Information Regulation, Financing, and Investment

Abstract

By allowing investors to efficiently allocate capital, developed financial markets promote economic growth. We revisit a key component of financial market development, namely financial reporting standards, to identify a channel underpinning this link. We focus on introductions of new financial reporting standards and construct a novel text-based, firm-level measure of sensitivity to these standards. Relative to insensitive firms, sensitive firms reduce securities issuance by 11.4% and, despite compensating with internal sources of funds, cut investment by 10.8% after standards. These findings demonstrate that new standards trigger a substantial reallocation of capital through financial markets.

JEL Classification: G21, G28, G32, M41

Keywords: capital markets regulation, capital allocation, financing, investment

1 Introduction

An important motivation for financial markets research and policy is the established link between financial market development and economic growth (King and Levine 1993; Levine and Zervos 1998; Guiso, Sapienza, and Zingales 2004; Bekaert, Harvey, and Lundblad 2005; Levine 2005). The quality of financial reporting standards within a country has been identified by prior work as a key component of financial market development that is consistently associated with economic growth (Rajan and Zingales 1998; Levine, Loayza, and Beck 2000; La Porta, Lopez-de-Silanes, and Shleifer 2006). Theoretically, this link follows from arguments made by Schumpeter (1911), and expanded upon by Rajan and Zingales (1998), that developed financial markets allow investors to efficiently reallocate capital to its best use. This point has been used to support modern policymaking. For example, Arthur Levitt, the former chairman of the Securities Exchange Commission (SEC), which is tasked with creating, interpreting, and enforcing disclosure regulation, stated that "high quality accounting standards… reduce capital costs (Levitt 1998)." In this paper, we extend the literature on the finance-growth nexus using firm-level data and a new measure of sensitivity to financial reporting standards to uncover and quantify a capital allocation channel through which standards foster economic growth.

In the United States, the SEC provides the Financial Accounting Standards Board (FASB) with authority over a key aspect of information regulation: the creation of accounting standards. The resulting web of accounting standards determines the aggregation technology that transforms millions of corporate transactions into summary accounting measures, such as

earnings. The goal of this paper is to use the setting of accounting standards to identify a mechanism underlying the finance-growth relationship. To do so, we estimate the effects of information regulation on capital allocation across firms using the implementation of FASB standards as quasi-natural experiments.[1] To identify treatment and control groups, we introduce a novel firm-specific measure of ex ante sensitivity to individual standards based on voluntary disclosures that mention the standard after it is publicly proposed but before it becomes effective. The key benefit of our measure is that it is commonly defined across standards, but varies at both the standard and firm levels. We interpret pre-effective date references to individual standards as signals that subsequently disclosed financial information will be materially affected by the new standard (Skinner 1994; Healy and Palepu 2001; Balakrishnan, Billings, Kelly, and Ljungqvist 2014; Billings, Cedergren, and Dube 2016). Using a generalized difference-in-differences design, we use our cross-sectional measure of sensitivity to estimate the *average* effect of a new standard on securities issuance, internal sources of funds, and investment.

Our study uncovers several new findings consistent with new accounting standards triggering a substantial reallocation of capital through financial markets. First, relative to insensitive firms, sensitive firms reduce issuance of new equity and debt following a new standard. These effects are economically large; for example, on average, sensitive firms reduce

[1] Prior work has studied context-dependent effects of individual FASB standards, including the compensation effects of standards concerning stock option expensing and the risk-taking effects of standards concerning fair value adjustments (Carpenter, Stanton, and Wallace 2010; Loutskina 2011; Huizinga and Laeven 2012; Milbradt 2012; Anantharaman and Lee 2014; Hasan, Hoi, Wu, and Zhang 2014; Bakke, Mahmudi, Fernando, and Salas 2016; Glover and Levine 2017; Shue and Townsend 2017; Jochem, Ladika, and Sautner 2018; Brown, Gredil, and Kaplan 2019).

total securities issuance by 11.4% relative to insensitive firms. These results highlight a key mechanism through which information regulation affects firm outcomes. Namely, new standards may separate good type and bad type firms that were previously able or forced to pool on non-disclosure of value-relevant items (e.g., Fishman and Hagerty 1989; Diamond and Verrecchia 1991). Investors rationally respond to this revelation by reallocating capital away from bad type firms and toward good type firms. Our difference-in-differences estimates are therefore well-suited to estimate the reallocation of capital between sensitive and insensitive firms. However, an important caveat is that our design is less well-suited to testing another important implication of theory that the aggregate cost of capital should fall when risk-averse investors face less uncertainty about firm type.

Our second main finding is that firms respond to decreased external financing opportunities by finding alternative sources of funds. In particular, sensitive firms are 1.6 percentage points (8.3%) more likely to sell assets and reduce payout by 9.0%, both by cutting dividends and by reducing repurchases. Despite these economically significant behavioral responses, these alternative sources of funds make up for less than 20% of the reduction in securities issuance for the average firm.

Given the net decline in available resources for sensitive firms relative to insensitive firms, we next investigate the investment response of these firms to new standards. Our third main finding is that the relative investment growth of sensitive firms drops following accounting standards, consistent with an increase in financial constraints. Our most restrictive

specifications estimate that sensitive firms reduce capital expenditures by 11.8%, research and development expenditures by 4.7% and acquisition expenditures by 5.1%. Together, these main findings suggest that the *average* standard's introduction has meaningful real effects on firms due to capital reallocation in primary debt and equity markets.

To address potential measurement and endogeneity concerns with our empirical design, we conduct a series of additional tests. Using three particular standards, 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, respectively, as would be expected given the topic of each standard. These results suggest that our ex ante measure of sensitivity has the desirable properties of being applicable to all standards and predicting the ex post sensitivity of firms to standards. Additionally, we provide evidence of substantial within-firm variation in sensitivity across standards; for example, fixed firm factors explain only 11.4% of the variation in sensitivity, while firms are, on average, sensitive to 24.2% of standards. This suggests that firms cannot be easily classified into *fixed* information-sensitive and information-insensitive groups, mitigating concerns that firms' economic conditions or information environments can explain our findings.

We also address several potential identification concerns related to the choice to introduce a new standard and anticipation by affected firms or market participants. One such concern may be that new standards are a response to the rise of specific reporting concerns. Even though we cannot fully eliminate this concern, we believe it is unlikely to be important for

the interpretation of our findings for several reasons. The first reason is that we measure the effects of the standard when changes to audited financials become effective. Due to the length of the standard setting process, this typically occurs more than two years after introduction of the potential standard, and therefore well after the concerns that may have led to the standard in the first place. This helps to rule out alternative mechanisms related to the timing of standards. The second reason is that we include firm-by-standard fixed effects in our preferred specifications. Using firm-by-standard fixed effects means that we use only within-firm variation in outcomes before versus after each standard becomes effective. This framework mitigates endogeneity concerns related to the selection of standards or the possibility that particular standards are purposefully designed to affect certain firms. Of course, we must also consider parallel trends for the two main outcomes we study – total issuance and total investment – and we find no systematic evidence of differences in trends for sensitive and insensitive firms before standards become effective. Statistically, this is unsurprising given the frequency of accounting standards introductions and given the lack of persistence in sensitivity at the firm level. This should nonetheless mitigate concerns about anticipation.

Overall, our findings are relevant to ongoing policy debates and establish a financial markets channel through which information regulation has real effects. This channel is directly related to the finance-growth nexus, which is largely documented in prior cross-country studies (Rajan and Zingales 1998, 2001; Carlin and Mayer 2003; Levine 2005; LaPorta, Lopez-de-Silanes, and Shleifer 2006). Levine (2005) argues that one important purpose of financial systems is "…to produce information ex ante about possible investments and allocate capital."

Indeed, Rajan and Zingales (1998) propose accounting standards as a measure of the degree of capital markets sophistication and find evidence that country-level variation in accounting standards is strongly correlated with economic growth.[2] Using this variation in accounting standards sophistication, La Porta, Lopez-de-Silanes, and Shleifer (2006) provide "…strong evidence that laws mandating disclosure … benefit stock markets." Relative to this literature, we propose a new measure of financial market development based on accounting standards. Our measure captures the sensitivity of firms to each new accounting standard. This granular variation allows us to study cross-sectional exposure to policy updates rather than endogenous accounting choices, incorporate firm-by-standard and industry-by-quarter fixed effects that rule out various alternative explanations, and exploit a controlled institutional environment that mitigates concerns about reverse causality.

Our work is also the broader literature on the real effects of disclosure regulation (Leuz and Wysocki 2016). The papers in this literature that are most closely related to ours are Bushee and Leuz (2005), Greenstone, Oyer, Vissing-Jorgensen (2006), and Fernandes, Lel, and Miller (2010), which study market responses and liquidity effects of new and expanded SEC regulation, and Cumming, Johan, and Li (2011), Christensen, Hail, and Leuz (2016), and Breuer, Hombach, and Muller (2018), which focus on the effects of European Union directives on market liquidity and credit market structure. Relative to this literature, we exploit a new

[2] Further, Carlin and Mayer (2003) find that few structural characteristics of a country are as important as "accounting standards for R&D in skill- and equity-dependent industries."

source of identifying variation in mandatory disclosure and contribute new firm-level evidence on the real effects of these policies.

A related literature does investigate the real effects of disclosure policy, but typically does so in context-dependent settings. For example, Cho (2015) and Jayaraman and Wu (2019) study the investment efficiency consequences of mandatory segment reporting as imposed by SFAS 131. Other studies have focused on the compensation effects of standards concerning stock option expensing and the risk-taking effects of standards concerning fair value adjustments (Carpenter, Stanton, and Wallace 2010; Loutskina 2011; Huizinga and Laeven 2012; Milbradt 2012; Anantharaman and Lee 2014; Hasan, Hoi, Wu, and Zhang 2014; Bakke, Mahmudi, Fernando, and Salas 2016; Glover and Levine 2017; Shue and Townsend 2017; Jochem, Ladika, and Sautner 2018; Brown, Gredil, and Kaplan 2019). Because of their emphasis on policy evaluation, the economic message of these papers largely depends on the content of the individual regulatory events studied.[3] By their very nature, new standards are different from the standards that came before, so they tend to include an idiosyncratic component. Because we estimate *average* effects across all standards, our findings are informative about the systematic features of the regulatory process, and, in turn, convey potentially valuable insights about future standards or regulation coming from this process. Our evidence on the systematic effects of accounting standards has implications for prior research on individual standards that mix these idiosyncratic and systematic effects.

