2020) show that firms' disclosure changes are concentrated in the MD&A section of 10-Ks.

TABLE 9

Aggregate Effects

	R&D (SIRD)				***Innovative firms (SIRD)***	
	R&D/sales			***Ln(R&D)***	***Count***	***Share***
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
EDGAR filers$_t$/total number of firms$_{1992}$	−5.631***	−6.144***	−1.194	−0.371	−0.287	−0.381
	(4.536)	(4.593)	(1.563)	(1.365)	(0.366)	(1.204)
Fixed effects						
Industry	No	No	Yes	Yes	Yes	Yes
Year	No	Yes	Yes	Yes	Yes	Yes
Observations	32	32	32	56	56	56
Adjusted/Pseudo R^2	0.15	0.11	0.98	0.99	0.95	0.74

***, **, * Indicate statistical significance at the 1, 5, and 10 percent levels, respectively.
The table reports results from industry-level regressions of aggregate R&D investment and number of innovative firms on the fraction of EDGAR filers within an industry. The sample is a panel of firm-year observations of 14 industries from 1993 to 1996 using data from the National Science Foundation's SIRD. *R&D/sales* is the total R&D expenses of an industry divided by its sales, as reported by SIRD. *Ln(R&D)* is the logarithm of R&D expenditures. Innovative firms are defined in SIRD as those with at least $1 million in R&D expenditures in a year. In computing the share of innovative firms in an industry, we divide the number of innovative firms in SIRD by the total number of firms with at least 100 employees using U.S. Census Bureau data. *EDGAR filers$_t$/total number of firms$_{1992}$* is the fraction of publicly listed firms in an industry filing through EDGAR in a year divided by the total number of firms with at least 100 employees in 1992. The coefficients are from Ordinary Least Squares estimations in columns (1) to (4) and PPML estimations in columns (5) and (6), respectively. t-statistics based on standard errors robust to heteroskedasticity are shown in parentheses below the coefficient estimates.
Appendix A contains detailed variable definitions.

Aggregate and Public versus Private Firm Distributional Effects

Our study has so far focused exclusively on EDGAR's effect on the innovation activities of public companies. Consequently, the aggregate and public versus private firm distributional effects of the EDGAR reform on corporate innovation are not clear. We provide suggestive evidence on these dimensions in this section. To provide evidence on the aggregate innovation effects of EDGAR, we start by conducting an industry-level analysis that should better account for public-private firm spillovers for firms operating in the same industry. In these tests, we examine the relation between industry-level R&D activity and the proportion of firms filing through the EDGAR platform. A significant positive association would imply a net positive aggregate effect, whereas a negative association would imply a net negative aggregate effect.

This analysis explicitly relies on cross-industry variation in the share of firms filing through EDGAR due to EDGAR's staggered implementation. For example, industries with larger (public) firms would exhibit a higher fraction of firms that are subject to EDGAR in the early periods of the EDGAR staggered roll-out. The main strength of this setting is that we can capture both direct and indirect effects (e.g., knowledge spillovers between public and private firms) of EDGAR at a high level of aggregation. Thus, we are more likely to estimate EDGAR's net impact on corporate innovation. We acknowledge, however, the limitations of this design due to the nonrandom assignment of firms across industries, potential omitted variables, and the omission of cross-industry spillovers.

Our tests utilize the National Science Foundation's Survey of Industrial Research and Development (SIRD), an annual sample survey that represents all for-profit R&D-performing companies, either publicly or privately held. The survey is completed by representatives at manufacturing and nonmanufacturing companies known to conduct R&D and by representatives from samples of companies in both sectors that may conduct R&D. SIRD provides key variables of interest such as industry-level R&D expenditures, R&D scaled by sales, and number of firms with positive R&D expenditures.[32]

Table 9, columns (1)–(3) presents results from our industry-level regressions with varying fixed effect schemes. The dependent variable of interest, *R&D/sales*, is the industry-level ratio of R&D expenditure to sales.[33] The independent

[32] More information about the National Science Foundation's SIRD can be found at https://www.nsf.gov/statistics/srvyberd/prior-descriptions/overview-sird.cfm#avail
[33] We use R&D scaled by sales instead of assets due to data availability from the SIRD database.

