Does Financial Reporting Matter? Evidence from Accounting Standards*

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Abstract

We exploit firms' voluntary disclosures as a novel firm-specific measure of ex ante sensitivity to individual FASB accounting standards to study the real effects of information regulation. We find that accounting standards impact capital allocation by increasing the cost and reducing the supply of credit and equity financing for sensitive firms. Affected firms respond by drawing down cash reserves and selling more assets compared to insensitive firms. Facing these financial constraints, affected firms cut payout by 2.6% and investment by 3.6%, on average. Our results suggest that accounting standards have economically significant real effects because they reallocate capital in financial markets.

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1 Introduction

The demand for credible information about the stocks and flows of corporate wealth from capital market participants has existed since such markets existed (Berle and Means 1932; Akerlof 1970; Healy and Palepu 2001). To fulfill this demand and to bridge the information gap between companies and capital providers, the practice of financial reporting has arisen as an endogenous response (Previts, Walton, and Wolnizer 2010). The credibility of accounting information depends on the ability of corporations to commit to disclosing summary measures of their recent transaction histories in all states (Sunder 1997), necessitating the regulation and enforcement of financial disclosures (Hope 2003; Hail and Leuz 2006; Christensen, Hail, and Leuz 2013). Since the Securities Exchange Act of 1934, this commitment has been provided by the Securities and Exchange Commission (SEC). From 1972 onward, the process of changing accounting practices has been the responsibility of the Financial Accounting Standards Board (FASB). New standards change the measurement of reported figures as well as the scope of financial disclosure.

In this paper, we examine the role accounting plays in mitigating asymmetric information inefficiencies borne by public companies. In the absence of large-sample data that pre-dates the introduction of regulated financial reporting, we address this fundamental question by studying the incremental changes in accounting practices that accompany new standards. Despite this innovation, we still face the classic empirical challenge of identifying valid counterfactual outcomes for affected firms. Scarce data and endogenous listing decision invalidate private firms as a potential control group, and accounting standards change the properties of accounting for all public companies.

We overcome this identification challenge by introducing a novel data set of firm-specific sensitivities to each accounting standard based on corporate disclosures. Using textual analysis, we collect explicit and direct mentions of each accounting standard in annual corporate filings (i.e., SEC Form 10-K) that pre-date the standard becoming effective. Because companies mention standards in a common format and mentions occur in measurable quantities for every accounting standard, this measure is general to all accounting standards. The interpretation of mentions as the sensitivity to treatment requires no judgement by the econometrician and is pre-determined by construction. Companies are not required to mention standards in their filings, and mentions of specific standards before they become effective are a voluntary disclosure or warning about sensitivity to potential upcoming changes in accounting practices.

With data on the introduction of new standards and a new firm-specific measure of sensitivity to each standard, we study the effect of accounting standards on debt contracting, equity issuance, internal sources of financing, and investment. We use a difference-in-differences methodology that we validate with a series of tests of the treatment and control groups, case studies of individual accounting standards, and dynamic tests of parallel trends. We contribute three new findings.

First, we show that accounting standards help lenders better screen potential borrowers. In particular, compared to insensitive firms, sensitive firms' private loan and public bond contracts are 1.0% and 5.4% less likely to include financial covenants, respectively. This reduction in state contingent contracting is offset by economically and statistically significant increases in the cost of debt and reliance on collateral and decreases in issuance amounts and maturities. These results highlight two potential mechanisms through which accounting standards affect firm outcomes. First, the increase in the cost and decrease in the supply of debt is consistent with accounting standards inducing a negative revelation about sensitive firms.

Second, the reduction in financial covenant usage is consistent with a reduction in the contracting value of accounting information for sensitive firms. This is inconsistent with various alternative explanations, including the negative revelation mechanism, because low quality borrowers tend to receive strict covenant packages (Murfin 2012). Regardless of the mechanism, we find that accounting standards change accounting information such that affected firms become more restricted in their access to debt markets.

Second, we provide evidence that sensitive firms also experience a relative decline in equity issuance by 4.2%, and rely more on internal sources of funds to maintain their operations. Compared to insensitive firms, sensitive ones reduce cash reserves by 1.3%, increase asset sales by 1.1%, and reduce total payout by 2.3%. Third, in line with this increase in financial constraints, we find that sensitive firms cut investment. Our most restrictive specifications suggest that, compared to insensitive firms, sensitive firms cut capital expenditures (CAPEX) by 2.4%, research and development (R&D) expenses by 0.9%, and acquisition expenditures by 2.5%, respectively, following accounting standards. These three sets of results suggest that accounting standards have real effects on firms whose financial information is sensitive to them due to capital reallocation in primary debt and equity markets.

To address identification concerns about our measurement of standard-specific sensitivity, we conduct a series of tests that explore within-firm variation in sensitivity to accounting standards, decompose variation in sensitivity into firm-specific and standard-specific components, link our measure of sensitivity to the particular content of standards in the cross-section of firms using individual standards as case studies, and analyze the key parallel trends assumption of our difference-in-differences empirical design.

These tests uniformly support the causal interpretation of our findings that accounting affects corporate financing and investment policies. The cross-sectional average and standard

deviation of the proportion of standards for sensitivity is 29% and 14.6%, respectively, which suggests that firms cannot be strictly classified into accounting-sensitive and accounting-insensitive groups. Correspondingly, time-invariant firm factors explain only 9.09% of the variation in sensitivity, which mitigates concerns that firm economic conditions or information environment can explain our findings. Using SFAS 123R, 142, and 157 as case studies, we find that sensitive firms grant more options, have larger goodwill accounts, and have more fair value assets and liabilities than insensitive firms, despite the amount of within-firm variation in sensitivity to these standards. These results suggest that our measure of sensitivity has the desirable properties of being general to all accounting standards and capturing the firm-specific exposure of financial statements to accounting changes. Finally, tests of parallel trends for all dependent variables reveals that differential trends in outcomes arise only after standards become effective, and sensitive and insensitive firms behave remarkably similarly before standards become effective. That the parallel trends assumption is not violated is unsurprising given the frequency of accounting standards and lack of persistence in sensitivity at the firm level.

In isolation, our evidence that accounting standards have real effects is not directly informative about the welfare implications of accounting standards. It would be tempting to infer that the real effects of accounting standards for sensitive firms, such as the decline in investment, reduce welfare. However, these changes may be socially optimal if, for example, asymmetric information problems allowed standard-sensitive firms to overinvest beforehand. In this case, accounting standards reduce asymmetric information, which allows debtholders to ration credit to these firms and equityholders to reallocate capital away from these firms.

We interpret the real effects of accounting standards as welfare improving, but acknowledge that our interpretation depends on prior literature. First, if interpreted from the

voluntary disclosure perspective (e.g., Skinner 1994), our sensitivity measure reflects the pre-emptive voluntary disclosure of negative information. If sensitivity to accounting standards is a negative revelation, then we should infer that the real effects we document are reducing pre-existing allocative inefficiencies. Second, Bird, Karolyi, and Ruchti (2017) shows that accounting standards create equity value by reducing asymmetric information. This is consistent with the above mechanism in which accounting standards improve information, which allows for better governance by stakeholders. However, our results on the use of financial covenants in private loans and public bonds suggest that accounting standards reduce the contracting value of accounting information in debt markets. This implies an information design tradeoff between the needs of equityholders and debtholders such that even though accounting standards make equityholders better off, they may make debtholders worse off. This means that accounting standards have heterogeneous effects, which are more likely to be negative for debt-dependent firms.

This paper contributes to several strands of the literature. Our evidence sheds new light on the real effects of disclosure regulation from a comprehensive perspective. In line with Leuz and Wysocki (2016), we provide inferences on the overall desirability, efficiency, and outcomes of reporting and disclosure regulation. Our findings on corporate investments, in particular, further our understanding of the economic consequences of regulation to investors or consumers. Prior research has provided interesting pieces of evidence from policy-evaluation and accounting-choice perspectives by investigating the effects of individual standards (e.g., Botosan and Stanford 2005; Ahmed, Kilic, and Lobo 2006; Beatty and Weber 2006; Hayes, Lemmon, and Qiu 2012; Ertan and Karolyi 2017). Our universal, judgment-free, and ex ante metric of firm-specific sensitivity does not only lend support to these conclusions from a larger and standard-independent perspective. Our evidence also highlights the importance of debt and equity

financing channels, the impact on real activities, and the eventual capital reallocation in the economy.

Our paper is also relevant to the firm-level work examining the link between accounting and investment. Biddle and Hilary (2006) report evidence that accounting quality enhances investment efficiency by mitigating information asymmetry between managers and external capital providers. McNichols and Stubben (2008) show that firms that are subject to SEC enforcements and those that restate their financial statements over-invest during the misreporting period. Biddle, Hilary, and Verdi (2009), on the other hand, draw attention to the role of moral hazard and adverse selection in investment decisions. The authors find that reporting quality allays both over- and under-investment problems. We contribute to these efforts by highlighting the role financial reporting regulation—as opposed to financial reporting choice—plays in disciplining capital allocation and investment efficiency in the economy.