[3] One exception is Shroff (2017), which explores the relationship between corporate investment and the cumulative effects of firm-level accounting choices on earnings.

2 Institutional Background, Data, and Methodology

2.1 Institutional Background

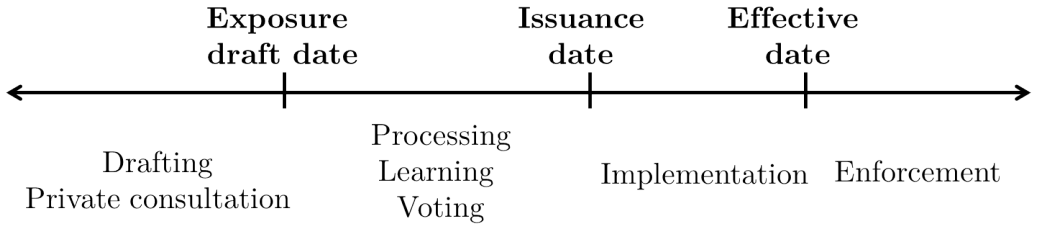
The SEC has overall responsibility for regulation of US capital markets, though it has, since 1973, delegated the administration of accounting rules to the accounting profession via the Financial Accounting Standards Board (FASB). The FASB is a private organization headquartered in Norwalk, Connecticut, 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 "establish and improve financial accounting and reporting standards to provide useful information to investors and other users of financial reports."[4] In line with the theoretical rationales for public disclosure and comparability in disclosure, FASB's Rules of Procedure specifically state that "decisions about the allocation of resources rely heavily on credible, concise, and understandable financial information." Furthermore, the 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

[4] https://www.fasb.org/facts/index.shtml

FASB's work, meaning the cost of preparing, providing, and auditing financial information is explicitly considered.

As can be seen in the timeline below, the standard setting process involves a series of steps. The board identifies a topic it believes new standards could improve upon, 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 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

Figure 1 presents a time series bar plot of the frequency of FASB standards, which highlights the post-2000 subsample of standards in our estimation sample in gray. There is considerable intertemporal variation in exposure draft frequency during our sample period, with

a peak of eight standards issued in 2005. The frequency of new standards decreased after the 1980s, perhaps 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 (ASU). An ASU is structurally similar to an SFAS and follows approximately the same timeline and adoption 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). We use quarterly data on equity and debt issuance, asset sales, dividends, repurchases, capital expenditures, research and development expenditures, and acquisition expenditures from COMPUSTAT. Because 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 2000 and 2010. We also restrict the sample to standards for which we have complete data on exposure draft, issuance, and effective dates. As a result of these data requirements, we have a total of 21 standards in our main estimation sample.

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. Consistent with the disclosure requirements of SAB 74, which indicate that firms must disclose elements that it believes to be necessary to understand its financial condition and results of operations, we interpret a firm's mentioning of a standard in its 10-K as a voluntary disclosure about its sensitivity to that standard.[5] We collect standard mentions (e.g., "SFAS 123R") from 10-K filings. Our primary measure of treatment is *Sensitive*, which is an indicator variable that equals one if the firm mentions the standard at least once before the standard becomes effective and zero otherwise.[6]

[5] SAB 74, Interpretive Response to Question 1.

[6] We investigate the intensive margin of sensitivity in Section 5 and find evidence consistent with our extensive margin tests (i.e., using the indicator variable *Sensitive*).

Panel A of Table 1 presents summary statistics. As the mean value of *Sensitive* indicates, 24.2% of firms mention the standard at least once before its effective date.[7] Among sensitive firms, the average number of pre-effective date 10-K mentions of a particular standard is 13.3. Firm characteristics are consistent with prior work. The average firm-quarter observation in our estimation sample has total issuance of $46.9 million and total investment of $25.0 million, respectively.

Panel B of Table 1 presents evidence of covariate balance across sensitive and insensitive groups in the quarter before the issuance date of each standard. Although the identification assumption required for our difference-in-differences design is that sensitive and insensitive groups have parallel pre-standard trends in characteristics, it would be reassuring if sensitive and insensitive groups were also similar in pre-standard *levels* across characteristics. We investigate pre-standard balance across sensitive and insensitive groups for each of our outcome variables of interest as well as two baseline firm characteristics, size and age. Panel B of Table 1 shows that, with one exception, sensitive and insensitive groups are statistically indistinguishable across these covariates. Sensitive firms have higher levels of capital expenditures ($p = 0.029$) in the quarter before the issuance dates of standards. However, they are not statistically different in total investment ($p = 0.368$), suggesting that the difference in capital expenditures may be spurious. From Panel B of Table 1, we cautiously conclude that

[7] This extent of sensitivity disclosure is consistent with case study evidence from FIN 48 (Alexander, Ettredge, Stone, and Sun 2011), and the fact that SAB 74 compliance has been a recent point of emphasis for regulators (https://www.thecaq.org/sites/default/files/caq_alert_2017-03_focus_on_disclosures_new_accounting_standards.pdf).

sensitive and insensitive firms are similar before the relevant standard becomes effective, supporting the external validity of our findings.

2.3 Measurement of *Sensitive*

An ideal measure of treatment by new financial reporting standards should be applicable to all 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 introduce a new measure of ex ante sensitivity to standards that is derived from direct mentions of in-process standards in corporate filings. Because companies mention standards in a common format and mentions occur in measurable quantities for every accounting standard, this measure can be applied to all accounting standards. The measure is ex ante by construction as we utilize only mentions that occur before the effective date of a new standard. In practice, on average, only a minority of companies mention new accounting standards, consistent with our interpretation of mentions as voluntary disclosures or warnings about the sensitivity to upcoming changes. Thus, our standard-specific measure of sensitivity is general to all firms and standards, requires no judgment by researchers, and does not rely on ex post outcomes.

Our measure of sensitivity is subject to two types of selection on the part of firms disclosing about the standard in their 10-Ks. The first concern is that firms strategically report their sensitivity to standards. Firms are known to disclose strategically in a variety of settings

(Schrand and Walther 2000; Lougee and Marquardt 2004; and Li 2008), typically in order to derive some benefit. However, since in our setting the outcomes for disclosing firms are predominantly negative, there is much less incentive for strategic disclosure. The second concern regards firms that state explicitly that they are not affected by the standard, so mentioning the standard despite their financials not being materially affected. This form of selection should attenuate our findings since it pushes firms which should have been in the control group into the treatment group.

Another potential concern regarding our measure is that standards are not introduced randomly. Indeed, the issuance of new standards may be a response to the rise of specific reporting concerns. This should naturally predict a change in behavior *before* the standard is discussed, let alone issued and made effective. This is another reason we use the effective date to measure the informational effects of new standards; this choice helps to minimize the influence of alternative mechanisms on our results. Moreover, this type of anticipation by market participants should lead to a violation of the parallel trends assumption. Indeed, we show empirical evidence of parallel trends in corporate outcomes in Sections 3.1 and 4.1, which mitigates this and related concerns.

To investigate variation in sensitivity across firms and standards, we provide a variance decomposition of our measure of sensitivity.[8] First, Panel A of Figure 2 presents a histogram of the proportion of standards to which each firm is sensitive during the period the firm is in the

[8] We note that our difference-in-differences methodology, presented in Section 2.4, isolates within-firm variation in outcomes around each accounting standard using firm by standard pairwise fixed effects. This further alleviates concern that potential contaminants of our sensitivity measure, like differences in firm quality, drive our estimates.

sample. The histogram shows that sensitivity is not persistent within firm; the average firm is sensitive to 24.2% of the standards and the cross-sectional standard deviation is 14.4%. The 99th percentile of the distribution of firm-level sensitivity across standards is 60%, meaning the distribution is bounded well below 100% and very few firms are persistently sensitive to all reporting standards.

Second, we decompose the variance of *Sensitive* using regression analysis. In particular, we are interested in the fraction of variation in *Sensitive* that 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 untabulated tests, we regress *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 *Sensitive* that can be explained by time-invariant firm factors is large, then 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 11.4% of variation in sensitivity, suggesting that persistent cross-sectional firm heterogeneity is not driving our β_3 estimates. In contrast, 33.8% 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 individual accounting standards. The residual variation that we use as identifying variation in our empirical tests is attributable to firm factors that are specific to individual standards.

To further validate *Sensitive* as a discriminator for treatment by accounting standards, 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 *Sensitive* to such ex post measures using SFAS 123R, SFAS 142, and SFAS 157 as case studies because these three accounting standards are significant and related to specific items that can be reliably evaluated in large samples.[9]

SFAS 123R (2005) required that firms expense granted stock options at fair value, rather than intrinsic value, affecting both the compensation landscape and 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 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 a lot of 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.

[9] We study ex post outcomes for two reasons. First, observing, for example, fair value adjustments before the effective date of SFAS 157 requires rare pre-adoption. Second, ex post outcomes incorporate the deterrence effect of new standards on behavior. Therefore, using this measure allows us to test whether *Sensitive* anticipates future firm behavior. For example, stock options use decreased after SFAS 123R (Carter, Lynch, and Tuna 2007).

To test this hypothesis, we present three tables in Appendix B that link our measure of ex ante sensitivity to standard-specific ex post measures in the cross-section of firms. In Table B1, we link the total number of options granted and options outstanding at fiscal year-end to *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 B2 we link the value of the goodwill account to *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 B3 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 applicable across accounting standards and linked to the ex post consequences of accounting standards.

