Journal of Accounting and Economics xxx (xxxx) xxx

Contents lists available at ScienceDirect

Journal of Accounting and Economics

journal homepage: www.journals.elsevier.com/journal-of-accounting-and-economics

Do industry-specific accounting standards matter for capital allocation decisions?

Peter Fiechter [a,*], Wayne R. Landsman [b], Kenneth Peasnell [c], Annelies Renders [d]

[a] Faculty of Economics and Business, University of Neuchatel, Neuchatel, Switzerland
[b] Kenan-Flagler Business School, University of North Carolina at Chapel Hill, Chapel Hill, USA
[c] The Management School, Lancaster University, Lancaster, UK
[d] BI Norwegian Business School, Oslo, Norway

ARTICLE INFO

JEL classification:
E22
G21
G28
G32
K23
M41
M48

Keywords:
Capital flows
Industry-specific standards
Comparability
Transparency
Accounting uniformity
Stacked regression

ABSTRACT

This study examines whether the implementation of industry-specific accounting standards helps capital market participants in making decisions about providing capital to firms. We predict and find an, on average, increase in firms' capital growth in years following implementation of the relevant industry standard. The increase in capital growth arises primarily from equity issuances and is attributable to the implementation of the standards rather than industry-specific trends or economic shocks. We explore heterogeneity in industry standards and find more pronounced effects for (i) industry standards that reveal new information, provide explicit guidance, or increase accounting uniformity, and (ii) small firms, firms with greater information asymmetry, and firms with greater capital constraints before implementation of the standards. We also find evidence consistent with two channels explaining the documented increase in capital flows: reduction of information asymmetry and increase in financial statement comparability.

1. Introduction

The primary objective of financial statements is to improve decision-making by existing and potential investors, lenders, and other creditors about providing resources to the entity (FASB, 2018). Although nearly a quarter of all U.S. accounting standards introduced between 1973 and 2014 pertain to industry-specific topics (Jiang et al., 2018), industry-specific standards have been gradually replaced by "one-size-fits-all" standards. For example, the new revenue recognition standard (ASC 606) eliminates most industry-specific applications and exceptions but is not without controversy (Cohn, 2018). This raises the question as to whether industry-specific standards are helpful for investors in making decisions about providing capital to firms.

In this study, we address this question by examining whether implementation of industry-specific standards on average leads to increases in capital flows to firms in affected industries. However, not all industry-specific standards are created equally, and thus, an average effect across different standards could mask substantial heterogeneity. Therefore, by exploring heterogeneity in industry standards—e.g., the extent to which they provide new information to investors or impose uniformity in disclosures and

* Corresponding author.
E-mail address: peter.fiechter@unine.ch (P. Fiechter).

https://doi.org/10.1016/j.jacceco.2023.101670
Received 19 September 2022; Received in revised form 25 October 2023; Accepted 18 December 2023
Available online 28 December 2023

measurement—we shed light on which type of standard is associated with increased capital flows. Finally, our study aims to provide evidence regarding which firms benefit from implementation of industry-specific standards, e.g., whether such standards help capital-constrained firms to obtain more capital.

Before an industry standard is implemented, there is presumably an equilibrium amount of capital supplied that depends on the quality of information available to capital providers (Myers and Majluf, 1984; Diamond and Verrecchia, 1991). By requiring more disclosures and prescribing consistent accounting methods through the implementation of new industry standards, the FASB aims to increase transparency and comparability (FASB, 2018). In the absence of accounting regulation, firms may not always consider comparability effects (e.g., Corona et al., 2023; Dye and Sridhar, 2008; Roychowdhury et al., 2019). An industry standard that increases transparency or comparability (or both) can reduce disclosure processing costs (e.g., Blankespoor et al., 2020) and may lower the cost of capital (Lambert et al., 2007), thus increasing the equilibrium amount of capital provided to the average firm in the affected industry.[1]

Whether industry-standards achieve the intended objective of increasing transparency and comparability is *a priori* unclear. For example, industry standards may not require sufficient new information to alter transparency levels and investors' processing costs. In addition, imposing more uniform accounting practices does not necessarily result in increased comparability (Corona et al., 2023; Dye and Sridhar, 2008; Zeff, 2007). In our setting, however, we posit that industry-specific standards potentially increase comparability because they are applied to a more homogenous set of firms in the same industry relative to firms across different industries. Specifically, Corona et al. (2023) shows that accounting uniformity improves comparability and capital allocation decisions in settings with firms that have less idiosyncratic production functions. In sum, it is an empirical question whether implementation of industry-specific standards leads to an on average increase in capital provided to firms in the affected industries.

Because industry standards are not created equally, their effects on capital flows likely vary with the characteristics of industry standards. On the one hand, if an industry-specific standard is simply a codification of existing AICPA industry guidance, the incremental information provided by firms likely is limited. In addition, industry standards that increase managerial judgment and accounting choices (e.g., Healy and Palepu, 2001; Watts and Zimmermann, 1990) might not improve comparability. On the other hand, industry standards that provide explicit guidance (Schipper, 2003) and make accounting methods more uniform likely increase comparability of firms in the affected industry and facilitate capital allocation decisions (Corona et al., 2023; Dye and Sridhar, 2008). Taken together, we predict stronger effects for those industry standards that (i) reveal incremental information—i.e., no AICPA codifications, (ii) do not increase managerial judgment and accounting choice, (iii) provide explicit guidance, and (iv) increase accounting uniformity.