We also extend Shroff (2017), perhaps the closest paper in this literature to ours. Shroff (2017) explores the relationship between cumulative effects of accounting changes and corporate investment. To the extent cumulative effects of accounting changes capture all changes due to a revision in accounting method, this proxy could be interpreted as a small-sample (as most firms report zero effect for accounting changes) and ex post complement to our ex ante measure of sensitivity to accounting standards. Whereas the cumulative effect of accounting changes captures a one-off contemporaneous impact of reporting regulation on earnings, our inferences are based on a within-firm differences-in-differences design that also allows us to consider managers' anticipation of new standards, as well as to sidestep the simultaneity between reported effects of accounting changes and other accounting figures (e.g., corporate investment and earnings).

Collectively, we contribute to the literatures of on audit and enforcement. Most, if not all, standards that we examine change the nature of regulated disclosure (Daske, Hail, Leuz, and Verdi 2008, 2013; Leuz and Wysocki 2016). In this sense, our findings provide new and comprehensive evidence underscoring the importance of audited numbers that are under the purview of the SEC. Even though accounting standards themselves do not entail any immediate or direct real consequence, they change firm behavior by altering the landscape between firms and capital providers.

As an aside, our novel measure of firm-specific sensitivity is relevant to the work on voluntary disclosure (Healy and Palepu 2001). Skinner (1994) proposes litigation and reputation costs as potential explanations for firms' incremental tendency to voluntarily disclosure of bad news. Billings, Cedergren, and Dube (2016) find that managers, following litigation, increase the frequency and timeliness of bad news warnings. Balakrishnan et al. (2014) document that companies respond to a loss of public information by enhancing their voluntary guidance. Our measure of sensitivity is based on the concept how companies brace for mandatory reporting regulation by providing voluntary disclosure. More generally, our study also complements Bird et al. (2017), who suggest that the standardization and regulation of information likely improves investor welfare because incremental disclosure regulation increases public information. Different from this paper, however, we focus on the financing effects as well as real consequences of regulation. Taken together with Bird et al. (2017), we provide evidence confirming the predictions of Fishman and Hagerty (1989): disclosure regulation, by increasing the efficiency of security prices and reassuring external capital providers, can lead to more efficient investment decisions.

2 Institutional background and data

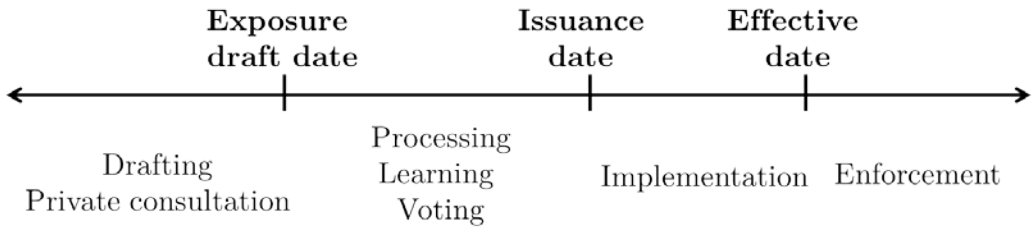
2.1 FASB accounting standards

The SEC has overall responsibility for regulation of US capital markets, though it has, since 1973, delegated the administration of accounting rules to the accounting profession via the Financial Accounting Standards Board (FASB). The FASB is a private organization headquartered in Norwalk, Connecticut, that is overseen by the Financial Accounting Foundation (FAF). FASB is responsible for updating and revising Generally Accepted Accounting Principles (GAAP), which are the common set of accounting principles and rules used to generate financial statements. Between 1973 and 2010, which encompasses our sample period, these GAAP updates were known as Statements of Financial Accounting Standards (SFAS).

In issuing standards, FASB's stated objective is to "foster financial reporting by nongovernmental entities that provides decision-useful information to investors and other users of financial reports." In line with the theoretical rationales for public disclosure and comparability in disclosure discussed above, FASB's Rules of Procedure specifically state that "decisions about the allocation of resources rely heavily on credible, concise, and understandable financial information." Furthermore, FASB recognizes that some decision makers must rely on public financial reports because they lack the ability or influence to compel firms to provide the information they need directly. Cost-benefit analysis of potential standards is a key principle underlying the FASB's work, meaning the cost of preparing, providing, and auditing financial information is explicitly considered.

The FASB consists of seven board members appointed to five-year terms by the FAF, which can be extended for one additional term. Board members must be independent, so they are required to sever ties with their previous organizations. The current membership of the FASB has diverse experience; most members have prior experience at one a public audit firm, but it also includes an academic as well as former corporate officers and investment professionals. The FASB also employs a technical staff of more than sixty people to support the board members.

As can be seen in the timeline below, the standard setting process involves a series of steps. The board identifies a topic which it believes new standards could improve, and then conducts research on the specific issues of relevance. These issues are deliberated at a public meeting. FASB then releases an exposure draft, which outlines the views of the board on the accounting issues at hand, in order to solicit feedback from the public on the proposed new standard. Further public hearings are held, and the board considers comment letters which it receives from stakeholders, such as investors, auditors, and public companies. The board then makes a decision on the form and substance of the new accounting standard, and the individual members vote on whether the standard should be issued. If the vote passes, the standard is released publicly, and becomes part of GAAP.



Timeline: Standard Setting Process

From 1996 to 2010, the FASB issued 44 Statements of Financial Accounting Standards. There is considerable intertemporal variation in exposure draft frequency, with peaks of eight standards issued in 1996 and 2005. As discussed in Bird et al. (2017), the frequency of new standards decreased in the 1990s and 2000s, likely reflecting the possibility that a more developed system of measurement for financial reporting yields fewer opportunities for improvement. Our sample ends in 2010 because the structure of accounting rules switched from the SFAS model to the Accounting Standards Codification, which is updated by the issuance of Accounting Standards Updates. Accounting Standards Updates are structured as SFAS and behaviorally approximate the SFAS timeline and process described above. However, while the standards that we study are the key building blocks of the Codification, the actual standards themselves (mentions of which we collect, as described below) are now technically superseded.

2.2 Data

We collect data on standards from two main sources: the text of the standards themselves and related documents available through FASB, and mentions of standards in firms' 10-Ks (annual reports). Data on private loans and public bonds come from LPC's Dealscan database and the Fixed Income Securities Database (FISD). We use annual data on equity issuance, internal sources of financing, payout, and investment from Compustat. Because private loan data is widely available starting in 1995 and we require a pre-effective date period for our difference-in-differences methodology, we restrict our tests to include standards made effective by FASB between 1996 and 2010.

As described in Section 2.1, FASB takes several steps in the process of implementing a standard. After deliberation and public consultation to determine pressing financial reporting

issues, the formal process begins with the drafting, by the members of the FASB, of an "exposure draft" for a proposed standard. This exposure draft is disseminated to public firms as well as the SEC for comments. We refer to the date on which this document is made public as the exposure draft date. After a period of time, usually longer than a year, the FASB receives input regarding the proposed standard, and revises the draft until a completed version is decided upon. The standard is then made public, and becomes part of GAAP. We refer to the date on which the standard is formally issued as the issuance date. Standards must be announced before they can be implemented by firms and their auditors. The date the standard becomes binding for subsequent financial reports is the effective date.

Using the text from the standards, we collect the dates of associated exposure drafts and the effective date of the standard. Several standards have multiple exposure drafts. When this is the case, we consider the first exposure draft as the first event related to the standard. Because the FASB standards themselves do not specify a day on which they were issued, only a month, we collect issuance dates from FASB press releases regarding the issuance of each standard.

A firm has a variety of ways to communicate information in a 10-K outside of financials. Management discussion and analysis is an opportune place to produce a narrative regarding the performance of a firm. However, in describing the factors affecting that performance, explanations for various financial statement or other outcomes are used and often required in footnotes and other items. If a firm's reported performance is affected by a particular standard, the firm will potentially mention that standard in footnotes or even management discussion and analysis. We take the position that the more times a firm mentions a standard in its 10-K, the

more sensitive that firm is to the standard. We collect standard mentions (e.g., "SFAS 123R") from firm 10-Ks.

Table 1 depicts the pertinent summary statistics. As the mean value of 1[*Sensitive*] indicates, 29.82% of firms mention the standard at least once before its effective date. In continuous form, the average value of *Sensitivity* suggests that there are 3.74 10-K mentions of the standard before it becomes effective. Firm characteristics are consistent with prior work. Most variables (e.g., total assets, cash holdings, investments) are highly skewed, which is why we use this variable in the logged form. The average dollar amounts for CAPEX and R&D are 108 million and 52 million USD respectively.

The attributes of the primary debt deals we employ in our tests are consistent with prior work. The average loan has a funding amount of 264 million USD, price of some 200 bps over LIBOR, and a maturity of about four years. Most deals are collateralized and almost half of them include financial covenants. The deals in the bond market are larger (392 million USD on average) and less pricey (about 145 basis points), consistent with the higher creditworthiness of the borrowers in this market. Likewise, corporate bonds in our sample also have longer maturity, over 10 years.