TABLE 10

Distributional Effects among Public and Private Firms

	Number of patents					
	First Two Cohorts				**All Cohorts**	
			Excluding 1993Q2–1996Q1			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Public	3.705***		3.651***		3.324***	
	(31.057)		(30.638)		(31.163)	
Post	0.672***		0.790***		0.790***	
	(32.754)		(32.998)		(32.998)	
Public × Post	−0.264***	−0.232**	−0.304***	−0.265**	−0.171*	−0.171*
	(2.991)	(2.443)	(2.874)	(2.364)	(1.682)	(1.682)
Fixed effects						
Firm	No	Yes	No	Yes	No	Yes
Quarter	No	Yes	No	Yes	No	Yes
Observations	4,157,034	4,157,034	2,892,508	2,892,508	2,945,008	2,945,008
Pseudo R^2	0.25	0.68	0.25	0.68	0.24	0.69

***, **, * Indicate statistical significance at the 1, 5, and 10 percent levels, respectively.
The table reports average treatment effect (ATT) estimates of the implementation of EDGAR on the number of patent filings of publicly listed firms using difference-in-differences PPML regressions. The sample is a panel of firm-quarter observations of 102,657 innovative publicly listed and private firms from 1988Q1 to 2001Q4. *Number of patents* is the number of patents filed in a quarter. *Public* is an indicator variable that equals 1 if a firm is publicly listed and 0 otherwise. *Post* is an indicator variable that equals if the quarter is greater than or equal to 1993Q2, the quarter when EDGAR was first introduced, and 0 otherwise. In columns (1) to (4), publicly listed firms are restricted to those in the first two EDGAR implementation cohorts. Publicly listed firms from all EDGAR implementation cohorts are included in columns (5) and (6). Additionally, in columns (3) to (6), the EDGAR implementation period from 1993Q2 to 1996Q1 is excluded from the sample. t-statistics based on standard errors clustered at the firm level are shown in parentheses below the coefficient estimates.
Appendix A contains detailed variable definitions.

variable of interest, *EDGAR filers/total number of firms*$_{1992}$, is the number of firms in an industry filing through EDGAR during a given period scaled by the total number of firms (fixed as of 1992). The coefficients of *EDGAR filers/total number of firms*$_{1992}$ are negative across all three specifications and statistically significant at conventional levels in two out of the three specifications. Notably, the most restrictive specification including both year and industry fixed effects in column (3) produces a coefficient that is statistically insignificant at conventional levels (with a t-statistic of 1.563), potentially reflecting the low power of our test due to a small sample size. Keeping this most restrictive fixed effects scheme, we redefine our dependent variables in the next three columns of the table using the natural logarithm-adjusted amount of industry-level R&D in column (4), the total number of innovative firms in column (5), and the share of innovative firms scaled by the total number of firms in an industry in column (6). The results are consistent with those of column (3)—whereas the coefficients of *EDGAR filers/total number of firms*$_{1992}$ are negative across all three specifications, they are statistically insignificant at conventional levels. Overall, Table 9 provides suggestive but weak and inconclusive evidence that industries with a greater proportion of EDGAR filers may invest less in innovation. Our findings are directionally consistent with Breuer et al. (2022), who argue that the proprietary costs induced by mandatory reporting requirements in Germany had adverse effects on aggregate innovation.

Our second test sheds light on the distributional effect of the EDGAR implementation on innovation of public *vis-à-vis* private firms. To this end, we use the USPTO patent data available for both private and public firms (Kogan, Papanikolaou, Seru, and Stoffman 2017). Applying several variations of DiD specifications, we compare the evolution of patenting activity for public firms (i.e., the firms treated by the EDGAR implementation) versus private firms (i.e., firms that were not directly impacted by EDGAR) around the EDGAR implementation. Table 10 presents the results.[34]

[34] Unlike our staggered setting, we are able to utilize a symmetric pre- and post-period from 1988 through 2001 in Table 10. Using a longer (five-year) treatment window mitigates concerns regarding the slow-moving nature of innovation.

Across all specifications, we observe that the coefficient of the interaction term *Public* × *Post EDGAR* is negative and statistically significant. Such results suggest that public firms' innovation output, as measured by patent filings, slowed down relative to private firms' output after the EDGAR implementation. Coupled with our previous results, this second finding also points to a potential redistribution of innovative activity between public and private firms, whereby the EDGAR implementation appears to put public firms at a disadvantage relative to private firms (e.g., due to the proprietary cost of disclosures), the former of which curtail innovation in response.

VII. CONCLUSION

The introduction of the SEC's EDGAR system revolutionized the way corporate disclosures are disseminated and processed by market participants. Evidence on the benefits and, perhaps more importantly, the unintended costs of EDGAR is instructive and timely for several reasons. First, information technology developments are intensifying and continue to impact the information environment in which companies operate. Second, there is a growing regulatory interest in expanding U.S. reporting mandates to private firms (Kiernan 2022). Lastly, the steady rollout of EDGAR-like systems internationally indicates that countries continue to implement platforms like EDGAR internationally (McClure, Shi, and Watts 2023). Our findings are particularly relevant to the EU, which recently agreed to create a single access point for all financial and sustainability reports from EU member firms (McGowan 2023).