Even if implementation of industry standards leads to average increases in capital provided to firms in affected industries, it is unclear which firms benefit most from the new standards. Diamond and Verrecchia (1991) show that small firms provide fewer voluntary disclosures than large firms, resulting in higher information asymmetry and limiting the capital provided to smaller firms (Myers and Majluf, 1984). In addition, the investment efficiency literature shows that financially constrained firms benefit more from higher financial reporting and audit quality (Biddle et al., 2009; Shroff, 2020). Hence, we predict more pronounced increases in capital flows after the implementation of industry-specific standards for ex ante small firms, firms with higher information asymmetry, and firms with greater capital constraints.

We conduct our empirical tests by using a "stacked cohort" regression model (Armstrong et al., 2022; Baker et al., 2022; Cengiz et al., 2019; Gormley and Matsa, 2011), in which we create separate datasets for each of the industries we identify as being affected by specific industry standards introduced by the FASB between 1975 and 2011. The key explanatory variables are a series of indicator variables that denote fiscal years in the five years before and five years after the year in which an industry-specific standard is effective, omitting the year before implementation of the standard. In the baseline specification, we measure capital growth as the year-to-year change in firms' long-term debt and equity capital, and test whether firms in affected industries experience greater capital growth following implementation of an industry-specific standard.

Based on a sample of 221,477 listed U.S. non-financial firm-year observations, we find an average increase of approximately 4.5% in capital growth over the post-period relative to the pre-period for firms in an industry after implementation of a standard that affects their industry. As described in more detail in Section 3.2 below, our inclusion of fixed effects and control variables allows us to interpret this result as an incremental effect of industry-specific accounting standards on capital growth beyond other factors such as firms' investment opportunities or their self-financing ability. In addition, we find no clear trend in capital growth in the years preceding implementation of the standards, which increases our confidence that the documented increase in capital in the post-period is attributable to the standards' implementation.

Capital growth can arise from increases in either equity or debt capital. Because debt capital providers may have access to information about the firm that is not necessarily available to equity investors, the information provided by financial statements following implementation of a new industry standard may have less of an impact on debt capital providers (Beatty et al., 2009; Bharath et al., 2008; Plumlee et al., 2015). Consistent with this line of reasoning, our findings show that the post-standard increase in capital growth is mainly attributable to equity issuances. Therefore, we use both capital growth and equity issuances as outcome variables in the remaining analyses.

We next conduct four additional tests to address the possibility that our findings are attributable to industry-specific trends or other economic events unrelated to industry-specific standards. First, we include an additional control variable that reflects industry trends

[1] Industry-specific standards may also affect within-industry capital flows. However, empirically distinguishing between within-industry and across-industry capital flows is challenging because doing so requires holding the flow of capital into the industry constant.

in capital flows. Second, we include Canadian firms in our sample, which permits us to include industry-by-year-by-cohort fixed effects that absorb industry-specific changes in capital flows and that otherwise could not be included when our sample is limited to U.S. firms. Third, we use current liabilities in place of capital flows, changes of which are likely to be the outcome of firm-level operating decisions rather than decisions by investors to provide capital. Fourth, we exploit the fact that some standards have a relatively long standard setting process (i.e., three or more years) by conducting a placebo test with the initiation year as the placebo treatment year. We then contrast these findings to a test around the true effective year. Taken together, the findings from these four additional tests are consistent with the documented effects being attributable to the implementation of industry-standards.

As discussed above, the average effect of industry standards on capital flows likely masks heterogeneity across different standards. Therefore, we test whether variation in the type of standards affect capital flows by identifying similarities between individual standards and grouping them according to certain characteristics discussed in prior literature (Corona et al., 2023; Healy and Palepu, 2001; Schipper, 2003; Watts and Zimmermann, 1990). Consistent with our predictions, relative to other industry standards, we find insignificant and smaller effects for industry standards that simply codify existing industry guidance and that increase the level of managerial judgment or the number of accounting choices. In contrast, we find significant and more pronounced increases in capital flows for those industry standards that provide explicit guidance and that increase accounting uniformity, as compared to other industry standards.

For the firm-level cross-sectional tests, we run regressions for separate subsamples of (i) small firms defined as those with log of total assets below the sample median before implementation of the standard, (ii) firms with high information asymmetry defined as those with above median Amihud (2002) illiquidity score before implementation of the standard, and (iii) capital constrained firms defined according to the two-stage methodology of Biddle et al. (2009) before implementation of the standard. Consistent with our predictions, we find that the increases in capital flows are more pronounced for ex ante small firms, firms with relatively high illiquidity, and firms with greater capital constraints. These effects are concentrated in those standards that provide new information, include explicit guidance, and impose accounting uniformity. We interpret these findings as indicating that investors of such firms benefit particularly from increased comparability through the implementation of more uniform standards in these industries.

In additional analyses, we shed light on two non-mutually exclusive mechanisms explaining why implementation of industry-specific standards leads to greater capital flows: increases in transparency or comparability or both. We test for increases in transparency by investigating whether implementation of industry-specific accounting standards is associated with an increase in stock price liquidity based on the Amihud (2002) illiquidity measure. We test for increases in comparability by using the De Franco et al. (2011) comparability measure, ensuring that there is no overlap around the implementation years. Findings indicate that both stock price liquidity and financial statement comparability increase after implementation of industry-specific standards. We also find weak evidence of more pronounced liquidity and comparability effects for those industry standards that provide new information, include explicit guidance, and introduce more uniform accounting practices.