3 Identification strategy

3.1 Difference-in-differences estimator

To estimate the average treatment effect of accounting standards, we rely on a difference-in-differences methodology. This methodology requires that we measure outcomes for treated and control units before and after the accounting standard becomes effective. Our

outcomes of interest include contract terms in the private loan and public bond markets as well as firms' sources and uses of funds. We isolate treated firms by collecting mentions of specific accounting standards in 10-K filings, focusing specifically on those mentions which appear in 10-K filings before standards become effective. The following is our preferred specification:

$$Y_{ist} = \alpha + \beta_1 1[Sensitive]_{is} + \beta_2 Post_{st} + \beta_3 1[Sensitive]_{is} \times Post_{st} + u_{i \times s} + v_t + \varepsilon_{ist} \qquad (1)$$

where i, s, and t denote to firm, standard, and year, respectively. $1[Sensitive]_{is}$ is an indicator variable that equals one if firm i mentions standard s at least one time in a 10-K filing before the effective date of standard s and zero otherwise.[1] $Post_{st}$ is an indicator that equals one if year t is after the effective date of standard s and zero otherwise. Y_{ist} denotes one of many dependent variables of interest. In Section 4, these will correspond to contract terms of private loan issued by firm i in year t. In Section 5, these will correspond to contract terms of public bonds issued by firm i in year t. In Section 6, these will correspond to internal sources of funds used by firm i in year t, and in Section 7, these will correspond to investment by firm i in year t.

We include a restrictive set of fixed effects; $u_{i \times s}$ and v_t denote firm-by-standard pairwise and year fixed effects, respectively. Year fixed effects force coefficient estimates to be identified using variation from the cross-section of firms from a given year (e.g., comparing treated firms to control firms at the same time and under the same economic conditions). Similarly, firm-by-standard pairwise fixed effects further adjust coefficient estimates for level differences between firms around each accounting standards (i.e., firm A has high Y and firm B has low Y around SFAS 157), which means that we use only within-firm variation in outcomes before versus after

[1] In robustness tests, we provide evidence using the natural log of the count of mentions, suggesting that our results hold on both extensive and intensive margins.

each standard becomes effective. We use these pairwise fixed effects because they account for both ex ante differences between sensitive and insensitive groups and compositional change in the sensitive and insensitive groups from before to after the standards become effective.

This procedure requires a more complex data structure than a simple panel estimator; we need to observe accounting standard event windows for every firm-standard combination, which means that we will observe Y_{it} as many times as there are standards that overlap with year t. For example, there were eight standards that became effective in 2005, so we observe Y for every firm i during windows before the standard became effective (e.g., 2004) and after the standard became effective (e.g., 2005). In many of our tests, Y is observed at the loan package or bond issue level, so we observe every loan or bond issued by firm i during the window before and after the standard becomes effective one time for each accounting standard. We account for this stacking procedure by clustering heteroskedasticity-adjusted standard errors at the firm i level.

To ascribe a causal interpretation to our estimates of β_3 from equation (1), we assume that control units provide a valid counterfactual trend for treated units *after* accounting standards become effective. In subsequent subsections, we validate this assumption by introducing our preferred measure of treatment and discussing potential alternatives, analyzing the distribution of treated firms within and across accounting standards and firms, and evaluating the parallel trends assumption with figures.

3.2 Ex ante standard sensitivity

A valid measure of treatment by accounting standards should be general to all accounting standards, should be free of researcher interventions or judgment, and should be

predetermined in the sense that it does not depend on outcomes that are observed after the standard becomes effective. We accomplish this by introducing a new measure of ex ante sensitivity to accounting standards that is derived from direct mentions of in-process accounting standards in corporate filings. Because companies mention standards in a common format and mentions occur in measurable quantities for every accounting standard, this measure is general to all accounting standards. The interpretation of mentions as the sensitivity to treatment requires no judgement by the researchers and is ex ante by construction. Companies are not required to mention standards in their filings, and mentions of specific standards before they become effective are, by definition, a voluntary disclosure or warning about the sensitivity to upcoming potential regulation of accounting information. Thus, our standard-specific measure of sensitivity to accounting standards is general to all firms and standards, requires no judgment by researchers, and does not rely on ex post outcomes.

We note here that our proxy is not the only alternative measure of sensitivity to accounting standards, but it is, to our knowledge, the only one that has these three desirable properties. One could derive, for example, for each accounting standard, a measure of sensitivity based on corporate operations. The derivation process might be general to all standards, but it would require significant analysis of corporate operations, which itself might be so costly that it would need researcher judgement. Such a measure would also be inherently ex post in the sense that the features of corporate operations that make a firm sensitive to a given standard are colored by our interpretation of corporate operations and the effects of standards known to us after the standards become effective. The closest paper to ours in the literature on changes in accounting information is Shroff (2017), which studies cumulative changes in earnings due to

accounting changes. Although Shroff (2017) does not follow this interpretation, one could interpret these changes as a proxy for sensitivity to accounting changes. However, cumulative changes in earnings due to accounting changes is inherently ex post and is not general to all accounting standards since it focuses only on earnings, so it does not have two of the desirable features of a valid measure of treatment. Moreover, accounting standards impact the information contained in accounts apart from those that impact earnings, and, in line with this observation, this alternative only affects approximately 2% of our sample firm-year observations, or about one order of magnitude less than our preferred measure of sensitivity.

3.3 Distribution of sensitivity

A leading concern with studies of continuous regulation is that sensitivity to individual regulatory events is persistent, either because sensitivity is related to quality or because regulation is discriminatory. We first note that our difference-in-differences methodology isolates within-firm variation in outcomes around each accounting standard and eliminates differences in the outcomes between treated and control firms using firm-by-standard pairwise fixed effects. This alleviates the concern that potential contaminants, like differences in quality, of our sensitivity measure could be driving our estimates of β_3. Nevertheless, to further mitigate such concerns, we provide summary statistics of the persistence of sensitivity and a variance decomposition of our measure of sensitivity.

First, Figure 1 provides a histogram of the propensity of firms to be sensitive to a given standard using data from the cross-section of firms. The histogram shows that sensitivity is not persistent within-firm; the average firm is sensitive to 29% of the standards and the cross-sectional standard deviation is 14.6%. The maximum firm-level sensitivity across standards is

83%, meaning the distribution is bounded well below 100% and no firms are persistently sensitive to all accounting standards.

Second, we decompose the variance of 1[*Sensitive*] using regression analysis. In particular, we are interested in the fraction of variation in 1[*Sensitive*] which can be explained by firm-specific factors. If this fraction is large, it would indicate that time-invariant firm characteristics explain sensitivity to accounting standards, contrary to the distributional evidence presented above. In particular, we regress 1[*Sensitive*] on firm and standard fixed effects to attribute variation in sensitivity to time-invariant firm factors and standard-specific factors. Unexplained variation in this specification can be attributed to firm factors that vary with individual standards. This test is useful because if the variation in 1[*Sensitive*] that can be explained by time-invariant firm factors is small, then it casts doubt on a causal interpretation of β_3 estimates from equation (1) because sensitivity may simply be due to cross-sectional differences in the economic conditions or information environment of firms. We find that time-invariant firm factors can explain only 9.09% of variation in sensitivity, suggesting that persistent cross-sectional firm heterogeneity is not driving our β_3 estimates. In contrast, 39.89% of variation in sensitivity can be explained by standard-specific factors, which suggests that accounting standards vary in their impact, consistent with the variation in academic and media attention paid to accounting standards. The residual 51.02% of variation is attributable to firm factors that are specific to individual standards, which support our treatment effects interpretation.

3.4 Case studies of sensitivity

Our measure of sensitivity required no researcher interventions or information about ex post outcomes for interpretation. A valid measure of standard-specific sensitivity should, however, also correlate with ex post sensitivity, particularly in cases in which standards make specific and significant changes. To further validate sensitivity as a discriminator between treated and control firms, we provide evidence that our ex ante standard-specific measure of sensitivity is correlated with ex post measures of sensitivity in case studies of individual standards. In particular, we examine the relation of $1[Sensitive]$ to such ex post measures using SFAS 123R, SFAS 142, and SFAS 157 as case studies because these three accounting standards are the most mentioned (Bird et al., 2017).

We concentrate on these standards, as they are quite significant and relevant yet related to specific items that can be reliably evaluated in large samples. SFAS 123R (2005) required that firms expense granted stock options at fair value, rather than intrinsic value, affecting compensation landscape as well as corporate risk-taking (e.g., Hayes, Lemmon, and Qiu 2012). SFAS 142 (2001) provided a uniform structure to accounting for business combinations. The regulation eliminated the pooling-of-interests method and goodwill amortization, rendering the purchase method as the only option for mergers and acquisitions (e.g., Beatty and Weber 2006). Finally, SFAS 157 (2007) established a framework for fair-value assets and liabilities and expanded required disclosures about fair value measurements. In keeping with the objective and structure of these standards, we expect affected firms—i.e., option-intensive firms (123R), companies with large goodwill (142), and those with a large fraction of fair value assets (157)—to issue voluntary disclosures before the effective date and, thus, to be captured by our measure. We test whether this consideration is confirmed empirically.