2.4 Empirical Methodology

To estimate the average treatment effect of accounting standards, we rely on a generalized difference-in-differences methodology using the stacked event study approach, following Gormley and Matsa (2011). Notably, several recent papers have shown renewed interest in related methods, even documenting potential estimation bias and inference problems (Borusyak and Jaravel 2017; Abraham and Sun 2019; Goodman-Bacon 2019). Because our novel

measure of sensitivity varies across standards for each firm, providing a control group for each standard or "cohort," our methodology sidesteps most of these inference problems.[10]

The following is our preferred specification:

$$Y_{ist} = \alpha + \beta Sensitive_{is} \times Post_{st} + u_{i \times s} + v_{n \times t} + \varepsilon_{ist} \qquad (1)$$

where i, n, s, and t denote firm, industry (i.e., two-digit SIC), standard, and quarter, respectively. $Sensitive_{is}$ 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.[11] $Post_{st}$ is an indicator that equals one if quarter t is after the effective date of standard s and zero otherwise. Y_{ist} denotes one of our dependent variables of interest. Our outcomes of interest include securities issuance, internal sources of funds, and investment. In our main tests, we present estimates using event windows with four quarters before the issuance date of standard s and four quarters after the effective date of standard s. We exclude the interim period because, for a subset of standards, firms can voluntarily pre-adopt the standard. In robustness tests, we present estimates using alternative event windows with as few as two quarters and as many as twelve quarters from each of the pre and post periods.

[10] Our methodology is not subject to concerns that would be relevant to other event study difference-in-differences models in which the control group for each cohort is comprised of other subsequently or previously treated firms (Abraham and Sun 2019). Similarly, our methodology differs from staggered panel difference-in-differences models in which all firms eventually become treated by a homogeneous treatment at random times (Borusyak and Jaravel 2017). Goodman-Bacon (2019) shows that estimation bias in generalized difference-in-differences methods can arise from variation in treatment across cohorts or from differences in cohort sizes. The former source of bias is not relevant in our case because our measure of sensitivity allows firms to be either treated or control units in each cohort. We investigate the latter source of bias in Section 5.3.

[11] Because in some years there are multiple new standards, firms could be sensitive to multiple standards in one year. This means that our main independent variable of interest can be interpreted as an index of sensitivity across standards. In robustness tests, we provide evidence using a continuous measure of the number of mentions.

We include a restrictive set of fixed effects: $u_{i\times s}$ and $v_{n\times t}$ denote firm by standard pairwise and industry by quarter fixed effects, respectively. The inclusion of industry by quarter fixed effects means that coefficient estimates are identified using variation from the cross-section of firms from the same industry in a given event quarter (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 standard. Moreover, if there is any endogeneity driving standard selection or which firms are affected by particular standards, firm by standard fixed effects eliminate that variation from our tests. This means that we use only within-firm variation in outcomes before versus after 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 firm-level panel; 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 quarter 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 our tests, Y is observed at the firm-standard-quarter level, so our estimation sample includes duplicates of each firm i-quarter t observation if multiple standards become effective in quarter t. This means that our main estimation sample

has implicit regression weights that emphasize firm-quarter observations from quarters in which multiple standards become effective. We account for these implicit weights in the estimation of standard errors by double clustering at the firm and quarter levels. In Section 5.3, we analyze the impact of these implicit regression weights on our coefficient estimates in robustness tests. To do so, we estimate our main specifications with an explicit regression weight that is the inverse of the number of standards effective in a quarter.

An important caveat of our generalized difference-in-differences design in this setting is that the control firms may be indirectly affected by new accounting standards, such as through product market interactions with directly affected treated firms. Because our design cannot account for these indirect effects, we believe that our estimates will be attenuated.[12] Moreover, although our design is ideally placed to estimate the relative effects of standards on sensitive and insensitive firms, it is not designed to estimate aggregate effects around the implementation of new standards. For example, aggregate changes in issuance or investment around standards may be conflated by other time-varying economic conditions or policies.

3 Financing and Information Regulation

3.1 Visual Evidence

We first graphically investigate the difference in issuance between sensitive and insensitive firms following new accounting standards. In Panel A of Figure 3, we plot the

[12] Another caveat is that accounting standards could potentially lead firms to list or delist (Fernandes, Lel, and Miller 2010). The most obvious form of this selection would involve delisting by the worst of the sensitive firms. In light of our findings, this would also be attenuating.

difference in total issuance, the sum of new equity and debt issues, between sensitive and insensitive firms, relative to the corresponding value 12 quarters (three years) before that standard becoming effective. We also include 95% confidence intervals, which are derived using standard errors that are double clustered by firm and standard, and show that in the pre-period, there is no statistically significant difference between these groups. In the two quarters immediately prior to the effective date, the difference is quantitatively zero as well. However, in the three quarters immediately following, sensitive firms do roughly 40% less issuance when compared to insensitive firms. This difference falls over time, eventually returning to no quantitative difference after ten quarters. Because there is no rebound in the relative difference in total issuance, Panel A of Figure 3 shows that this relative loss of issuance is not subsequently recouped by sensitive firms, i.e. there is not just a delay in raising capital for sensitive firms but a real decline.

3.2 Issuance

To verify that our graphical findings hold in a multivariate setting, we estimate equation (1) in which Y_{it}, the dependent variable, is one of the following: total issuance, equity issuance, or debt issuance. Note that we do not include firm outcomes as controls in most of our financing tests as these outcomes are endogenous to financing decisions and may be "bad controls," which could confound estimates.[13]

[13] See Griliches and Mason (1972) for the seminal exploration of this issue in the effects of education literature.

Table 2 reports the relative change in total debt and equity issuance for sensitive firms after a new standard becomes effective. The results in column (1) show that, after controlling for quarter fixed effects, sensitive firms reduce equity issuance by 15.7% following the effective date for new standards. If we isolate within-firm variation in outcomes around each accounting standard by using firm by standard fixed effects, as in column (2), this effect falls to an 11.8% decrease. Note that this design alleviates the concern that omitted factors, such as differences in quality among firms over time, drive our estimates. This decline in issuance is quantitatively similar if we also include quarter or industry by quarter fixed effects, as in columns (3) and (4).

Table 3 separately evaluates changes in equity issuance and debt issuance to examine whether there are differing effects across financial markets. In Panel A, we find that sensitivity to a standard is associated with a 10.7% relative decrease in equity issuance when controlling for quarter fixed effects in column (1), or a decrease of 7.9% when we also control for firm by standard fixed effects. In Panel B, we find similar effects for debt issuance. Specifically, in column (1) we find that debt issuance falls by 8.4% for sensitive firms, or roughly 6.2% if we only use within-firm variation surrounding each standard. Overall, these results are consistent with accounting standards inducing a negative revelation to markets about sensitive firms and firms rationally responding by decreasing issuance. This effect is slightly more pronounced in equity markets, but the effect on debt issuance is economically meaningful as well.

3.3 Other Sources of Funds

The previous section provides evidence that accounting standards increase the cost of both equity and debt financing for sensitive firms. In this section, we explore whether sensitive firms offset this reduced access to equity and debt markets with other sources of funds, such as selling assets or cutting payout to shareholders. We again use equation (1), replacing Y with an indicator variable for whether the firm did an asset sale or not, total payout, dividends, or repurchases. Combined with equity and debt issuance, these outcome variables provide a picture of the sources of funds for sensitive firms when compared to insensitive firms surrounding the implementation of new accounting standards.

Tables 4, 5, and 6 provide evidence that sensitive firms increasingly rely on alternative sources of funds. Table 4 shows the change in likelihood of engaging in an asset sale for sensitive firms relative to insensitive firms following a new accounting standard. Across our specifications, sensitive firms are roughly 1.6 percentage points more likely to use this strategy. This is an economically significant 8.3% relative increase when compared to the baseline incidence of asset sales of 19.7%.

In Table 5, we investigate whether sensitive firms decrease their total payout. In column (1), we find that sensitive firms decrease payout by 14.0% relative to insensitive firms when controlling for quarter fixed effects. This effect is roughly 9.0% in columns (2)-(4) in which we control for firm by standard fixed effects. In Table 6, Panel A, we focus on dividends alone, and find only a marginally significant decrease in dividends paid. However, in Panel B, we find that

the decrease in total payout is largely due to a decrease in repurchases. In our specifications with firm by standard fixed effects, we see an 8.1% decrease in repurchases by sensitive firms. These findings are not surprising given that cutting dividends could be a particularly negative signal to shareholders (Guay and Harford 2000; Michaely, Rossi, and Weber 2019). Overall, the results in Tables 5 and 6 indicate that sensitive firms keep more of their internally generated cash by significantly reducing payout in response to new accounting standards. This shift to other sources of funds only partially compensates for the lost access to external capital markets, making up for about 20% of the reduction in securities issuance for the average firm.

4 Investment and Information Regulation

A critical outcome of capital allocation is real investment. Whether the reshuffling of capital from sensitive to insensitive firms leads to a change in output depends on whether and how capital allocation affects the real side of the firm. 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, which will show up in our results as a relative decrease among sensitive firms.

4.1 Visual Evidence

We first graphically investigate the difference in total investment between sensitive and insensitive firms following new accounting standards. In Panel B of Figure 3, we plot the difference in total investment, the sum of capital expenditures, research and development

expenditures, and expenditures on acquisitions, between sensitive and insensitive firms. We plot this difference relative to the corresponding value 12 quarters (three years) before that standard becoming effective. In the pre-period, there is no statistically significant difference between these groups. These underlying confidence intervals are constructed using standard errors that are double clustered by firm and standard. In the five quarters immediately following the effective date of a new standard, sensitive firms do roughly 10-15% less investment when compared to insensitive firms. This difference gets smaller over time, eventually falling to zero after ten quarters. Because there is no rebound in the relative difference in total investment after that point, Figure 3 implies that sensitive firms' lost investment is not recouped later.