Our study contributes to the literature by showing that industry-specific standards appear to help investors in making decisions about providing capital to firms. In addition, our finding that different types of industry standards have different effects on capital flows—depending on the extent to which these standards reveal new information, provide guidance, or increase accounting uniformity—are not only informative to regulators and standard setters, but also inform the longstanding debate about the extent to which accounting standardization and uniformity are desirable for capital allocation decisions (e.g., Corona et al., 2023; Dye and Sridhar, 2008; Healy and Palepu, 2001, p. 404).

Our study also complements prior literature that investigates stock returns to the announcement of industry standards. Early literature on the passage of certain industry standards such as SFAS 19, SFAS 44, or SFAS 90 provides mixed evidence on whether investors perceive industry standards to be cost-beneficial (e.g., Collins and Dent, 1979; Dyckman and Smith, 1979; Martin et al., 2000; Schipper et al., 1987). More recently, Khan et al. (2018) investigates short-term market reactions around events that relate to the passage of virtually all FASB standards (including industry-specific standards). Khan et al. (2018) finds that although most events are value-neutral for shareholders, two of the industry-specific standards were perceived as value-increasing. However, these prior announcement-return studies do not provide evidence on whether capital actually flows to firms after the implementation of industry-specific standards.

Finally, our findings complement the literature on financial reporting and investment efficiency (e.g., Badertscher et al., 2013; Biddle et al., 2009; Biddle and Hilary, 2006; Chen et al., 2011; Shroff, 2017). By showing that changes in financial reporting lead to greater capital flows from investors to the firm, we shed light on one proposed underlying channel, namely whether financial reporting improves firms' access to external capital, through which financial reporting affects investment efficiency (Roychowdhury et al., 2019, pp. 4–5).

2. Data and sample

As a starting point, we use Table 2 of Khan et al. (2018) to identify industry-specific accounting standards and the affected industries. We eliminate industry-specific standards that only affect the financial industry because the financial industry has a substantially different business model from firms in other industries and is highly regulated. As a result, it is difficult to predict how changes in accounting standards would affect capital flows.[2] We then retain only those industry standards that affect specific SIC codes

[2] Excluding the financial industry and the associated industry standards eliminates 27 industry-specific standards considered by Khan et al. (2018).

or Fama-French 49 industries. This approach allows us to define treated industries (identified by SIC codes or Fama-French 49) and control industries (all other unaffected SIC codes or Fama-French 49), as required by our research design (see Equation (1) in section 3.2 below).

The selected industry-specific accounting standards were introduced between 1975 and 2011. We identify the effective date for each industry-specific standard, allowing us to determine the fiscal year in which the industry-specific standard is implemented. Appendix I presents an overview of the identified industry-specific accounting standards, the affected industries and associated SIC codes, the initiation year of the standard (i.e., when the FASB started the standard setting process), the effective year (i.e., the first fiscal year the standard is implemented), and different characteristics of industry-specific standards (see section 4 for more details).[3]

We conduct our tests using Compustat data for all U.S. publicly listed non-financial firms between 1970 and 2017. We require that firms have sufficient data available to calculate the variables included in the study. In addition, we require firms to have a December fiscal year-end to eliminate ambiguity regarding the year of standard implementation.

Table 1 presents the descriptive statistics of the variables for the full sample as well as for two subsamples: the sample comprising firms in industries that eventually are subject to an industry-specific standard during the sample period, i.e., the "treatment" sample, and firms in industries that are never subject to an industry-specific standard during the sample period, i.e., the "control" sample. The procedure for identifying the treatment and control samples is described in Section 3.2 below. 7.5 percent of the sample firm-years relate to firms in industries subject to an industry-specific standard (mean *Standard* = 0.075), and of these, 56 percent are firm-years that follow implementation of an industry-specific standard (mean *PostStandard*/mean *Standard* = 0.042/0.075). Descriptive statistics for the treatment and control samples reveal that the two groups have essentially the same levels of capital growth (*CapitalGrowth*) and do not have substantial differences in the other firm characteristics such as sales growth (*SalesGrowth*), Tobin's Q ratio (*Q*), cash as a proportion of total assets (*Cash*), firm size (*Size*), financial leverage (*Leverage*), and return on assets (*ROA*).

3. The effect of industry-specific standards on capital flows

3.1. Conceptual underpinning

To identify good investment opportunities, capital providers have to rely on information that firms provide, e.g., in their prospectuses or their financial statements. Biddle and Hilary (2006) and Biddle et al. (2009) provide evidence that firms with higher levels of financial reporting quality make higher levels of real investment than those with lower quality. The studies attribute these findings to financial reporting quality mitigating capital constraints arising from information asymmetry between the firm and external providers of capital. However, these studies do not explicitly examine the extent to which higher levels of financial reporting quality lead to greater capital flows from investors to the firm (Roychowdhury et al., 2019).

Before an industry standard is implemented, presumably there is an equilibrium amount of capital supplied that meets industry demand for capital. Such an equilibrium depends on the quality of information available to capital providers (Diamond and Verrecchia, 1991). Other things equal, the lower is the quality of information, the greater is the information asymmetry between firms and capital providers, and the lower is the amount of capital that will be provided at a given price (Myers and Majluf, 1984). In addition, information is more useful to capital providers if the information can be used to identify which firms are better prospects, which necessitates comparing and contrasting firms' prospects. Heterogeneity in firms' accounting and disclosure choices can make such comparisons difficult for capital providers because firms might not always take comparability considerations into account in the absence of accounting and disclosure regulation (e.g., Corona et al., 2023; Dye and Sridhar, 2008; Roychowdhury et al., 2019).