We present three tables in Appendix A that link our measure of ex ante sensitivity to standard-specific ex post measures in the cross-section of firms. In Table A1, we link the total number of options granted and options outstanding at fiscal year-end to 1[*Sensitive*]. We find that firms that mention SFAS 123R grant 8.3% more options and have 24.0% more options outstanding than firms that did not mention the standard in their pre-effective date 10-K filings. Similarly, in Table A2 we link the value of the goodwill account to 1[*Sensitive*] for SFAS 142. We find that sensitive firms have 200.9% larger goodwill accounts than insensitive firms in the year after SFAS 142. Finally, in Table A3 we link sensitivity to SFAS 157 to measures of fair value assets, liabilities, and earnings changes. We find that sensitive firms have 49.2% more fair value assets and 20.2% more fair value liabilities, and their earnings are affected 2.7% more than firms that do not mention SFAS 157. Together, this collage of case studies confirms that our ex ante measure of sensitivity is (i) general to all accounting standards, and (ii) directly linked to the ex post effects of accounting standards.

3.5 Parallel trends

The final measure we take to validate 1[*Sensitive*] as a measure of treatment by accounting standards is a visual test of the parallel trends assumption. In Figure 2, we present parallel trends plots for all dependent variables of interest. These figures show trends for sensitive and insensitive firms, normalizing each group to zero in year $t - 3$ for ease of comparison. This normalization preserves pre-effective date trends for both groups, so it does not impact a visual test of parallel trends.

Panel A of Figure 2 presents parallel trends plots for each of the four contract terms from private loans that we study. Each plot shows visual evidence of contract terms changing

for sensitive firms relative to the counterfactual terms of insensitive firms at $t = 0$, which is the year of the effective date. Loan spreads, amounts, collateral usage, and covenant usage are all increasing for both sensitive and insensitive firms around accounting standards. After standards' effective dates, loan spreads and collateral usage increase for sensitive firms relative to insensitive firms, and loan amounts and covenant usage decrease for sensitive firms relative to insensitive firms. These results are consistent with a decrease in the debt contracting value of accounting information and a corresponding substitution toward collateral. Overall, these results suggest that the cost of loans increases and supply of loans decreases for sensitive firms relative to insensitive firms. More importantly, a visual inspection of the plots in Panel A shows that the pre-effective date trends for sensitive and insensitive firms are nearly identical and only significantly diverge after the effective date.

The plots in Panel B, which present visual evidence of parallel trends for the four public bond terms that we study, tell a similar story. On average, for sensitive and insensitive firms, bond spreads and amounts are increasing and maturities and covenant usage are decreasing around accounting standards. However, after standards become effective, differential trends emerge with bonds issued by sensitive firms showing a larger increase in spreads, smaller increase in amounts, and larger decreases in both maturity and covenant usage. These results echo the results in the private loan market, and suggest that both the debt contracting value of accounting information decreases for sensitive firms relative to insensitive firms and that issuers substitute covenant usage for larger spreads, smaller amounts, and shorter maturities. As with the results in Panel A, the pre-effective date trends are remarkably similar, and significant differences between the two groups occurs only after standards become effective.

Lastly, Panel C presents parallel trends plots for the collection of dependent variables that measure other sources and uses of funds for sensitive and insensitive firms. These dependent variables complete the picture of the financing and real effects of accounting standards. Relative to insensitive firms, sensitive firms issue less equity, increasingly rely on internal sources of funds (i.e., cash reserves and asset sales), reduce payout to shareholders, and significantly cut various forms of investment (i.e., R&D and capital expenditures). These effects only present themselves in the years following accounting standards' effective dates, and, in all seven cases, these plots show evidence of parallel pre-effective date trends for sensitive and insensitive firms. In sum, combined with the distributional and variance analysis in previous subsections, this parallel trends evidence suggests that our difference-in-differences methodology can be interpreted causally.

4 Accounting standards and private loans

How does accounting regulation impact financing? We first explore the private debt market to shed light on this question. Banks expand the debt contracting space especially for smaller and informationally opaque borrowers by overcoming or mitigating information asymmetry by virtue of their expertise and superior screening and monitoring abilities (Diamond 1984; Diamond 1991; Holmstrom and Tirole 1997). In fact, bank loans have constituted the largest source of external financing for US corporation for decades (Gorton and Winton 2003). Accounting research also confirms the view that banks are more willing to lend to firms with poorer accounting quality (e.g., Bharath, Sunder, and Sunder 2008); therefore, it is necessary to individually evaluate bank and public debt.

To verify that our findings discussed above holds in a multivariate setting, we estimate equation (1) in which $Y_{i,t}$, the dependent variable, is one of the following: spreads, amounts, covenant usage, collateral usage. Tables 2 through 6 explore the effect of accounting standards on the private debt contracting landscape. Our inferences provide unanimous support to the notion that inefficient firms that are singled out by accounting standards switch to a new regime in which credit is less available and more expensive.

Table 2 depicts this link between for the amount of credit issued by borrowers surrounding the event window. The results in column (1) show in a within-firm and within-standard model that sensitive firms reduce loan issuance amounts by a relative 2.36%. This figure goes up to the 7.84% level (columns (2) and (3)) after accounting for Year fixed effects and Firm $\times$ Standard fixed effects, which essentially yield a within-firm-standard research design that concentrates on firms that issue debt before as well as after the regulation. This finding provides rigorous multivariate support to the conclusion that lenders increase the credit availability toward borrowers that are affected positively by accounting standards.

Table 3 echoes these findings for the cost of debt. In particular, we observe a relative increase in the credit spread paid by sensitive firms following regulation. Economically, our estimates correspond to a 2.66–3.68% increase in the spread charged for the average contract. This result complements the credit rationing findings from Table 2. Both tables support the inference that the supply of credit declines for sensitive borrowers, so that they need to pay more for credit.

Table 4 presents evidence on the effects of accounting standards on covenant usage. Restrictive financial covenants, which are written on accounting ratios and amounts, reflect, in

part, the willingness of lenders to contract on accounting information. The differences-in-differences estimates in Table 4 show that the propensity of using financial covenants decreases for sensitive firms relative to insensitive firms after accounting standards. The economic magnitude is significant as well; 43.6% of loans in our sample contain at least one financial covenant, so the marginal decrease for sensitive firms due to an accounting standard is between 2.13% and 4.02%.

The differences-in-differences estimator in Table 5 indicates that sensitive are more likely than other borrowers to pledge collateral while borrowing post-regulation. We interpret this finding as an increase in lenders' requirement of tangible guarantees from these borrowers. This interpretation complements our evidence on the reduced usage of financial covenants, which reflects the diminished debt contracting value of accounting information. One alternative explanation is that sensitive borrowers, which are affected more directly by the regulation, now has more reliable asset figures that are more acceptable by lenders as collateral.

Table 6 provides a comprehensive robustness assessment of the bank-debt inferences we discuss thus far. Each column in this table corresponds to a previous table. Column (1) reexamines the amount tests (originally displayed in Table 2), while Column (2) tests the sensitivity of the findings for the spread (main results shown in Table 3), and so on. The nature of robustness comes from the definition of the event window and that of our sensitivity measure. Looking at the first six rows, we find that for event windows of three months and six months, most of our results are economically coherent and consistent with the main findings, which are based on an event windows of two years before and after each regulation. For windows between one year and five years, all of our findings are economically as well as statistically robust.

The bottom half of Table 6 explores the robustness of conclusions to changes in our main measure, *Sensitivity*. The first row, 1[*Sensitive*], shows the results using this variable on the right-hand side. Recall that this variable is an indicator that switches on if any of the borrower's 10-K filings from before the effective date of mention the accounting standard. Since 1[*Sensitive*] is the proxy we used in the tests displayed Tables 2 through 5, the coefficients reported for 1[Sensitive] in Table 6 the same as these tables: 7.84% (amount), 3.68% (spread), 1.75% (collateral), and -0.93 (covenants). The last two rows in Table 6 show how these inferences change we replace 1[*Sensitivity*] with (i) ln*Sensitivity* (the natural log of the number of times the borrower's 10-K filings from before the effective date mention the accounting standard) and with (ii) 1[*Sensitivity*>Mean] (an indicator that equals one if the borrower's 10-K filings from before the effective date mention the accounting standard greater than the mean number of mentions and zero otherwise). The uniformly significant coefficient estimates suggest that our inferences remain statistically and economically significant for alternative and more heterogeneous definitions of firms' sensitivity to accounting standards.

5 Accounting standards and public bonds

We next examine the implications of accounting standards in the public bond markets. This is important for understanding whether the effects are limited to banks and relatively opaque borrowers typical in the private debt market. Accordingly, we estimate equation (1) but replace Y with bond terms: *amounts*, *spreads*, *maturity*, and *covenants*. Table 7 reports the estimation results only for the most saturated model that includes Firm × Standard as well as Year fixed effects.