4.2 Investment

We now verify our graphical evidence by estimating equation (1) in which Y_{it}, the dependent variable, is one of the following: total investment, capital expenditures, research and development expenditures, or expenditures on acquisitions. Table 7 provides evidence that, relative to insensitive firms, sensitive firms reduce investment after new accounting standards. This reduction in investment occurs despite a shift to alternative sources of funds, suggesting again that the net effect of the financing changes discussed in Section 3 was to significantly increase the cost of external financing for sensitive firms relative to insensitive firms. Our preferred estimates suggest that the reduction in total investment is 10.8%, which is similar to the reduction implied in Figure 3.

In Table 8, we break down total investment into its three components. In Panel A, we find that capital expenditures fall by roughly 12.0% when using only within-firm and standard variation to identify changes in investment. In Panel B, we investigate research and development and find a slightly smaller decrease of 4.7%. Similarly, in Panel C, we show that the amount spent on acquisitions falls by 5.1% for sensitive firms. Overall, sensitive firms appear to reduce investment generally, with a particularly large decrease occurring in capital expenditures.

These estimates are obtained while controlling for firm-by-standard and industry-by-quarter fixed effects. In combination with our other findings on primary capital markets and alternative sources of funds, these results are consistent with new accounting standards producing a negative revelation about sensitive firms, and causing capital to be reallocated toward insensitive firms.

5 Robustness

5.1 Standard Heterogeneity

Because we study the implementation of many different standards covering a variety of different types of information, it is useful to check for possible heterogeneity in the financing and investment effects of these standards. For example, the conclusions one would draw from the results presented in Sections 3 and 4 would likely be different, and would certainly be less general, if they were being driven by a single particular standard. The simplest way to address

this issue is to re-run our analysis for the two main outcomes we study, total issuance and total investment, systematically dropping one standard at a time. By individually excluding standards from our analysis, we can understand the degree to which decreases in issuance or decreases in investment are driven by individual standards.

In Figure 4, the top row of plots consists of histograms for the coefficient on *Sensitive* from equation (1), first for total issuance and then for total investment; these estimates correspond with column (1) of Table 2 and Table 7, respectively. In the bottom row of plots, we show histograms for the corresponding *t*-statistics for these coefficient estimates. What we find in both cases is a unimodal distribution with a small spread around the baseline coefficient. The coefficients remain negative, indicating that there are no outlier standards that are either driving our findings or otherwise meaningfully reducing the impacts we find. Furthermore, the *t*-statistics are stable and are all above the 95% significance level. Overall, this table shows that our findings are robust to the exclusion of particular events and is consistent with all of the regulatory outcomes emanating from the same process in our sample yielding reasonably comparable economic effects.

5.2 Event Windows

Our multivariate tests up to this point use event windows of four quarters, effectively comparing the year following the effective date for new accounting standards with the year preceding the issuance data. Of course, Panels A and B of Figure 3 show that the greatest

impacts on both total issuance and total investment occur in the quarters immediately following the implementation of a new standard. However, we are interested in investigating whether our general findings are robust to alternative event windows. We now implement our most restrictive fixed effects specifications, but with different windows.

In Table 9, Panel A, we estimate equation (1) with $\ln TotalIssuance$ as the dependent variable. Our specification includes both firm by standard and industry by quarter fixed effects, which corresponds to estimates in column (4) of Table 2. Beginning in column (1), we perform our tests with event windows of two, four, six, eight, and twelve quarters, meaning that the estimate in column (2) corresponds to the same estimate in Table 2. In keeping with the visual evidence from Figure 3, using a shorter event window isolates quarters in which the decrease in total issuance is greatest, and we find an effect on the order of 12.7%. If we extend the event window to more quarters, the effect monotonically decreases though remains statistically significant, falling to 7.4% for a twelve quarter window.

In Panel B, the specification again is our most restrictive, and corresponds to estimates in column (4) of Table 7, where $\ln TotalInvestment$ is the dependent variable. The coefficients we estimate are similar across different event windows, ranging from a 3.4% decrease in total investment to a 10.8% decrease. Consistent with the visual evidence in Figure 3, the effects on total investment dissipate after eight quarters. Overall, these robustness tests illustrate both that the effects are persistent across long intervals and that there are real effects for sensitive firms relatively quickly after the effective date of a new standard. Moreover, these tests show that our findings are not dependent on the choice of window.

5.3 Regression Weights

Because we study 21 standards with effective dates in the years 2000-2010, there are quarters in our generalized difference-in-differences design for which there are multiple standards that have become effective in the previous four quarters. This effectively means that some firm-standard event blocks overlap in our sample. As a result, one may be interested in whether our results are robust to alternative weightings being placed on observations in quarters in which multiple standards have recently become effective.

In Table 10, Panel A, we estimate equation (1) with $\ln TotalIssuance$ as the dependent variable, but place sample weights of $1/n$, where n is the number of standards effective in the same quarter as the focal standard. This approach places less weight on the effects of a standard if it is happening concurrently with other standards, whereas our approach in the rest of our designs weights equally. This approach therefore underweights parts of the sample in which there is more standard-setting activity. We find, in our preferred and most restrictive specification, that total issuance falls by 11.1% for sensitive firms, when applying this alternative regression weighting scheme. This is roughly the same as our finding of an 11.4% decrease in column (4) of Table 2.

In Panel B, we estimate the same equation, now placing $\ln TotalInvestment$ as the dependent variable, again placing alternative sample weights of $1/n$, where n is the number of standards effective in the same quarter as the focal standard. We find, in our preferred and most restrictive specification, that total investment falls by 11.9% for sensitive firms, which is

similar to the unweighted estimate of 10.8% in column (4) of Table 7. Overall, these tests indicate that our findings are robust to alternative weightings of standards based on standard-setting activity and mitigate concern that particularly busy times for standard setters lead to new standards with different effects on capital allocation.

5.4 Treatment Intensity

Our main measure of treatment by information regulation, *Sensitive*, is an indicator that equals one if any of the firm's 10-K filings from before the effective date mention the standard by name and zero otherwise. While treating sensitivity as an indicator variable is useful for interpretation (a firm is either sensitive, or it is not), one may be interested in whether increased mentions of a standard are in fact associated with increased sensitivity to that standard. Further, one may be interested in whether this increased sensitivity leads to a greater response in terms of financing or investment. We define a new independent variable, ln*Sensitivity*, as the log of one plus the count of the number of mention of the accounting standard in the firm's 10-K filings before the effective date of the standard.

In Table 11, we now estimate equation (1) substituting ln*Sensitivity* for *Sensitive* in our regressions. In Panel A, our dependent variable is ln*TotalIssuance*, which means that we can interpret coefficients as percentage differences in total issuance given a 100% increase in sensitivity. If we control only for quarter fixed effects, as we do in column (1), we see that a 100% increase in sensitivity is associated with firms having 5.5% lower issuance. This effect falls to a 5.1% decrease if we include firm by standard fixed effects. In Panel B, our dependent

variable is $\ln TotalInvestment$, and we find that a 100% increase in sensitivity is associated with roughly 5% lower investment, whether we include firm by standard fixed effects or not. For an easier comparison with our main findings, we alternatively investigate the intensive margin in Table 12 by adding an interaction between *Post* and *HighSensitivity*, an indicator that equals one if the firm-standard observation is in the top quartile of standard mentions, conditional on having any mentions. In our preferred specifications, the evidence in Table 12 shows that especially sensitive firms experience approximately twice the decline in total issuance and investment as other sensitive firms.

6 Conclusion

We extend prior cross-country work on the finance-growth nexus by identifying and quantifying a channel underpinning this link. We revisit a key component of financial market development, namely financial reporting standards, and provide evidence that new standards trigger a substantial reallocation of capital through financial markets. To do so, we focus on introductions of new standards as quasi-natural experiments and construct a new measure of sensitivity to financial reporting standards that varies across standards for a given firm. Our evidence is best explained by a mechanism in which financial reporting standards induce a negative revelation about sensitive firms, and highlights a general economic mechanism that motivates most forms of information-based capital markets regulation. Because we focus on the systematic effects of new regulation across many policies, our findings are informative about the likely effects of future policies.

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Appendix A – Variable Definitions

Variable	**Definition**	**Source**
Total Assets	**atq**	COMPUSTAT
Age (yrs)	floor((**datadate** − **ipodate**)/365)	COMPUSTAT
Total Issuance	*Equity Issuance* + *Debt Issuance*	COMPUSTAT
Equity Issuance	**cshiq** × (0.5 × **prchq** + 0.5 × **prclq**)	COMPUSTAT
Debt Issuance	**dltisy** (adjusted by fiscal quarter)	COMPUSTAT
$1[$*Asset sales*$]$	Indicator that equals one if *Asset sales* > 0, zero otherwise.	COMPUSTAT
Asset sales	**sppivy** (adjusted by fiscal quarter)	COMPUSTAT
Payout	*Dividends* + *Repurchases*	COMPUSTAT
Dividends	**dvy** (adjusted by fiscal quarter)	COMPUSTAT
Repurchases	**cshopq** × **prcraq**	COMPUSTAT
Total Investment	*CAPEX* + *R&D* + *Acquisitions*	COMPUSTAT
CAPEX	**capxy** (adjusted by fiscal quarter)	COMPUSTAT
R&D	**xrdq**	COMPUSTAT
Acquisitions	**aqcy** (adjusted by fiscal quarter)	COMPUSTAT
Sensitive	Indicator that equals one if firm i mentions standard s between the exposure draft and effective dates, zero otherwise.	Collected from 10-K filings and FASB
ln*Sensitivity*	Natural log of one plus the number of mentions of standard s in firm i's 10-K filings between the exposure draft and effective dates.	Collected from 10-K filings and FASB

Appendix B – Measurement Validation

Table B1. 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. *Sensitive* is an indicator that equals one if any of the firm'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)
Sensitive	8.334***	23.973***
	(2.515)	(4.073)
R^2	0.0010	0.0033
Obs.	11,422	

Table B2. 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. *Sensitive* is an indicator that equals one if any of the firm's 10-K filings from before the effective date mention "SFAS 142" 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)
Sensitive	200.931***
	(3.808)
R^2	0.1288
Obs.	9,847

Table B3. 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. *Sensitive* is an indicator that equals one if any of the firm'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)
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. Time Series of FASB Standards

This figure plots the annual frequency of FASB effective dates between the inception of the FASB in 1973 and the FASB's final standard in 2010, preceding the transition to the Accounting Standards Codification (ASC). The gray (white) bars represent standards that are included (excluded) from our data because collecting mentions of specific FASB standards in 10-K filings is feasible starting with the cohort of standards with exposure drafts released in 1996.