By increasing disclosures and/or reducing heterogeneity in the application of accounting methods and disclosure practices through the implementation of new accounting standards, the FASB aims to reduce information asymmetries between capital providers and management and to improve comparability. Hence, implementation of a new accounting standard that reduces information asymmetries and enhances comparability should be reflected in the reduction of information processing costs for investors (Blankespoor et al., 2020) and may lower the cost of capital (Lambert et al., 2007). Other things equal, a reduction in information processing costs and/or cost of capital likely shifts the industry supply curve of capital to the right, thereby increasing the equilibrium amount of capital supplied. An increase in transparency or comparability can also affect within-industry capital flows.

Although it is unclear whether industry standards provide sufficient incremental information that substantially enhances transparency and potentially alters investors' processing costs, we posit that comparability effects are more likely to obtain for industry-specific standards because they are applied to a more homogenous set of firms in the same industry as compared to firms from different industries. Corona et al. (2023) shows that accounting uniformity improves comparability and capital allocation decisions in settings with firms that are less idiosyncratic in terms of their production functions. Relatedly, Dye and Sridhar (2008) provides

[3] In some cases, industries are affected by multiple standards separated in time. For example, the oil & gas industry was affected by SFAS 009, *Accounting for Income Taxes: Oil and Gas Producing Companies*, in 1976, and again by SFAS 019, *Financial Accounting and Reporting by Oil and Gas Producing Companies*, in 1979. In such cases, we put these standards into different cohorts (see section 3.2).

Table 1
Descriptive statistics.

Variables	N	Mean	p10	p25	Median	p75	p90	Std. dev.
Full sample:								
Standard	221,477	0.075	0.000	0.000	0.000	0.000	0.000	0.263
PostStandard	221,477	0.042	0.000	0.000	0.000	0.000	0.000	0.202
CapitalGrowth	221,477	0.099	−0.230	−0.034	0.067	0.193	0.474	0.438
SalesGrowth	221,477	0.111	−0.186	−0.019	0.090	0.211	0.426	0.380
Q	221,477	1.735	0.773	0.931	1.183	1.749	2.940	1.933
Cash	221,477	0.138	0.008	0.023	0.069	0.182	0.379	0.173
Size	221,477	4.889	2.020	3.281	4.773	6.452	7.952	2.276
Leverage	221,477	0.533	0.204	0.347	0.512	0.656	0.808	0.326
ROA	221,477	−0.012	−0.179	−0.014	0.045	0.091	0.150	0.297
Treatment sample: firms from industries subject to an industry-specific standard								
CapitalGrowth	16,553	0.102	−0.134	−0.008	0.068	0.174	0.390	0.358
SalesGrowth	16,553	0.115	−0.158	−0.003	0.088	0.207	0.424	0.370
Q	16,553	1.587	0.819	0.938	1.113	1.527	2.457	1.791
Cash	16,553	0.099	0.004	0.013	0.041	0.118	0.263	0.148
Size	16,553	5.399	2.107	3.689	5.495	7.325	8.423	2.382
Leverage	16,553	0.557	0.232	0.419	0.581	0.665	0.774	0.283
ROA	16,553	0.016	−0.086	0.015	0.048	0.078	0.137	0.238
Control sample: firms from industries that are not subject to an industry-specific standard								
CapitalGrowth	204,924	0.099	−0.239	−0.037	0.067	0.194	0.481	0.444
SalesGrowth	204,924	0.111	−0.189	−0.020	0.090	0.211	0.427	0.381
Q	204,924	1.747	0.769	0.931	1.192	1.766	2.976	1.944
Cash	204,924	0.141	0.009	0.024	0.071	0.187	0.386	0.174
Size	204,924	4.847	2.015	3.253	4.720	6.371	7.886	2.262
Leverage	204,924	0.531	0.202	0.343	0.506	0.655	0.812	0.329
ROA	204,924	−0.014	−0.186	−0.018	0.044	0.092	0.151	0.301

This table reports summary statistics for 221,477 U.S. listed, nonfinancial firm-year observations from 1970 to 2017. The sample comprises 14 cohorts (i.e., affected and unaffected firms around introduction of industry-specific standards) based on an event window that begins in year $t-5$ and ends in year $t+5$ around the effective year of the standard, $t=0$. The dependent variable, *CapitalGrowth*, is the log of invested capital divided by lagged invested capital (equity and long-term debt). *Standard* is an indicator variable equal to one for firms from industries that receive an industry-specific standard during the sample period, and zero otherwise. *PostStandard* is an indicator variable equal to one for firm-years after the implementation of the industry-specific standard. *SalesGrowth* is the log of sales divided by lagged sales. *Q* is the market value of equity plus total liabilities, divided by total assets. *Cash* is lagged cash divided by lagged total assets. *Size* is the log of lagged total assets. *Leverage* is lagged total liabilities divided by lagged total assets. *ROA* is net income divided by lagged total assets. All continuous variables are winsorized at the 1st and 99th percentiles. See Appendix II for a more detailed variable description.

theoretical support for the notion that more rigid (i.e., uniform) standards perform better in terms of investors' ability to make better investment choices when the underlying transactions are more homogenous, as is the case within industries rather than across industries. In sum, it is an empirical question whether implementation of industry-specific accounting standards on average results in an increase in capital provided to firms in the affected industries.[4]