The differences-in-differences estimator shows that accounting standards have similar effects on the bond and loan issuance behavior of sensitive firms. Sensitive firms experience a reduction in bond issuance amounts, maturities, and covenant usage as well as an increase in spreads relative to insensitive firms after accounting standards, on average. The economic effects of standards on bond amounts are similar to our results from the loan markets: We observe a 7.4% relative decline in bond issuance amounts (compared to 7.8% for bank debt). Post-regulation bonds are 5.4% less likely to include covenants for sensitive firms than other firms. Likewise, the maturity of sensitive firms' bonds is 0.39 years shorter post-regulation, corresponding to 4% of the sample standard deviation. Overall, these results are consistent with the bank debt results in the previous section because they suggest that sensitive firms reduce the contracting value of accounting information for public bond markets and that the cost of debt increases for sensitive firms. This evidence also suggests that sensitive firms do not meaningfully substitute public debt for bank debt when bank debt becomes more expensive following accounting standards.

6 Accounting standards and alternative sources of funds

Previous sections provide evidence that accounting standards increase the cost of debt financing for sensitive firms and offer potential mechanisms related to the revelation of negative information and the debt contracting value of accounting information. In this section, we explore whether sensitive firms offset this reduced access to public and bank debt markets with equity issuance and internal funds. We again turn to our differences-in-differences estimator from equation (1), now replacing Y with equity issuance, cash balances, asset sales, and total

payout. Combined with debt issuance, these outcome variables provide a complete picture of the sources of funds for sensitive and insensitive firms around accounting standards.

Overall, the evidence in Tables 8 and 9 provide evidence that sensitive firms experience a reduction in access to external funds and increasingly rely on internal funds to maintain operations. Estimates in Table 8 suggest that sensitive firms reduce the amount of equity issuance by 4.23% to 7.14% relative to insensitive firms after accounting standards. Similarly, estimates in Table 9 suggest that sensitive firms draw down cash reserves by 1.27% to 2.38%, increase asset sales by 1.12% to 2.00%, and reduce total payout, which is comprised of share repurchases and dividends, by 2.26% to 3.36%. These findings universally show that accounting standards produce an economically significant shift from external to internal sources of funds for sensitive firms relative to insensitive firms.

7 The real effects of accounting standards

The critical outcome of capital allocation is real investment. If accounting standards improve the financing condition of insensitive firms at the expense of sensitive ones, then we should observe a relative increase in the investment of insensitive firms. Our differences-in-differences estimator would produce this result as a decline in the investments of sensitive firms. We examine this question using equation (1) in which we use capital and research & development, and acquisition expenditures as dependent variables.

Results shown in Panels A through C of Table 10 provide unanimous support for the conclusion that, relative to insensitive firms, sensitive firms reduce investment after accounting standards. As previous sections have shown, this reduction in investment occurs despite a shift

to internal sources of funds, suggesting that accounting standards significantly increase the cost of external financing for sensitive firms. Our most conservative estimates suggest that the reduction in capital, R&D, and acquisitions are economically significant; sensitive firms reduce capital expenditures by 2.41%, research and development by 0.89%, and acquisition expenditures by 2.49%. These figures are obtained holding Firm × Standard and Year fixed effects, as well as time-varying firm characteristics, including total assets, market-to-book ratio, and the cumulative effect of accounting changes on earnings (Shroff 2017).

8 Conclusion

We exploit novel data on firm-specific sensitivity to each FASB accounting standard to examine the capital market and real effects of accounting. Sensitivity to accounting standards varies significantly across and within firms, and sensitive and insensitive firms share common pre-standard trends, supporting a causal interpretation of our findings. Our debt markets evidence suggests that, in addition to inducing negative revelations about sensitive firms, accounting standards reduce the debt contracting value of accounting information, which otherwise increases the cost and reduces the supply of private loans and public bonds to sensitive firms. Relative to insensitive firms, sensitive firms reduce equity issuance, rely more on internal sources of funds, cut payout, and reduce investment in capital, research and development, and acquisitions. In sum, by studying changes in accounting that differentially affect valid treatment and control groups, we provide novel evidence that accounting matters.

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Appendix A

Table A1. Sensitivity to SFAS 123R

This table presents a cross-sectional regression of ex post measures of the sensitivity to SFAS 123R to our preferred ex ante standard-specific measure of sensitivity. The sample includes all public firms in 2006. $\ln OptionsGranted$ and $\ln OptionsOutstanding$ are the natural logs of the total options granted and total options outstanding at fiscal year-end. $1[Sensitive]$ is an indicator that equals one if any of the borrower's 10-K filings from before the effective date mention "SFAS 123R" and zero otherwise. Heteroskedasticity-robust standard errors are presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

Dependent variable:	$\ln OptionsGranted$	$\ln OptionsOutstanding$
	(1)	(2)
$1[Sensitive]$	8.334***	23.973***
	(2.515)	(4.073)
R^2	0.0010	0.0033
Obs.	11,422	

Table A2. Sensitivity to SFAS 142

This table presents a cross-sectional regression of ex post measures of the sensitivity to SFAS 142 to our preferred ex ante standard-specific measure of sensitivity. The sample includes all public firms in 2003. $\ln Goodwill$ is the natural log of total goodwill. $1[Sensitive]$ is an indicator that equals one if any of the borrower's 10-K filings from before the effective date mention "SFAS 123R" and zero otherwise. Heteroskedasticity-robust standard errors are presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

Dependent variable:	$\ln Goodwill$
	(1)
$1[Sensitive]$	200.931***
	(3.808)
R^2	0.1288
Obs.	9,847

Table A3. Sensitivity to SFAS 157

This table presents a cross-sectional regression of ex post measures of the sensitivity to SFAS 157 to our preferred ex ante standard-specific measure of sensitivity. The sample includes all public firms in 2007. $\ln TotalFVA$, $\ln TotalFVL$, and $\ln TotalFVCE$ are the natural logs of the total fair value assets, total fair value liabilities, and total fair value changes including earnings. $1[Sensitive]$ is an indicator that equals one if any of the borrower's 10-K filings from before the effective date mention "SFAS 157" and zero otherwise. Heteroskedasticity-robust standard errors are presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

Dependent variable:	$\ln TotalFVA$	$\ln TotalFVL$	$\ln TotalFVCE$
	(1)	(2)	(3)
$1[Sensitive]$	49.282***	20.192***	2.679**
	(7.674)	(6.220)	(1.374)
R^2	0.0042	0.0012	0.0002
Obs.	8,182		

Figure 1. Histogram of Sensitivity

This figure presents a histogram of the proportion of standards for which firms are sensitive. *Sensitive* is an indicator that equals one if any of the borrower's 10-K filings from before the effective date mention the accounting standard and zero otherwise. The mean and standard deviation of this proportion are 29% and 14.6%, respectively. The maximum proportion of standards to which a firm is sensitive is 83%, which suggests that the distribution is bounded below 100%.

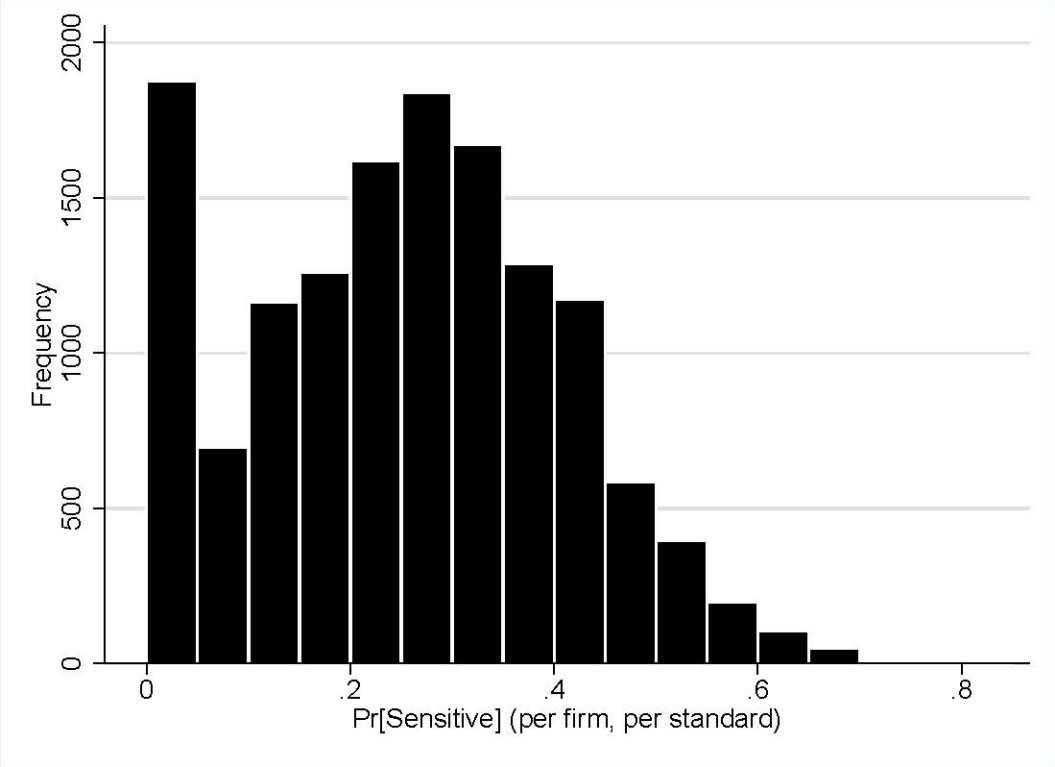
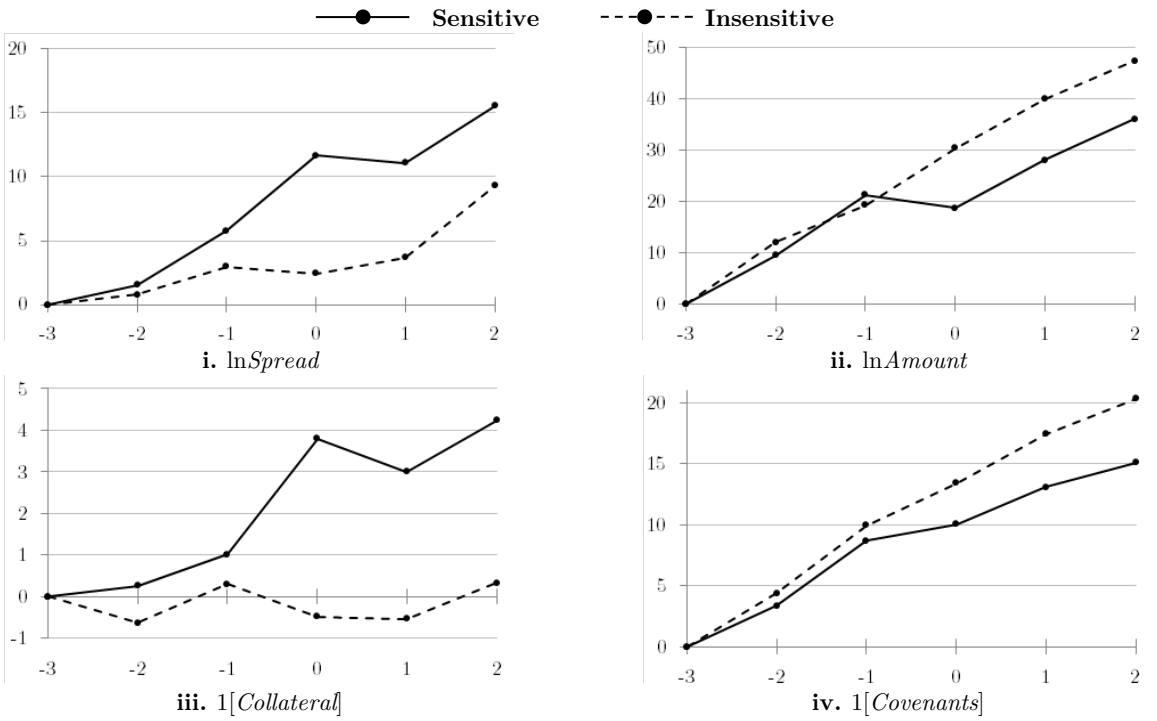