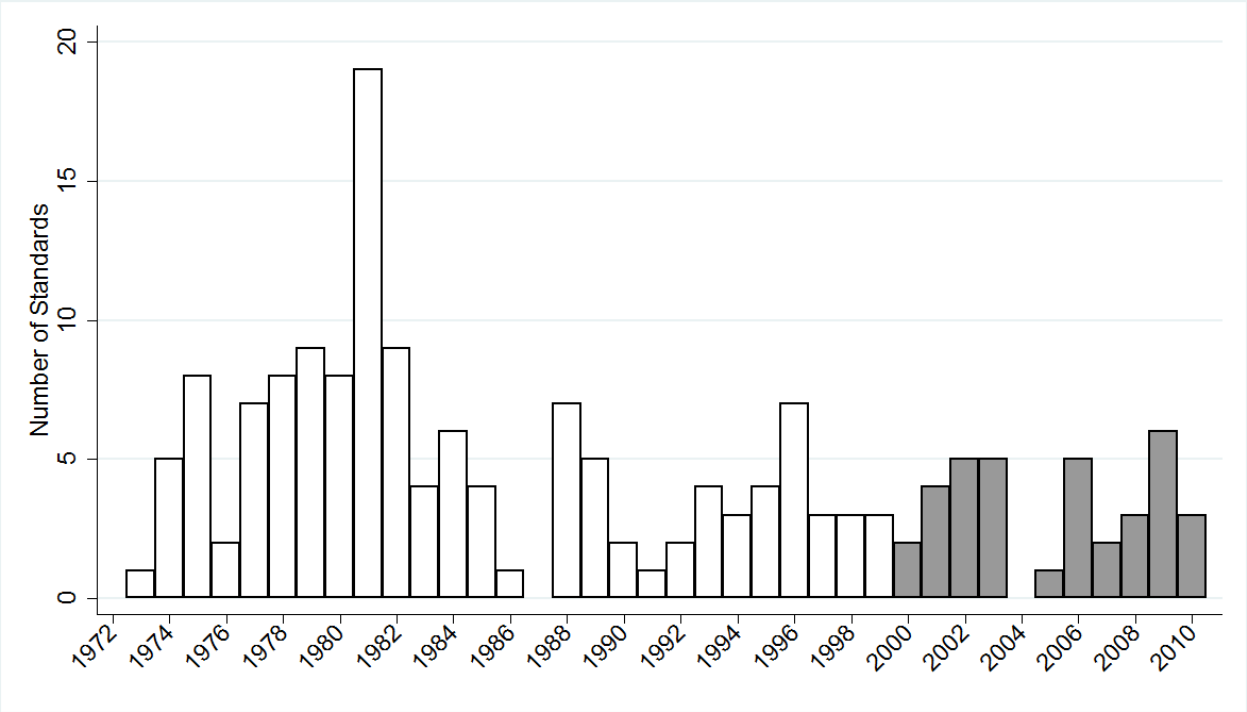
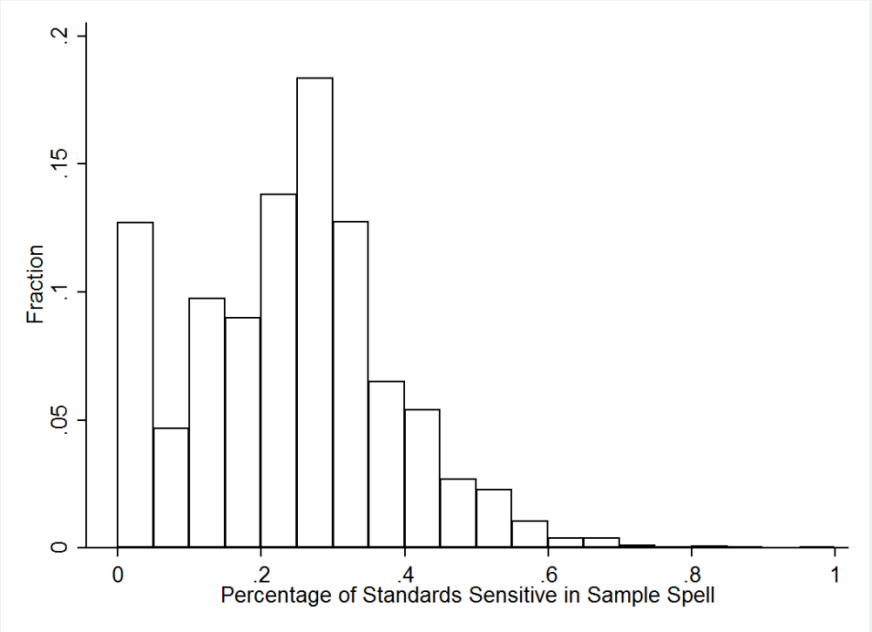
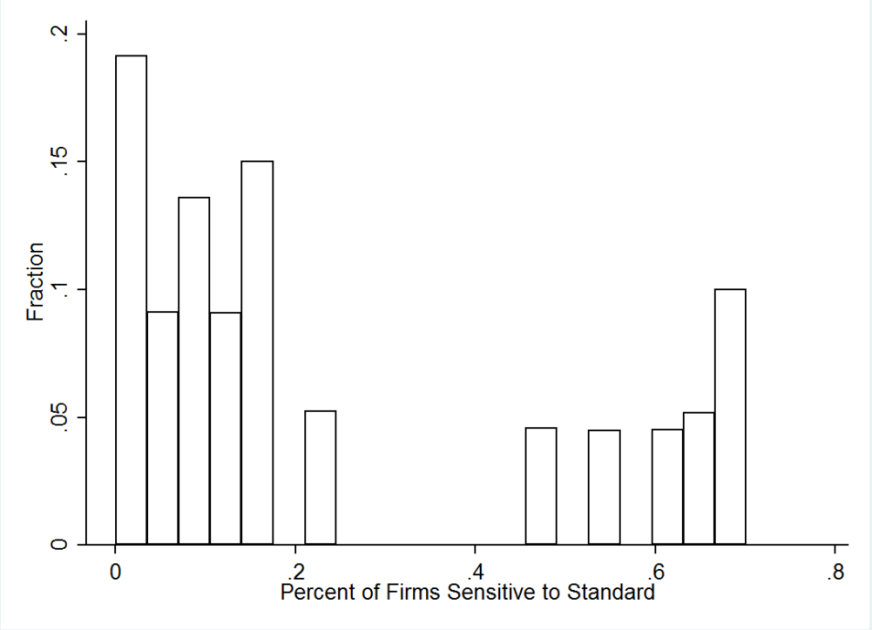


Figure 2. Variance Decomposition of Sensitivity to FASB Standards

Panel A of this figure presents a histogram of firm fixed effects estimates of Sensitive, which represent the percentage of FASB standards each firm is sensitive to in its sample spell. Panel B of this figure presents a histogram of standard fixed effects estimates of Sensitive, which represent the percentage of firms that are sensitive to each FASB standard. *Sensitive* is an indicator that equals one if any of the firm's 10-K filings from before the effective date mention the accounting standard and zero otherwise.