Industry standards are not created equally, and thus their effects on investors' decisions about providing capital to firms likely varies with the characteristics of industry standards. For example, if an industry-specific standard is simply a codification of existing industry guidance, firms are unlikely to provide much new information under the new standard, resulting in no effect on capital allocation. In addition, industry standards that increase the level of managerial judgment and the number of accounting choices might hamper capital providers' ability to compare firms within the industry. In contrast, if an industry-specific standard provides explicit implementation and disclosure guidance or makes application of accounting methods more uniform, this should increase comparability of firms in the affected industry and facilitate capital allocation decisions (Dye and Sridhar, 2008; Corona et al., 2023). Hence, if industry standards on average increase capital flows, we predict more pronounced effects for those industry standards that (i) are not AICPA codifications and thus plausibly provide incremental information, (ii) do not increase managerial judgment and accounting choices, (iii) provide explicit implementation and disclosure guidance, and (iv) increase accounting uniformity, e.g., by prescribing

[4] A related study, Bird et al. (2020), posits that accounting standards force firms to reveal only bad news and that accounting standards generally do not exert positive externalities (i.e., comparability effects). The study finds that firms that are sensitive to new standards reduce capital issuances and investments. However, in our setting of industry-specific standards, comparability effects likely play a more prominent role, and thus, our prediction of increases in capital flows after implementation of industry-specific standards is not necessarily in conflict with the findings of Bird et al. (2020).

Table 2
Event study DiD results.

Dependent variable: *CapitalGrowth*			
Event window:	$t-5$ to $t+5$	$t-8$ to $t+8$	$t-3$ to $t+3$
Variables	(1)	(2)	(3)
Standard-8		−0.021	
		(-1.16)	
Standard-7		−0.015	
		(-0.82)	
Standard-6		−0.010	
		(-0.59)	
Standard-5	−0.022	−0.016	
	(-1.28)	(-1.04)	
Standard-4	0.004	0.011	
	(0.28)	(0.73)	
Standard-3	0.021	0.026*	0.017
	(1.35)	(1.70)	(1.00)
Standard-2	0.003	0.006	0.000
	(0.28)	(0.51)	(0.04)
Standard-1	—	—	—
Standard0	0.027**	0.026**	0.029***
	(2.29)	(2.09)	(2.63)
Standard1	0.038**	0.038***	0.041***
	(2.56)	(2.65)	(2.61)
Standard2	0.047**	0.046**	0.047**
	(2.46)	(2.44)	(2.50)
Standard3	0.053***	0.049***	0.058***
	(3.46)	(3.44)	(3.75)
Standard4	0.050***	0.049***	
	(3.55)	(3.59)	
Standard5	0.054**	0.050**	
	(2.44)	(2.33)	
Standard6		0.033**	
		(2.03)	
Standard7		0.024	
		(1.16)	
Standard8		0.057*	
		(1.94)	
SalesGrowth	0.316***	0.338***	0.293***
	(15.63)	(15.33)	(15.07)
Q	0.007**	0.013***	−0.004
	(1.98)	(3.90)	(-0.85)
Cash	−0.221***	−0.254***	−0.219***
	(-4.02)	(-4.83)	(-4.10)
Size	−0.293***	−0.239***	−0.373***
	(-19.89)	(-18.70)	(-23.42)
Leverage	0.175***	0.168***	0.206***
	(8.68)	(9.13)	(7.90)
ROA	0.212***	0.210***	0.210***
	(3.97)	(4.02)	(3.74)
Year-cohort FE	Yes	Yes	Yes
Firm-cohort FE	Yes	Yes	Yes
Observations	221,477	338,216	139,606
R-squared	0.420	0.394	0.462

The table reports OLS coefficient estimates and, in parentheses, robust *t*-statistics clustered at the three-digit industry level. All regressions are based on a stacked event estimation with 14 cohorts. In Column (1), the event window begins in year $t-5$ and ends in year $t+5$ around the effective year of the standard, $t=0$. In Column (2), the event window begins in year $t-8$ and ends in year $t+8$ around the effective year of the standard, $t=0$. In Column (3), the event window begins in year $t-3$ and ends in year $t+3$ around the effective year of the standard, $t=0$. We use treatment indicator variables for all years of the estimation window, except for the year before the standard is effective, $t-1$, which serves as the base year and is omitted from the regressions. We include year-by-cohort fixed effects as well as firm-by-cohort fixed effects. The dependent variable, *CapitalGrowth*, is the log of invested capital divided by lagged invested capital (equity and long-term debt). ***, **, and * indicate statistical significance at the 1, 5, and 10 percent level (two-tailed).

consistent accounting methods.

Even if implementation of industry standards leads to average increases in capital flows, it is unclear which firms benefit more from the new standards. We posit that small firms likely benefit more from implementation of new standards. This is because in equilibrium small firms provide fewer voluntary disclosures than large firms (Diamond and Verrecchia, 1991), resulting in higher information asymmetry between managers and investors, which in turn limits the capital provided to smaller firms (Myers and Majluf, 1984). In addition, the investment efficiency literature shows that financially constrained firms benefit more from increased financial reporting quality (e.g., Biddle et al., 2009) and increased audit quality (Shroff, 2020). Therefore, to the extent that implementation of industry standards mitigates information asymmetries between the firm and capital providers, we predict that small firms, firms with greater information asymmetry, and firms with more capital constraints benefit more from implementation of industry-specific standards.