Figure 2. Parallel Trends Plots

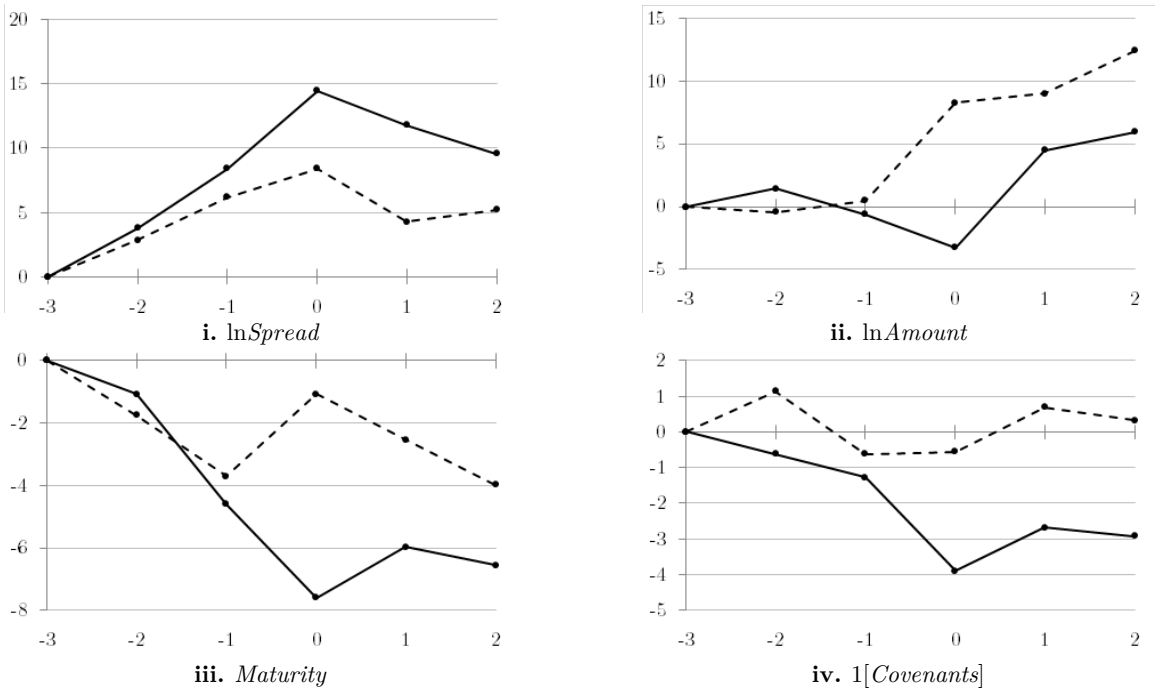
This figure presents a visual test of the parallel trends assumption for each dependent variable of interest. Connected scatterplots for sensitive and insensitive groups are plotted relative to their corresponding $t-3$ values, preserving pre-effective date trends for visualization. *Sensitive* is an indicator that equals one if any of the borrower's 10-K filings from before the effective date mention the accounting standard and zero otherwise. The solid (dashed) line corresponds to the sensitive (insensitive) group.



i. ln*Spread* **ii.** ln*Amount*

iii. 1[*Collateral*] **iv.** 1[*Covenants*]

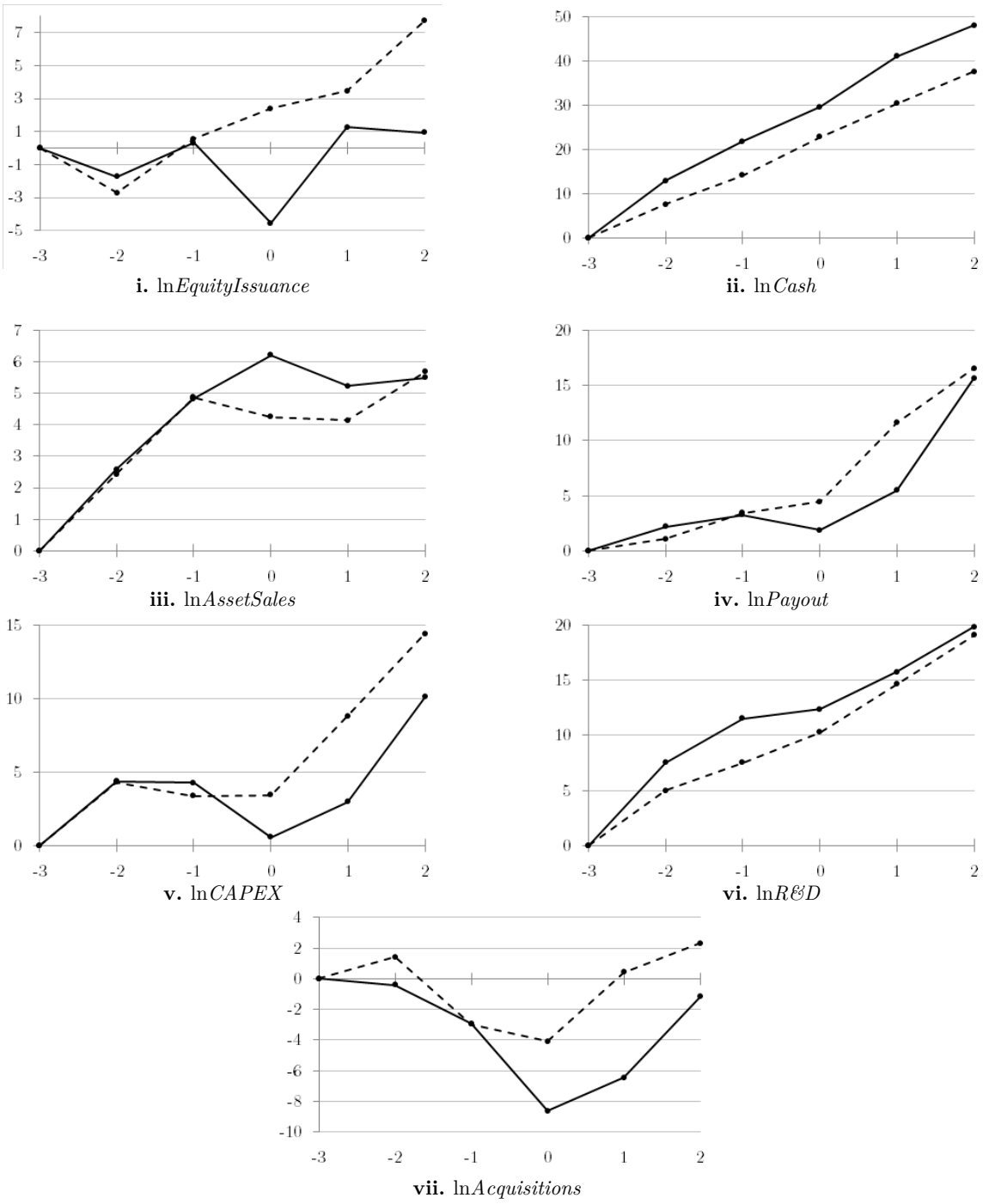
Panel A. Private Loans



i. ln*Spread* **ii.** ln*Amount*

iii. *Maturity* **iv.** 1[*Covenants*]

Panel B. Public Bonds



Panel C. Equity Issuance, Internal Sources of Funds, and Investment

Table 1. Summary Statistics

This table presents summary statistics for the regression variables of interest.