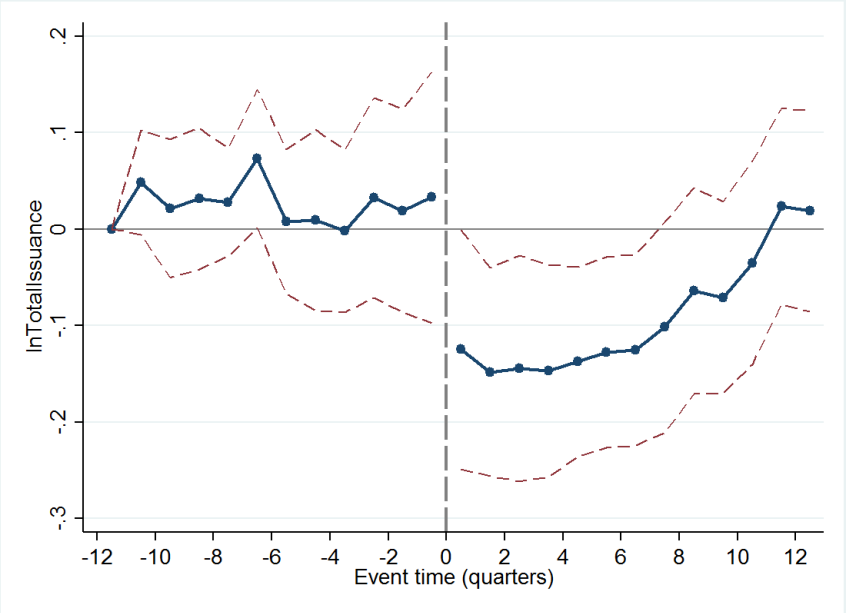
Panel A. Firm Fixed Effects



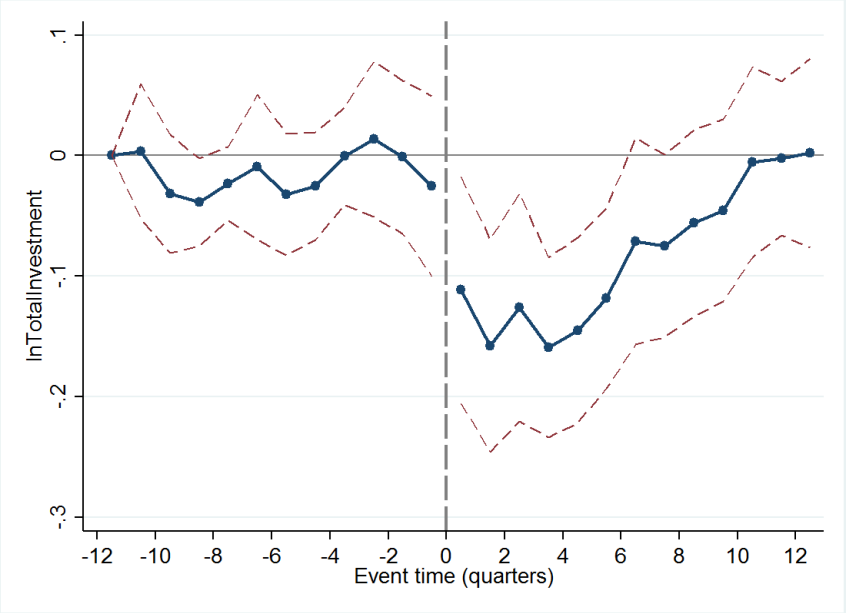
Panel B. Standard Fixed Effects

Figure 3. Dynamic Test of Parallel Trends

This figure presents a visual test of the parallel trends assumption for each dependent variable of interest over the 12 quarters before and after each FASB standard. Connected scatterplots (blue lines and dots) for sensitive and insensitive groups are plotted relative to their corresponding $t - 12$ values, preserving pre-effective date trends for visualization. The red dashed lines represent 95% confidence intervals derived from standard errors that are double clustered by firm and standard. Panel A presents the dynamics of total issuance (i.e., sum of equity and debt issuance) for sensitive firms relative to insensitive firms. Panel B presents the dynamics of total investment (i.e., sum of CAPEX, R&D, and acquisitions) for sensitive firms relative to insensitive firms. Outcome variables are seasonally adjusted. *Sensitive* is an indicator that equals one if any of the firm's 10-K filings from before the effective date mention the accounting standard and zero otherwise.



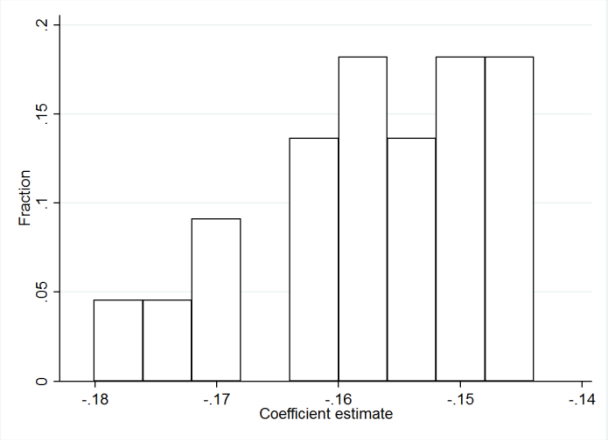
Panel A. Total Issuance



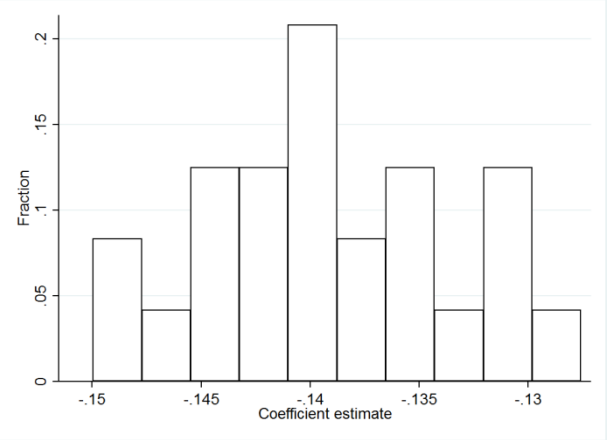
Panel B. Total Investment

Figure 4. Event Heterogeneity

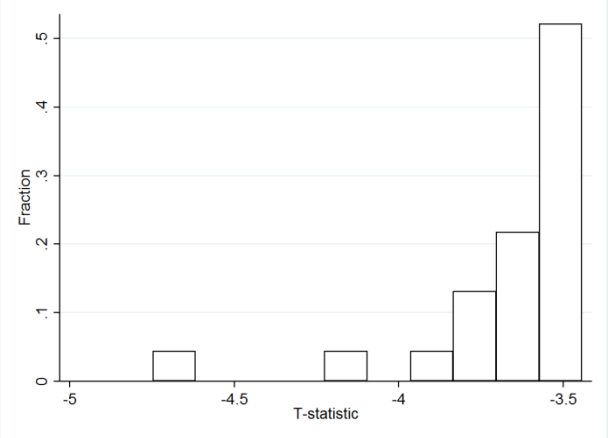
This figure presents analysis of the potential heterogeneous effects of FASB standards in our sample for the two main outcomes of interest: total securities issuance and total investment. In Panel A and B, we present histograms of coefficient estimates from specifications identical to those presented in column (1) of Tables 2 and 7, and we present the analogous histograms of the *t*-statistics corresponding to these estimates in Panels C and D. Panel A and C present estimates and *t*-statistics for total issuance and Panels B and D present estimates and *t*-statistics for total investment, respectively.



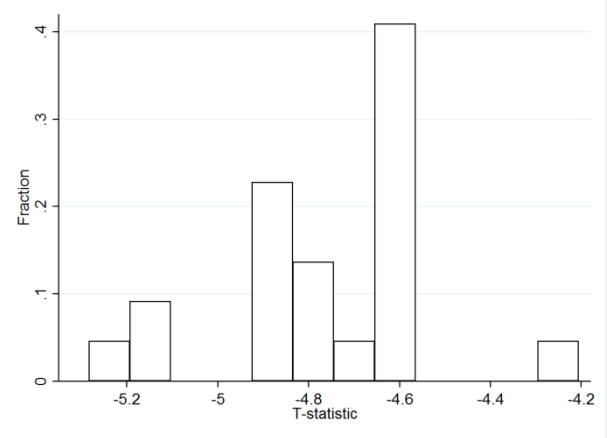
Panel A. Issuance Coefficient Estimate



Panel B. Investment Coefficient Estimate



Panel C. Issuance *t*-statistic



Panel D. Investment *t*-statistic

Table 1. Summary Statistics

Panel A. Summary Statistics

	Obs.	Mean	SD	P25	Median	P75
Total Assets ($M)	277,888	902.15	3,285.26	32.78	128.82	538.31
Age (yrs)	277,888	36.78	23.21	18	35	52
Total Issuance ($M)	277,888	46.87	1,130.95	0.07	0.33	1.97
Equity Issuance ($M)	277,888	1.33	6.89	0.05	0.20	0.76
Debt Issuance ($M)	277,888	45.54	1,130.74	0	0	0
1[*Asset sales*]	277,888	19.67%				
Asset sales ($M)	54,656	2.33	49.10	0.01	0.05	0.26
Payout ($M)	277,888	9.12	74.55	0	0	0
Dividends ($M)	277,888	2.97	27.89	0	0	0
Repurchases ($M)	277,888	6.14	67.98	0	0	0
Total Investment ($M)	277,888	24.01	145.47	0.70	3.70	12.95
CAPEX ($M)	277,888	7.80	39.42	0.05	0.50	3.21
R&D ($M)	277,888	9.72	54.25	0.03	1.43	6.10
Acquisitions ($M)	277,888	6.48	105.30	0	0	0
Sensitive	277,888	24.19%				
Sensitivity|*Sensitive*	67,212	13.35	16.18	4	8	16

Panel B. Pre-Standard Covariate Balance

	Mean		Difference	*p-value*
	Sensitive	Not Sensitive		
Total Assets ($M)	898.29	766.60	131.69	0.228
Age (yrs)	31.56	34.64	-3.08	0.308
Total Issuance ($M)	45.91	39.41	6.50	0.167
Equity Issuance ($M)	1.40	1.23	0.17	0.302
Debt Issuance ($M)	44.51	38.18	6.33	0.154
1[*Asset sales*]	18.94%	19.03%	-0.09	0.950
Asset sales ($M)	3.31	2.46	0.85	0.518
Payout ($M)	9.71	7.69	2.02	0.465
Dividends ($M)	3.22	2.27	0.95	0.145
Repurchases ($M)	6.48	5.42	1.06	0.645
Total Investment ($M)	26.39	22.69	3.70	0.368
CAPEX ($M)	9.20	6.73	2.47	0.029
R&D ($M)	9.14	8.88	0.26	0.795
Acquisitions ($M)	8.04	7.07	0.97	0.716

Table 2. Accounting Standards and Securities Issuance

This table presents generalized difference-in-differences estimates of the effect of accounting standards on total securities issuance for standards that became newly effective between 2000 and 2010. The event window is restricted to four quarters before the issuance date and four quarters after the effective date. $\ln TotalIssuance$ is the natural log of one plus the total amount of equity and debt securities issuance. *Sensitive* is an indicator that equals one if any of the firm's 10-K filings from before the effective date mention the accounting standard and zero otherwise. *Post* is an indicator that equals one after the accounting standard becomes effective and zero otherwise. Data definitions and sources are described in Appendix A. We include increasingly restrictive fixed effects to focus identifying variation on within standard and firm variation in loan amounts over time. Heteroskedasticity-robust standard errors are double clustered by firm and standard, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