3.2. Event study difference-in-differences

To test our predictions, we use a "stacked cohort" regression model (Armstrong et al., 2022; Baker et al., 2022; Cengiz et al., 2019; Gormley and Matsa, 2011). To do this, we create separate datasets for each of the industry standards identified in Appendix I. Some standards have the same effective date (e.g., SFAS 66, 67, and 68). In such cases, we treat these standards as one group (hereafter, "cohort"). For each cohort, we define an event window that begins in year $t-5$ and ends in year $t+5$ around the effective year of the standard, $t=0$. Firms in industries that are affected by the industry standards are labeled as treated firms, while firms in industries that are not affected by the industry standards in the event window are labeled as control firms. We then stack the 14 resulting cohorts into one dataset. This resulting dataset contains 221,447 firm-year observations, 16,553 of which relate to treatment firm-years and 204, 924 relate to control firm-years. Such a stacked event design avoids already-treated observations being used as controls for later treatments and eliminates the bias that might arise from time-varying treatment effects when using a staggered difference-in-differences (DiD) design (Baker et al., 2022; Barrios, 2021; Goodman-Bacon, 2021). The resulting "stacked" linear regression model is given by Equation (1) [5]:

$$CapitalGrowth_{it} = \alpha_{tc} + \alpha_{ic} + \sum_{k=-5}^{k=5} \gamma_k Standard_{k,jt} + \mu Controls_{it-1} + \varepsilon_{it} \quad (1)$$

where i indexes firms, j indexes industries, t indexes fiscal years, and c indexes cohorts, and α_{ct} and α_{ic} are year-by-cohort and firm-by-cohort fixed effects. The dependent variable, *CapitalGrowth*, is the log of invested capital divided by lagged invested capital (equity and long-term debt). Our dependent variable is closely linked to the conceptual construct of providing capital (Armstrong et al., 2022; Roychowdhury et al., 2019). We use treatment indicator variables for all years of the event window that begins in $t-5$ and ends in $t+5$ around the effective year, $t=0$, except for the year before the standard is effective, $t-1$, which serves as the base year and is omitted from the regression. If the implementation of an industry-specific accounting standard leads to an increase in capital growth, then the coefficients on $Standard_0$, $Standard_1$, $Standard_2$, $Standard_3$, $Standard_4$, and $Standard_5$ are positive and significant.

A key advantage of this research design described by Equation (1) is that it permits comparison of the effects of industry standards with those of a control group of firms from the same country (the U.S) under supervision of the same regulatory authority (the SEC) but from different, unaffected industries. However, as is the case with other studies examining disclosure regulations, the implementation of industry standards by the FASB is not exogenous, and thus our documented effects might be subject to industry trends or concurrent economic shocks (e.g., Leuz and Wysocki, 2016). We address these possibilities in Section 3.4 below.

We use a set of control variables, *Controls*, to account for factors such as the demand for external capital, investment opportunities, or self-financing ability. Because we are unaware of any studies that use capital growth as an outcome variable, we borrow from the investment efficiency literature (Biddle et al., 2009; Biddle and Hilary, 2006) for selecting our control variables: *SalesGrowth* defined as the log of sales divided by lagged sales, *Q* defined as market value of equity plus total liabilities divided by total assets, *Cash* defined as lagged cash divided by lagged total assets, *Size* defined as the log of lagged total assets, *Leverage* defined as lagged total liabilities divided by lagged total assets, and *ROA* defined as net income divided by lagged total assets.[6] We winsorize all continuous variables at the 1st and 99th percentiles.[7] Appendix II provides a detailed description of how the variables we use in our study are calculated. To account for potential correlations in capital flows within an industry, we cluster standard errors at the SIC 3-digit industry level.[8]

Table 2, Column (1), presents findings relating to the estimation of Equation (1). The findings reveal that all of the $Standard_0$, $Standard_1$, $Standard_2$, $Standard_3$, $Standard_4$, and $Standard_5$ coefficients are significantly positive, which support our prediction that

[5] For ease of exposition, we use the same notation for coefficients in all equations that follow.

[6] To avoid any potential contamination by the inclusion of post-treatment covariates as control variables in the regressions (Angrist and Pischke, 2009), we also estimate our regressions without control variables or excluding certain controls that might be affected by the treatment (Whited et al., 2022), e.g., *ROA* or *SalesGrowth*. Findings from these analyses presented in Online Appendix A reveal the same inferences as those based on tabulated findings.

[7] We also consider alternative methods to address whether a few influential observations (Leone et al., 2019) affect our inferences. These include estimating robust regressions, excluding observations with studentized residuals greater than [± 2], and using the raw (unwinsorized) data. Findings from these alternative estimations yield the same inferences as those based on tabulated findings.

[8] This clustering approach leads to a nested fixed effects structure (Conley et al., 2018) for the firm-by-cohort fixed effects. In Online Appendix B, we present findings from analyses using alternative clustering of standard errors. Our inferences are not sensitive to these alternative clustering approaches.

implementing an industry-specific accounting standard is associated with an increase in capital growth in subsequent years. The coefficients range from 0.027 to 0.054, with an average of 0.045, which implies an average increase of approximately 4.5% in capital growth over the post-period relative to the pre-period for firms in an industry after implementation of a standard that affects their industry.[9] In contrast, the Column (1) findings reveal that each of the pre-implementation year coefficients, i.e., those on *Standard*_2, *Standard*_3, *Standard*_4, and *Standard*_5, is insignificantly different from zero.