	Mean	SD	P25	Median	P75
Loans:					
Amount ($M)	264	699	23	85.2	250
Spread	205.92	146.28	87.5	200	280
Maturity	3.95	2.72	1.92	3.83	5
1[*CovenantIntensity*>0]	43.60%				
1[*Collateral*>0]	76.48%				
Bonds:					
Amount ($M)	392	2,819	80	200	365
Spread	144.95	165.37	40	95	185
Maturity	10.39	9.83	5	9	10
1[*CovenantIntensity*>0]	57.47%				
Firms:					
Total Assets ($M)	5,577.99	49,357.83	43.96	282.04	1,256.81
AC_CHG ($M)	-3.68	254.06	0	0	0
Market-to-Book	2.27	4.38	0.85	1.53	2.67
Equity Issuance ($M)	36.85	602.36	0	0.60	7.97
Cash ($M)	395.51	5,370.95	2.39	15.22	68.07
Asset sales ($M)	13.88	170.04	0.03	0.17	1.00
Payout ($M)	83.95	671.96	0.00	0.20	11.28
CAPEX ($M)	108.13	698.73	0.46	4.05	28.24
R&D ($M)	52.23	343.82	0.02	2.95	15.96
Acquisitions ($M)	40.65	489.43	0	0	0.53
1[*Sensitive*]	29.82%				
Sensitivity	3.74	11.45	0	0	2

Table 2. Accounting Standards and Credit Supply

This table presents staggered difference-in-differences estimates of the effect of accounting standards on loan amounts for standards that became effective between 1996 and 2010. The unit of observation is loan and the event window is restricted to the year immediately preceding and the year immediately following the standard becoming effective. *Amount* is the deal size. 1[*Sensitive*>0] is an indicator that equals one if any of the borrower's 10-K filings from before the effective date mention the accounting standard and zero otherwise. *Post* is an indicator that equals one for loans initiated after the accounting standard becomes effective and zero otherwise. We include increasingly restrictive fixed effects focus identifying variation on within standard and firm variation in loan amounts over time. Heteroskedasticity-robust standard errors are clustered by firm, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

Dependent variable: ln*Amount*			
	(1)	(2)	(3)
1[*Sensitive*] × *Post*	-2.361***	-7.840***	-7.837***
	(0.700))	(1.038)	(1.038)
Fixed Effects:			
Firm	Yes	Yes	Yes
Standard	Yes	Yes	Yes
Firm × *Standard*	No	Yes	Yes
Year	No	No	Yes
R^2	0.7113	0.8018	0.8018
Obs.	407,047		

Table 3. Accounting Standards and Cost of Debt

This table presents staggered difference-in-differences estimates of the effect of accounting standards on loan spreads for standards that became effective between 1996 and 2010. The unit of observation is loan and the event window is restricted to the year immediately preceding and the year immediately following the standard becoming effective. *Spread* is the all-in-drawn spread for the loan. 1[*Sensitive*>0] is an indicator that equals one if any of the borrower's 10-K filings from before the effective date mention the accounting standard and zero otherwise. *Post* is an indicator that equals one for loans initiated after the accounting standard becomes effective and zero otherwise. We include increasingly restrictive fixed effects focus identifying variation on within standard and firm variation in loan amounts over time. Heteroskedasticity-robust standard errors are clustered by firm, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

Dependent variable: ln*Spread*			
	(1)	(2)	(3)
1[*Sensitive*] × *Post*	2.661***	3.682***	3.683***
	(0.371)	(0.462)	(0.462)
Fixed Effects:			
Firm	Yes	Yes	Yes
Standard	Yes	Yes	Yes
Firm × *Standard*	No	Yes	Yes
Year	No	No	Yes
R^2	0.7137	0.8800	0.8801
Obs.	327,294		

Table 4. Accounting Standards and Financial Covenant Usage

This table presents staggered difference-in-differences estimates of the effect of accounting standards on covenant usage for standards that became effective between 1996 and 2010. The unit of observation is loan and the event window is restricted to the year immediately preceding and the year immediately following the standard becoming effective. 1[*CovenantIntensity*>0] is an indicator that equals one if the loan contains more than one restrictive financial covenants and zero otherwise. 1[*Sensitive*] is an indicator that equals one if any of the borrower's 10-K filings from before the effective date mention the accounting standard and zero otherwise. *Post* is an indicator that equals one for loans initiated after the accounting standard becomes effective and zero otherwise. We include increasingly restrictive fixed effects focus identifying variation on within standard and firm variation in loan amounts over time. Heteroskedasticity-robust standard errors are clustered by firm, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

Dependent variable: 1[*CovenantIntensity*>0]			
	(1)	(2)	(3)
1[*Sensitive*] × *Post*	-1.756***	-0.927***	-0.928***
	(0.260)	(0.341)	(0.341)
Fixed Effects:			
Firm	Yes	Yes	Yes
Standard	Yes	Yes	Yes
Firm × *Standard*	No	Yes	Yes
Year	No	No	Yes
R^2	0.4665	0.7093	0.7093
Obs.	407,102		

Table 5. Accounting Standards and Collateral Usage

This table presents staggered difference-in-differences estimates of the effect of accounting standards on collateral usage for standards that became effective between 1996 and 2010. The unit of observation is loan and the event window is restricted to the year immediately preceding and the year immediately following the standard becoming effective. 1[*Collateral*] is an indicator that equals one if the loan is secured by collateral and zero otherwise. 1[*Sensitive*] is an indicator that equals one if any of the borrower's 10-K filings from before the effective date mention the accounting standard and zero otherwise. *Post* is an indicator that equals one for loans initiated after the accounting standard becomes effective and zero otherwise. We include increasingly restrictive fixed effects focus identifying variation on within standard and firm variation in loan amounts over time. Heteroskedasticity-robust standard errors are clustered by firm, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

Dependent variable: 1[*Collateral*]			
	(1)	(2)	(3)
1[*Sensitive*] × *Post*	0.835***	1.751***	1.752***
	(0.243)	(0.347)	(0.348)
Fixed Effects:			
Firm	Yes	Yes	Yes
Standard	Yes	Yes	Yes
Firm × *Standard*	No	Yes	Yes
Year	No	No	Yes
R^2	0.6107	0.8259	0.8259
Obs.	255,299		

Table 6. Accounting Standards and Loan Terms: Robustness

This table presents staggered difference-in-differences estimates of the effect of accounting standards on loan terms for standards that became effective between 1996 and 2010. The unit of observation is loan and the event window is restricted to the year immediately preceding and the year immediately following the standard becoming effective. *Amount* is the deal size. *Spread* is the all-in-drawn spread for the loan. 1[*CovenantIntensity*>0] is an indicator that equals one if the loan contains more than one restrictive financial covenants and zero otherwise. 1[*Collateral*] is an indicator that equals one if the loan is secured by collateral and zero otherwise. 1[*Sensitive*] is an indicator that equals one if any of the borrower's 10-K filings from before the effective date mention the accounting standard and zero otherwise. ln*Sensitivity* is the natural log of the number of times the borrower's 10-K filings from before the effective date mention the accounting standard. 1[*Sensitivity*>Mean] is an indicator that equals one if the borrower's 10-K filings from before the effective date mention the accounting standard greater than the mean number of mentions and zero otherwise. *Post* is an indicator that equals one for loans initiated after the accounting standard becomes effective and zero otherwise. All specifications include firm × standard and year fixed effects to focus identifying variation on within standard and firm variation in loan amounts over time. For each loan term, we present coefficient estimates that correspond to event windows that vary between 3 months and 5 years (compared to our baseline event window is 1 year) as well as alternative measures of standard-specific sensitivity. Heteroskedasticity-robust standard errors are clustered by firm, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

Dependent variable:	ln*Spread*	ln*Amount*	1[*Collateral*]	1[*CovenantIntensity*>0]
	(1)	(2)	(3)	(4)
Event window:				
3 months	5.235***	-5.919	2.535	-1.319
6 months	1.661*	-5.424**	4.002***	-0.970
1 year	3.683***	-7.837***	1.752***	-0.928***
2 years	4.361***	-8.040***	1.932***	-2.458***
3 years	3.500***	-8.294***	2.517***	-3.082***
5 years	3.636***	-9.286***	3.099***	-4.237***
Sensitivity measure:				
1[*Sensitive*]	3.683***	-7.837***	1.752***	-0.928***
ln*Sensitivity*	0.571***	-2.934***	0.695***	-0.252**
1[*Sensitivity*>Mean]	1.713***	-7.999***	1.831***	-0.864**