Dependent variable: $\ln TotalIssuance$				
	(1)	(2)	(3)	(4)
Sensitive × *Post*	-0.1572***	-0.1179***	-0.1180***	-0.1143***
	(0.0423)	(0.0357)	(0.0357)	(0.0362)
Fixed Effects:				
Quarter	Yes	No	Yes	Yes
Firm × *Standard*	No	Yes	Yes	Yes
Industry × *Quarter*	No	No	No	Yes
Adj. R^2	0.002	0.669	0.669	0.671
Obs.	273,888	273,488	273,488	273,481

Table 3. Accounting Standards, Debt Issuance, and Equity Issuance

This table presents generalized difference-in-differences estimates of the effect of accounting standards on components of total securities issuance for standards that became newly effective between 2000 and 2010. The event window is restricted to four quarters before the issuance date and four quarters after the effective date. Panel A presents evidence on equity issuance, and Panel B presents evidence on debt issuance. ln*EquityIssuance* and ln*DebtIssuance* are the natural log of one plus the total amount of equity and debt securities issuance, respectively. *Sensitive* is an indicator that equals one if any of the firm's 10-K filings from before the effective date mention the accounting standard and zero otherwise. *Post* is an indicator that equals one after the accounting standard becomes effective and zero otherwise. Data definitions and sources are described in Appendix A. We include increasingly restrictive fixed effects to focus identifying variation on within standard and firm variation in loan amounts over time. Heteroskedasticity-robust standard errors are double clustered by firm and standard, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

Panel A. Equity Issuance

Dependent variable: ln*EquityIssuance*				
	(1)	(2)	(3)	(4)
Sensitive × *Post*	-0.1070***	-0.0787***	-0.0788***	-0.0786***
	(0.0212)	(0.0203)	(0.0203)	(0.0201)
Fixed Effects:				
Quarter	Yes	No	Yes	Yes
Firm × *Standard*	No	Yes	Yes	Yes
Industry × *Quarter*	No	No	No	Yes
Adj. R^2	0.002	0.932	0.932	0.933
Obs.	277,888	273,488	273,488	273,481

Panel B. Debt Issuance

Dependent variable: ln*DebtIssuance*				
	(1)	(2)	(3)	(4)
Sensitive × *Post*	-0.0844**	-0.0620**	-0.0621**	-0.0592**
	(0.0360)	(0.0308)	(0.0308)	(0.0301)
Fixed Effects:				
Quarter	Yes	No	Yes	Yes
Firm × *Standard*	No	Yes	Yes	Yes
Industry × *Quarter*	No	No	No	Yes
Adj. R^2	0.001	0.560	0.560	0.563
Obs.	277,888	273,488	273,488	273,481

Table 4. Accounting Standards and Asset Sales

This table presents generalized difference-in-differences estimates of the effect of accounting standards on the incidence of asset sales for standards that became newly effective between 2000 and 2010. The event window is restricted to four quarters before the issuance date and four quarters after the effective date. 1[*AssetSale*] is an indicator that equals one if the firm sells any assets and zero otherwise. *Sensitive* is an indicator that equals one if any of the firm's 10-K filings from before the effective date mention the accounting standard and zero otherwise. *Post* is an indicator that equals one after the accounting standard becomes effective and zero otherwise. Data definitions and sources are described in Appendix A. We include increasingly restrictive fixed effects to focus identifying variation on within standard and firm variation in loan amounts over time. Heteroskedasticity-robust standard errors are double clustered by firm and standard, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

Dependent variable: 1[*AssetSale*]				
	(1)	(2)	(3)	(4)
Sensitive × *Post*	0.0172**	0.0162**	0.0160**	0.0163**
	(0.0066)	(0.0078)	(0.0078)	(0.0077)
Fixed Effects:				
Quarter	Yes	No	Yes	Yes
Firm × *Standard*	No	Yes	Yes	Yes
Industry × *Quarter*	No	No	No	Yes
Adj. R^2	0.001	0.290	0.291	0.293
Obs.	277,888	273,488	273,488	273,481

Table 5. Accounting Standards and Payout Policy

This table presents generalized difference-in-differences estimates of the effect of accounting standards on total payout for standards that became newly effective between 2000 and 2010. The event window is restricted to four quarters before the issuance date and four quarters after the effective date. $\ln TotalPayout$ is the natural log of one plus the sum of dollars paid out as dividends and repurchases. *Sensitive* is an indicator that equals one if any of the firm's 10-K filings from before the effective date mention the accounting standard and zero otherwise. *Post* is an indicator that equals one after the accounting standard becomes effective and zero otherwise. Data definitions and sources are described in Appendix A. We include increasingly restrictive fixed effects to focus identifying variation on within standard and firm variation in loan amounts over time. Heteroskedasticity-robust standard errors are double clustered by firm and standard, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

Dependent variable: $\ln TotalPayout$				
	(1)	(2)	(3)	(4)
Sensitive × *Post*	-0.1399***	-0.0903**	-0.0911**	-0.0900**
	(0.0485)	(0.0406)	(0.0405)	(0.0398)
Fixed Effects:				
Quarter	Yes	No	Yes	Yes
Firm × *Standard*	No	Yes	Yes	Yes
Industry × *Quarter*	No	No	No	Yes
Adj. R^2	0.002	0.722	0.722	0.724
Obs.	277,888	273,488	273,488	273,481

Table 6. Accounting Standards and Payout Components

This table presents generalized difference-in-differences estimates of the effect of accounting standards on components of total payout for standards that became newly effective between 2000 and 2010. The event window is restricted to four quarters before the issuance date and four quarters after the effective date. Panel A presents evidence on dividends and Panel B presents evidence on repurchases. ln*Dividends* and ln*Repurchases* are the natural log of one plus the total amount of dividends and repurchases, respectively. *Sensitive* is an indicator that equals one if any of the firm's 10-K filings from before the effective date mention the accounting standard and zero otherwise. *Post* is an indicator that equals one after the accounting standard becomes effective and zero otherwise. Data definitions and sources are described in Appendix A. We include increasingly restrictive fixed effects to focus identifying variation on within standard and firm variation in loan amounts over time. Heteroskedasticity-robust standard errors are double clustered by firm and standard, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

Panel A. ln*Dividends*

	(1)	(2)	(3)	(4)
Sensitive × *Post*	-0.0530**	-0.0198	-0.0200	-0.0195
	(0.0216)	(0.0138)	(0.0138)	(0.0133)
Fixed Effects:				
Quarter	Yes	No	Yes	Yes
Firm × *Standard*	No	Yes	Yes	Yes
Industry × *Quarter*	No	No	No	Yes
Adj. R^2	0.001	0.866	0.866	0.867
Obs.	277,888	273,488	273,488	273,481

Panel B. ln*Repurchases*

	(1)	(2)	(3)	(4)
Sensitive × *Post*	-0.1100***	-0.0816**	-0.0823**	-0.0811**
	(0.0387)	(0.0371)	(0.0369)	(0.0366)
Fixed Effects:				
Quarter	Yes	No	Yes	Yes
Firm × *Standard*	No	Yes	Yes	Yes
Industry × *Quarter*	No	No	No	Yes
Adj. R^2	0.003	0.522	0.523	0.525
Obs.	277,888	273,488	273,488	273,481

Table 7. Accounting Standards and Total Investment

This table presents generalized difference-in-differences estimates of the effect of accounting standards on total investment for standards that became newly effective between 2000 and 2010. The event window is restricted to four quarters before the issuance date and four quarters after the effective date. $\ln TotalInvestment$ is the natural log of one plus the sum of dollars invested in capital expenditures, R&D, and acquisitions. *Sensitive* is an indicator that equals one if any of the firm's 10-K filings from before the effective date mention the accounting standard and zero otherwise. *Post* is an indicator that equals one after the accounting standard becomes effective and zero otherwise. Data definitions and sources are described in Appendix A. We include increasingly restrictive fixed effects to focus identifying variation on within standard and firm variation in loan amounts over time. Heteroskedasticity-robust standard errors are double clustered by firm and standard, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

Dependent variable: $\ln TotalInvestment$				
	(1)	(2)	(3)	(4)
Sensitive × *Post*	-0.1394***	-0.1097***	-0.1102***	-0.1078***
	(0.0286)	(0.0394)	(0.0348)	(0.0347)
Fixed Effects:				
Quarter	Yes	No	Yes	Yes
Firm × *Standard*	No	Yes	Yes	Yes
Industry × *Quarter*	No	No	No	Yes
Adj. R^2	0.001	0.884	0.884	0.885
Obs.	277,888	273,488	273,488	273,481

Table 8. Accounting Standards and Investment Components

This table presents generalized difference-in-differences estimates of the effect of accounting standards on components of total investment for standards that became newly effective between 2000 and 2010. The event window is restricted to four quarters before the issuance date and four quarters after the effective date. Panel A presents evidence on capital expenditures, Panel B presents evidence on R&D, and Panel C presents evidence on acquisitions. ln*CAPEX*, ln*R&D*, and ln*Acquisitions* are the natural log of one plus the amount of dollars invested in capital expenditures, R&D, and acquisitions, respectively. *Sensitive* is an indicator that equals one if any of the firm's 10-K filings from before the effective date mention the accounting standard and zero otherwise. *Post* is an indicator that equals one after the accounting standard becomes effective and zero otherwise. Data definitions and sources are described in Appendix A. We include increasingly restrictive fixed effects to focus identifying variation on within standard and firm variation in loan amounts over time. Heteroskedasticity-robust standard errors are double clustered by firm and standard, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

Panel A. ln*CAPEX*

	(1)	(2)	(3)	(4)
Sensitive × *Post*	-0.1520***	-0.1200***	-0.1203***	-0.1181***
	(0.0340)	(0.0387)	(0.0386)	(0.0381)
Fixed Effects:				
Quarter	Yes	No	Yes	Yes
Firm × *Standard*	No	Yes	Yes	Yes
Industry × *Quarter*	No	No	No	Yes
Adj. R^2	0.002	0.895	0.895	0.896
Obs.	277,888	273,488	273,488	273,481