The coefficients relating to the control variables, *SalesGrowth* and *Q*, are significantly positive, indicating that firms with greater growth opportunities obtain more capital (e.g., Biddle et al., 2009; Biddle and Hilary, 2006). The coefficient estimates for *Size* and *Cash* are significantly negative, which is consistent with larger firms with more cash available having lower external capital needs. The positive coefficients for *Leverage* and *ROA* are consistent with more leveraged and profitable firms having higher capital growth. Table 2, Columns (2) and (3), present findings from estimating versions of Equation (1) based on alternative windows ($t-8$ to $t+8$) and ($t-3$ to $t+3$) around the effective year, $t=0$. The findings reveal inferences that are consistent with those based on the Column (1) estimation, suggesting that the inferences regarding capital growth are not sensitive to the choice of length of window.

Fig. 1 plots point estimates together with 95% confidence intervals based on the regression model from Table 2, Column (1). The figure illustrates two key findings. First, the confidence interval for each of the $Standard_k$ coefficients in the years following implementation of the standard lies above zero. This finding is additional evidence that implementation of industry-specific accounting standards leads, on average, to an increase in capital growth in subsequent years. Second, in contrast, the confidence interval for each of the $Standard_k$ coefficients in the years preceding implementation of the standard straddles zero. Inspection of the magnitudes of the pre-implementation coefficients reveals some evidence of an upward (capital-increasing) trend from year $t-5$ to year $t-3$, but no pre-trend just before the effective year, which would be problematic for our inference that capital growth is attributable to the implementation of the industry-standards. Because there usually is a time lag between the year in which the FASB initiated the standard and the implementation year by firms (see Appendix I), the pattern in Fig. 1 could be an indication that the FASB reacts to industry trends. However, it is not obvious that such a pattern necessarily confounds our estimated treatment effects after the implementation date.

3.3. Sources of capital flows

We next investigate whether the increase in capital growth following implementation of industry-specific accounting standards applies to different sources of capital: all equity sources (capital growth excluding long-term debt), long-term debt, core capital (equity growth excluding retained earnings), debt issuance, and equity issuance. For the sake of parsimony, we conduct this analysis by replacing the yearly treatment indicators with the variable, *PostStandard*, that equals one for affected firms after implementation of an industry-specific standard, and zero otherwise. This results in the following regression model stated by Equation (2):

$$SourceGrowth_{it} = \alpha_{tc} + \alpha_{ic} + \gamma PostStandard_{jt} + \mu Controls_{it-1} + \varepsilon_{it} \quad (2)$$

SourceGrowth can be either *CapitalGrowth*, *AllEquity*, *LTDebt*, *CoreCapital*, *DebtIssuance*, or *EquityIssuance*. Unlike the other *SourceGrowth* measures, *DebtIssuance* and *EquityIssuance* are not annual growth measures but rather capital flow measures defined as the net debt issuance and the sum of the proceeds raised from the sale of common and preferred stock, respectively, scaled by lagged total assets (Shroff, 2020). There is a substantial literature that suggests that debt capital providers have access to information about the firm that is not necessarily available to equity investors (Beatty et al., 2009; Bharath et al., 2008; Plumlee et al., 2015). Hence the information provided by financial statements following implementation of a new industry standard may have less of an impact on the growth in long-term debt and firms' debt issuances.

Table 3, Columns (1) through (6), present the findings from estimation of Equation (2) using the six *SourceGrowth* measures. Not surprisingly, the findings relating to the *CapitalGrowth* specification in Column (1) reveal that the *PostStandard* coefficient, 0.042, is significantly positive and equal to approximately the average of the six post-standard coefficients in Table 2, Column (1). The findings in Table 3, Column (2) relating to the *AllEquity* specification reveal a *PostStandard* coefficient, 0.043, which is almost identical to that in Column (1). The findings in Columns (3) indicate that the *PostStandard* coefficient for the *LTDebt* specification is not significantly different from zero, but the magnitude of the coefficient is similar to that in the equity growth specification.

The finding in Column (4) relating to *CoreCapital* reveals a significantly positive coefficient of 0.052 for *PostStandard*. The fact that the *PostStandard* coefficient is not smaller for *CoreCapital* than for *AllEquity* suggests that our findings are not attributable to growth in retained earnings, but rather to new equity provided to the firm in the post-period. This interpretation is further supported by the findings in Column (6) when *EquityIssuance* is the dependent variable. The significantly positive *PostStandard* coefficient, 0.031, is consistent with the industry standards enabling firms to raise more equity capital.[10] In contrast, the estimated *PostStandard* coefficient

[9] Because we use a loglinear specification, the estimated coefficient implies an incremental increase in capital growth of 4.5%. This effect corresponds to an average incremental increase in invested capital of $85 million, relative to the mean invested capital of our sample firms. We deem this effect as economically meaningful but not too large to be implausible. For comparison, the average debt and equity capital raised by U.S. firms between 1990 and 2001 amounts to $535 million per issuance (Henderson et al., 2006), and the average capital raised by initial public offerings (IPOs) amounts to $78 million per deal (Ritter and Welch, 2002). Also, our estimated treatment effect is of similar magnitude compared to that of Cho (2015). Although using a different setting and specification, Cho (2015) finds that affected firms increase their internal capital allocation to segments with higher opportunities by 3.6% after adoption of SFAS 131.

[10] Our inferences are robust to estimating Equation (2) with a common sample, i.e., using the same number of observations (N = 169,400) for all six *SourceGrowth* measures.