Table 7. Accounting Standards and Public Bond Issuance

This table presents staggered difference-in-differences estimates of the effect of accounting standards on loan terms for standards that became effective between 1996 and 2010. The unit of observation is loan and the event window is restricted to the year immediately preceding and the year immediately following the standard becoming effective. *Amount* is the deal size. *Spread* is the gross spread for the bond. 1[*CovenantIntensity*>0] is an indicator that equals one if the bond contains at least one covenant and zero otherwise. *Maturity* is the bond maturity in years. 1[*Sensitive*] is an indicator that equals one if any of the borrower's 10-K filings from before the effective date mention the accounting standard and zero otherwise. *Post* is an indicator that equals one for loans initiated after the accounting standard becomes effective and zero otherwise. We include increasingly restrictive fixed effects focus identifying variation on within standard and firm variation in loan amounts over time. Heteroskedasticity-robust standard errors are clustered by firm, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

Dependent variable:	ln*Amount*	ln*Spread*	*Maturity (yrs)*	1[*CovenantIntensity*>0]
	(1)	(2)	(3)	(4)
1[*Sensitive*] × *Post*	-0.074***	0.660	-0.387**	-0.054***
	(0.019)	(1.967)	(0.168)	(0.021)
Fixed Effects:				
Firm	Yes	Yes	Yes	Yes
Standard	Yes	Yes	Yes	Yes
Firm × *Standard*	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
R^2	0.7066	0.6327	0.5595	0.6190
Obs.	157,735			

Table 8. Accounting Standards and Equity Issuance

This table presents staggered difference-in-differences estimates of the effect of accounting standards on leverage for standards that became effective between 1996 and 2010. The unit of observation is firm-year and the event window is restricted to the year immediately preceding and the year immediately following the standard becoming effective. ln*EquityIssuance* is the natural log of the total amount of equity issued. 1[*Sensitive*] is an indicator that equals one if any of the borrower's 10-K filings from before the effective date mention the accounting standard and zero otherwise. *Post* is an indicator that equals one for loans initiated after the accounting standard becomes effective and zero otherwise. We include increasingly restrictive fixed effects focus identifying variation on within standard and firm variation in loan amounts over time. Controls include the natural log of total assets, market-to-book ratio, and the cumulative effect of accounting changes on earnings (Shroff 2017). Heteroskedasticity-robust standard errors are clustered by firm, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

Dependent variable: ln*EquityIssuance*				
	(1)	(2)	(3)	(3)
1[*Sensitive*] × *Post*	-7.141***	-6.726***	-6.726***	-4.230***
	(0.694)	(0.720)	(0.721)	(0.801)
Controls	No	No	No	Yes
Fixed Effects:				
Firm	Yes	Yes	Yes	Yes
Standard	Yes	Yes	Yes	Yes
Firm × *Standard*	No	Yes	Yes	Yes
Year	No	No	Yes	Yes
R^2	0.5234	0.7776	0.7776	0.8206
Obs.	383,194			

Table 9. Accounting Standards and Internal Sources of Funds

This table presents staggered difference-in-differences estimates of the effect of accounting standards on the use of internal sources of funds for standards that became effective between 1996 and 2010. The unit of observation is firm-year and the event window is restricted to the year immediately preceding and the year immediately following the standard becoming effective. Panel A presents estimates of the effect on cash holdings. $\ln Cash$ is defined as the ratios of cash and equivalents to total assets. Panel B presents estimates of the effect on assets sales. $\ln AssetSales$ is the natural log of the cash in-flows from assets sales. Panel C presents estimates of the effect on payout. $\ln Payout$ is the natural log of the sum of total dividends and share repurchases. $1[Sensitive]$ is an indicator that equals one if any of the borrower's 10-K filings from before the effective date mention the accounting standard and zero otherwise. *Post* is an indicator that equals one for loans initiated after the accounting standard becomes effective and zero otherwise. We include increasingly restrictive fixed effects focus identifying variation on within standard and firm variation in loan amounts over time. Controls include the natural log of total assets, market-to-book ratio, and the cumulative effect of accounting changes on earnings (Shroff 2017). Heteroskedasticity-robust standard errors are clustered by firm, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

Panel A. Cash holdings

Dependent variable: $\ln Cash$				
	(1)	(2)	(3)	(4)
$1[Sensitive] \times Post$	-2.383***	-1.349***	-1.348***	-1.274***
	(0.358)	(0.362)	(0.362)	(0.385)
Controls	No	No	No	Yes
Fixed Effects:				
Firm	Yes	Yes	Yes	Yes
Standard	Yes	Yes	Yes	Yes
Firm × *Standard*	No	Yes	Yes	Yes
Year	No	No	Yes	Yes
R^2	0.8508	0.9618	0.9618	0.9628
Obs.	593,251			

Panel B. Asset sales

Dependent variable: ln*AssetSales*				
	(1)	(2)	(3)	(4)
1[*Sensitive*] × *Post*	1.996***	1.886***	1.886***	1.116***
	(0.359)	(0.359)	(0.358)	(0.348)
Controls	No	No	No	Yes
Fixed Effects:				
Firm	Yes	Yes	Yes	Yes
Standard	Yes	Yes	Yes	Yes
Firm × *Standard*	No	Yes	Yes	Yes
Year	No	No	Yes	Yes
R^2	0.4885	0.7566	0.7567	0.7612
Obs.	453,624			

Panel C. Total dividends plus share repurchases

Dependent variable: ln*Payout*				
	(1)	(2)	(3)	(4)
1[*Sensitive*] × *Post*	-3.360***	-2.616***	-2.617***	-2.256***
	(0.515)	(0.538)	(0.538)	(0.651)
Controls	No	No	No	Yes
Fixed Effects:				
Firm	Yes	Yes	Yes	Yes
Standard	Yes	Yes	Yes	Yes
Firm × *Standard*	No	Yes	Yes	Yes
Year	No	No	Yes	Yes
R^2	0.7700	0.9255	0.9255	0.9319
Obs.	342,602			

Table 10. Accounting Standards and Investment

This table presents staggered difference-in-differences estimates of the effect of accounting standards on investment for standards that became effective between 1996 and 2010. The unit of observation is firm-year and the event window is restricted to the year immediately preceding and the year immediately following the standard becoming effective. Panel A presents estimates of the effect on capital expenditures. $\ln CAPEX$ is defined as the natural log of capital expenditures. Panel B presents estimates of the effect on research and development expenditures. $\ln R\&D$ is defined as the natural log of research and development expenditures. Panel C presents estimates of the effect on acquisition expenditures. $\ln Acquisition$ is defined as the natural log of research and development expenditures. $1[Sensitive]$ is an indicator that equals one if any of the borrower's 10-K filings from before the effective date mention the accounting standard and zero otherwise. *Post* is an indicator that equals one for loans initiated after the accounting standard becomes effective and zero otherwise. We include increasingly restrictive fixed effects focus identifying variation on within standard and firm variation in loan amounts over time. Controls include the natural log of total assets, market-to-book ratio, and the cumulative effect of accounting changes on earnings (Shroff 2017). Heteroskedasticity-robust standard errors are clustered by firm, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

Panel A. Capital expenditures

Dependent variable: $\ln CAPEX$				
	(1)	(2)	(3)	(4)
$1[Sensitive] \times Post$	-4.060***	-3.615***	-3.616***	-2.409***
	(0.273)	(0.273)	(0.273)	(0.317)
Controls	No	No	No	Yes
Fixed Effects:				
Firm	Yes	Yes	Yes	Yes
Standard	Yes	Yes	Yes	Yes
Firm × *Standard*	No	Yes	Yes	Yes
Year	No	No	Yes	Yes
R^2	0.9124	0.9795	0.9796	0.9828
Obs.	387,132			

Panel B. Research and development expenditures

Dependent variable: ln*R&D*				
	(1)	(2)	(3)	(4)
1[*Sensitive*] × *Post*	-2.486***	-2.149***	-2.149***	-0.891***
	(0.238)	(0.221)	(0.221)	(0.245)
Controls	No	No	No	Yes
Fixed Effects:				
Firm	Yes	Yes	Yes	Yes
Standard	Yes	Yes	Yes	Yes
Firm × *Standard*	No	Yes	Yes	Yes
Year	No	No	Yes	Yes
R^2	0.9374	0.9906	0.9907	0.9927
Obs.	222,154			

Panel C. Acquisition expenditures

Dependent variable: ln*Acquisition*				
	(1)	(2)	(3)	(4)
1[*Sensitive*] × *Post*	-5.217***	-4.415***	-4.414***	-2.495***
	(0.708)	(0.756)	(0.756)	(0.927)
Controls	No	No	No	Yes
Fixed Effects:				
Firm	Yes	Yes	Yes	Yes
Standard	Yes	Yes	Yes	Yes
Firm × *Standard*	No	Yes	Yes	Yes
Year	No	No	Yes	Yes
R^2	0.5204	0.7815	0.7815	0.7927
Obs.	362,212			