In this paper, we study the impact of the EDGAR implementation on corporate innovation—a first-order determinant of firms' long-term prospects that is theoretically sensitive to proprietary disclosure costs. We find that a public firm's inclusion in EDGAR leads to an 11 percent decrease in innovation investment. The innovation output, sales, and market share of treated firms consequently also significantly decrease. Consistent with EDGAR exacerbating proprietary costs, we find that firms curtail R&D narratives in their filings. We further document large knowledge spillovers to companies' technology rivals after a firm's inclusion in EDGAR. Our findings support the idea that EDGAR has real effects on companies, particularly for innovative activities, whose returns crucially depend on information spillovers.

Overall, our study adds to the collective evidence regarding real effects on innovation of increasing financial reporting transparency abroad (e.g., Breuer et al. 2022; Berger et al. 2024). As we offer some suggestive but inconclusive evidence of negative aggregate effects and a redistribution of innovative activity from public to private firms not directly subject to EDGAR, we call for future research to better theoretically understand, causally identify, and empirically document the aggregate welfare implications of increased financial reporting transparency.

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APPENDIX A

Variable Definitions

This appendix contains detailed definitions of dependent and independent variables used in the analysis. Compustat data mnemonics are in italics within parentheses.

Variable Name	Description
Dependent variables	
Advertising expenses/total assets (%)	Annual advertising expenses (*xad*) divided by 4 and total assets and multiplied by 100.
Average rival R&D/focal firm assets	The average R&D expenditures of technology rivals divided by the total assets of the focal firms. Technological distances are calculated as in Bloom et al. (2013).
Ln(R&D) (SIRD)	The logarithm of R&D expenditures, as reported by the National Science Foundation SIRD.
Ln(Sales)	The logarithm of sales (*saleq*).
Market share	A firm's share of total sales (*saleq*) within its four-digit SIC code each quarter.
Meet or just beat analyst forecast (=1)	An indicator variable that equals 1 if a firm's realized earnings per share are exactly equal to the median analyst forecast in a quarter, or if realized earnings per share beat the median analyst forecast in a quarter by only one cent and 0 otherwise.

(continued on next page)

APPENDIX A (continued)

Variable Name	Description
Narrative R&D disclosure quantity	The fraction of R&D-related words in a firm's filing, where R&D-related words are identified using the list in Appendix A of Merkley (2014).
Number of patents	The number of patents filed in a quarter.
Number of patents/total assets	The number of patents filed in a quarter divided by total assets (*atq*).
R&D/total assets (%)	Research and development expenses (*xrdq*) divided by total assets (*atq*) and multiplied by 100.
Pre-R&D ROA	Operating income before depreciation (*oibdpq*) and R&D expenditures (*xrdq*) divided by total assets (*atq*).
Scaled originality	The HHI of cited patents across technology classes scaled by the average originality of all patents granted within a technology class-year.
Scaled generality	The HHI index of citing patents across technology classes scaled by the average generality of all patents granted within a technology class-year.
SG&A/total assets (%)	Selling, general, and administrative expenses net of R&D divided by total assets.
SG&A net of advertising expenses/total assets (%)	Selling, general, and administrative expenses net of R&D and advertising expenses divided by total assets.
Tail citations	The percentage of a firm's patents that are in the top 10 percent of citations among all patents applied for in a year.
Innovative firms count (SIRD)	The number of firms with at least $1 million in R&D expenditures in a year, as reported by the National Science Foundation SIRD.
Innovative firms share (SIRD)	The number of firms with at least $1 million in R&D expenditures in a year, as reported by the National Science Foundation SIRD divided by the total number of firms with at least 100 employees, as reported by the U.S. Census Bureau.
R&D/sales (SIRD)	The total R&D expenses of an industry divided by its sales, as reported by the National Science Foundation SIRD.
Independent and other variables	
Book leverage	Current (*dlcq*) and long-term (*dlttq*) debt divided by total assets (*atq*).
Cash/total assets	Cash (*chq*) divided by assets (*atq*).
Innovation leader	An indicator variable that equals 1 for firms with an above-median average share of patents granted within a technology class in the three years before a cohort's inclusion in EDGAR and 0 otherwise.
Innovation quality	An indicator variable that equals 1 for firms with an above-median average share of patent citations within a technology class in the three years before a cohort's inclusion in EDGAR and 0 otherwise.
Innovation secrecy	An indicator variable that equals 1 for firms in industries that rely on trade secrets to protect their innovations as defined in Cohen et al. (2000) and 0 otherwise.
Ln(Assets)	The logarithm of total assets (*atq*).
Public	An indicator variable that equals 1 for publicly listed firms.
Treated rivals	The weighted average number of a firm's treated technology rivals within a cohort. Technological distances are calculated as in Bloom et al. (2013).
Tobin's Q	Book value of assets (*atq*) minus book value of equity (*ceqq*) plus the market value of common equity (*cshoq* × *prccq*) divided by total assets (*atq*).