Panel B. ln*R&D*

	(1)	(2)	(3)	(4)
Sensitive × *Post*	-0.0549**	-0.0473*	-0.0476*	-0.0472*
	(0.0218)	(0.0281)	(0.0281)	(0.0276)
Fixed Effects:				
Quarter	Yes	No	Yes	Yes
Firm × *Standard*	No	Yes	Yes	Yes
Industry × *Quarter*	No	No	No	Yes
Adj. R^2	0.001	0.957	0.957	0.957
Obs.	277,888	273,488	273,488	273,481

Panel C. $\ln Acquisitions$

	(1)	(2)	(3)	(4)
Sensitive × *Post*	-0.0605***	-0.0514***	-0.0517***	-0.0508***
	(0.0176)	(0.0160)	(0.0159)	(0.0163)
Fixed Effects:				
Quarter	Yes	No	Yes	Yes
Firm × *Standard*	No	Yes	Yes	Yes
Industry × *Quarter*	No	No	No	Yes
Adj. R^2	0.001	0.306	0.307	0.309
Obs.	277,888	273,488	273,488	273,481

Table 9. Alternative Event Windows

This table presents generalized difference-in-differences estimates of the effect of accounting standards on total securities issuance and total investment for standards that became newly effective between 2000 and 2010. Panel A presents evidence on total issuance, and Panel B presents evidence on total investment. The specification matches those presented in column (4) of Tables 2 and 7, and the event window varies between two quarters before and after the effective date of the standard in column (1) to twelve quarters before and after the effective date of the standard in column (5). ln *TotalIssuance* is the natural log of one plus the total amount of equity and debt securities issuance. ln *TotalInvestment* is the natural log of one plus the sum of dollars invested in capital expenditures, R&D, and acquisitions. *Sensitive* is an indicator that equals one if any of the firm's 10-K filings from before the effective date mention the accounting standard and zero otherwise. *Post* is an indicator that equals one after the accounting standard becomes effective and zero otherwise. Data definitions and sources are described in Appendix A. We include increasingly restrictive fixed effects to focus identifying variation on within standard and firm variation in loan amounts over time. Heteroskedasticity-robust standard errors are double clustered by firm and standard, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

Panel A. ln *TotalIssuance*

	(1)	(2)	(3)	(4)	(5)
Sensitive × *Post*	-0.1266***	-0.1143***	-0.1079***	-0.1019***	-0.0737***
	(0.0418)	(0.0362)	(0.0315)	(0.0300)	(0.0252)
Window (quarters)	2	4	6	8	12
Fixed Effects:					
Quarter	Yes	Yes	Yes	Yes	Yes
Firm × *Standard*	Yes	Yes	Yes	Yes	Yes
Industry × *Quarter*	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.673	0.671	0.665	0.661	0.655
Obs.	150,198	273,481	395,749	512,715	738,281

Panel B. ln *TotalInvestment*

	(1)	(2)	(3)	(4)	(5)
Sensitive × *Post*	-0.0941***	-0.1078***	-0.0916***	-0.0724**	-0.0338
	(0.0350)	(0.0347)	(0.0332)	(0.0299)	(0.0234)
Window (quarters)	2	4	6	8	12
Fixed Effects:					
Quarter	Yes	Yes	Yes	Yes	Yes
Firm × *Standard*	Yes	Yes	Yes	Yes	Yes
Industry × *Quarter*	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.888	0.885	0.880	0.877	0.869
Obs.	150,198	273,481	395,749	512,715	738,281

Table 10. Sample Weights

This table presents generalized difference-in-differences estimates of the effect of accounting standards on total securities issuance (Panel A) and total investment (Panel B). The tables match Tables 2 and 7, except that they apply regression weights that equal the inverse of the number of standards effective in the same quarter. These weights deemphasize firm-standard observations that are duplicated in the estimation sample due to the stacked event study design and existence of quarters with multiple standards becoming effective. Standard error adjustments in Tables 2 and 7 account for these implicit weights, and this table investigates the impact of these implicit weights on coefficient estimates. ln *TotalIssuance* is the natural log of one plus the total amount of equity and debt securities issuance. ln *TotalInvestment* is the natural log of one plus the sum of dollars invested in capital expenditures, R&D, and acquisitions. *Sensitive* is an indicator that equals one if any of the firm's 10-K filings from before the effective date mention the accounting standard and zero otherwise. *Post* is an indicator that equals one after the accounting standard becomes effective and zero otherwise. Data definitions and sources are described in Appendix A. We include increasingly restrictive fixed effects to focus identifying variation on within standard and firm variation in loan amounts over time. Heteroskedasticity-robust standard errors are double clustered by firm and standard, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

Panel A. ln *TotalIssuance*

	(1)	(2)	(3)	(4)
Sensitive × *Post*	-0.1470***	-0.1134***	-0.1135***	-0.1112***
	(0.0451)	(0.0367)	(0.0367)	(0.0377)
Sample weight	Yes	Yes	Yes	Yes
Fixed Effects:				
Quarter	Yes	No	Yes	Yes
Firm × *Standard*	No	Yes	Yes	Yes
Industry × *Quarter*	No	No	No	Yes
Adj. R^2	0.003	0.912	0.913	0.915
Obs.	275,322	271,406	271,406	271,399

Panel B. ln *TotalInvestment*

	(1)	(2)	(3)	(4)
Sensitive × *Post*	-0.1477***	-0.1212***	-0.1217***	-0.1189***
	(0.0320)	(0.0367)	(0.0366)	(0.0363)
Sample weight	Yes	Yes	Yes	Yes
Fixed Effects:				
Quarter	Yes	No	Yes	Yes
Firm × *Standard*	No	Yes	Yes	Yes
Industry × *Quarter*	No	No	No	Yes
Adj. R^2	0.002	0.884	0.884	0.885
Obs.	275,322	271,406	271,406	271,399

Table 11. Treatment Intensity

This table presents generalized difference-in-differences estimates of the effect of accounting standards on total securities issuance (Panel A) and total investment (Panel B) for standards that became newly effective between 2000 and 2010. The tables match Tables 2 and 7, except that they use a continuous measure of treatment intensity, ln*Sensitivity*, instead of an indicator that identifies treated firms, *Sensitive*. ln*TotalIssuance* is the natural log of one plus the total amount of equity and debt securities issuance. ln*TotalInvestment* is the natural log of one plus the sum of dollars invested in capital expenditures, R&D, and acquisitions. ln*Sensitivity* is the natural log of one plus the number of times that firm i mentions standard s in firm i's 10-K filings from before the effective date. *Post* is an indicator that equals one after the accounting standard becomes effective and zero otherwise. Data definitions and sources are described in Appendix A. We include increasingly restrictive fixed effects to focus identifying variation on within standard and firm variation in loan amounts over time. Heteroskedasticity-robust standard errors are double clustered by firm and standard, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

Panel A. ln*TotalIssuance*

	(1)	(2)	(3)	(4)
ln*Sensitivity* × *Post*	-0.0554***	-0.0507***	-0.0507***	-0.0497***
	(0.0201)	(0.0137)	(0.0137)	(0.0137)
Fixed Effects:				
Quarter	Yes	No	Yes	Yes
Firm × *Standard*	No	Yes	Yes	Yes
Industry × *Quarter*	No	No	No	Yes
Adj. R^2	0.002	0.669	0.669	0.671
Obs.	277,888	273,488	273,488	273,481

Panel B. ln*TotalInvestment*

	(1)	(2)	(3)	(4)
ln*Sensitivity* × *Post*	-0.0579***	-0.0499***	-0.0501***	-0.0494***
	(0.0125)	(0.0134)	(0.0134)	(0.0133)
Fixed Effects:				
Quarter	Yes	No	Yes	Yes
Firm × *Standard*	No	Yes	Yes	Yes
Industry × *Quarter*	No	No	No	Yes
Adj. R^2	0.003	0.884	0.884	0.885
Obs.	277,888	273,488	273,488	273,481

Table 12. Intensive Margin of Treatment

This table presents generalized difference-in-differences estimates of the effect of accounting standards on total securities issuance (Panel A) and total investment (Panel B) for standards that became newly effective between 2000 and 2010. The tables match Tables 2 and 7, except that they add an interaction between *HighSensitivity*, an indicator that equals one if the firm-standard pair is in the highest quartile of mentions among those with any mentions of a standard and zero otherwise, and *Post*. All other data definitions and sources are described in Table 2, Table 7, and Appendix A. We include increasingly restrictive fixed effects to focus identifying variation on within standard and firm variation in loan amounts over time. Heteroskedasticity-robust standard errors are double clustered by firm and standard, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

Panel A. ln *TotalIssuance*

	(1)	(2)	(3)	(4)
Sensitive × *Post*	-0.1552***	-0.0987***	-0.0988***	-0.0948**
	(0.0512)	(0.0381)	(0.0382)	(0.0380)
HighSensitivity × *Post*	-0.0080	-0.0723*	-0.0724*	-0.0739**
	(0.1014)	(0.0367)	(0.0366)	(0.0348)
Fixed Effects:				
Quarter	Yes	No	Yes	Yes
Firm × *Standard*	No	Yes	Yes	Yes
Industry × *Quarter*	No	No	No	Yes
Adj. R^2	0.002	0.669	0.669	0.671
Obs.	277,888	273,488	273,488	273,481

Panel B. ln *TotalInvestment*

	(1)	(2)	(3)	(4)
Sensitive × *Post*	-0.1113***	-0.0808***	-0.0812***	-0.0789**
	(0.0282)	(0.0277)	(0.0277)	(0.0282)
HighSensitivity × *Post*	-0.1091*	-0.1092***	-0.1094***	-0.1097***
	(0.0564)	(0.0403)	(0.0404)	(0.0397)
Fixed Effects:				
Quarter	Yes	No	Yes	Yes
Firm × *Standard*	No	Yes	Yes	Yes
Industry × *Quarter*	No	No	No	Yes
Adj. R^2	0.004	0.884	0.884	0.885
Obs.	277,888	273,488	273,488	273,481