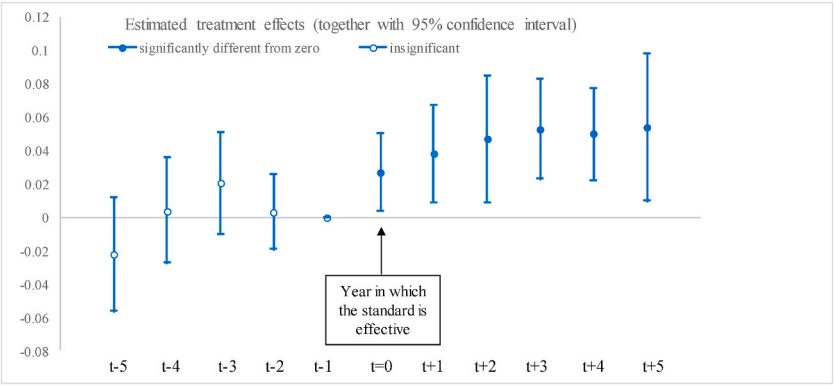


Fig. 1. Treatment Effects over Time for *CapitalGrowth*
This figure plots point estimates together with 95% confidence intervals from a regression of capital growth on yearly treatment indicators. We include year-by-cohort fixed effects as well as firm-by-cohort fixed effects, and we cluster standard errors at the three-digit industry level. The sample comprises 14 cohorts (i.e., affected and unaffected firms around implementation of industry-specific standards) based on an event window that begins in year $t-5$ and ends in year $t+5$ around the effective year of the standard, $t=0$. We use treatment indicator variables for all years of the estimation window except for the year before the standard is effective, $t-1$, which serves as the base year and is omitted from the regression. The dependent variable, *CapitalGrowth*, is the log of invested capital divided by lagged invested capital (equity and long-term debt).

Table 3
Sources of capital flows.

Dependent variable:	*Capital Growth*	*All Equity*	*LT Debt*	*Core Capital*	*Debt Issuance*	*Equity Issuance*
Variables	(1)	(2)	(3)	(4)	(5)	(6)
PostStandard	0.042***	0.043***	0.033	0.052***	0.007	0.031***
	(2.90)	(3.23)	(1.25)	(4.41)	(1.10)	(3.80)
SalesGrowth	0.316***	0.292***	0.404***	0.184***	0.045***	0.137***
	(15.63)	(12.93)	(16.56)	(10.23)	(6.40)	(9.03)
Q	0.007**	0.007**	−0.025***	0.007*	−0.002**	0.012***
	(1.99)	(2.37)	(-3.27)	(1.75)	(-2.53)	(4.39)
Cash	−0.221***	−0.222***	0.175**	−0.351***	−0.029***	−0.316***
	(-4.02)	(-4.50)	(2.54)	(-6.56)	(-3.02)	(-6.66)
Size	−0.293***	−0.276***	−0.241***	−0.215***	−0.042***	−0.200***
	(-19.89)	(-17.11)	(-21.14)	(-13.03)	(-15.00)	(-12.47)
Leverage	0.175***	0.464***	−1.085***	0.292***	−0.138***	0.123***
	(8.68)	(9.37)	(-18.68)	(8.21)	(-10.10)	(5.23)
ROA	0.212***	0.362***	−0.359***	−0.110*	−0.057***	−0.298***
	(3.97)	(4.45)	(-6.83)	(-1.84)	(-5.36)	(-12.67)
Year-cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	221,477	216,968	180,891	218,338	205,805	215,473
R-squared	0.420	0.397	0.227	0.375	0.293	0.537

The table reports OLS coefficient estimates and, in parentheses, robust *t*-statistics clustered at three-digit industry level. All regressions are based on a stacked DiD with 14 cohorts, using an event window that begins in year $t-5$ and ends in year $t+5$ around the effective year of the standard, $t=0$. We include year-by-cohort fixed effects as well as firm-by-cohort fixed effects. The dependent variables are: capital growth (*CapitalGrowth)*, capital growth excluding long-term debt (*AllEquity*), long-term debt (*LTDebt*), growth in common stock and capital surplus (*CoreCapital*), net debt issuance scaled by lagged total assets (*DebtIssuance*), and equity issuance (*EquityIssuance*) defined as the sum of the proceeds raised from the sale of common and preferred stock, scaled by lagged total assets. *PostStandard* is an indicator variable that equals one for firm-years following implementation of an industry-specific standard. See Appendix II for a more detailed variable description. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent level (two-tailed).

in the *DebtIssuance* specification, 0.007 (*t*-statistic = 1.10) (column 5), is statistically and economically insignificant. In addition, the difference of 0.024 between the coefficient estimate in the *DebtIssuance* specification (column 5) and *EquityIssuance* specification (column 6) is significant at the 1% level (*t*-statistic = 3.21), suggesting that affected firms raise more equity, but not more debt capital.[11]

Collectively, Table 3 findings suggest that implementation of industry-specific standards affect primarily equity capital flows. Therefore, in the remainder of this study, we present findings for estimations using *CapitalGrowth* and *EquityIssuance*. In addition, Online Appendix C presents descriptive statistics and a replication of Table 2 for *EquityIssuance* as the dependent variable. The findings in Online Appendix C and Fig. 2 are consistent with a marked increase in new equity provided to firms after implementation of industry standards and with no significant nor systematic pre-trends in equity issuances.

3.4. Confounding industry-specific trends

We next address the possibility that our findings are attributable to industry-specific trends or economic events unrelated to industry-specific standards. It is also possible that our finding that implementation of industry-specific standards results in a subsequent increase in capital flows reflects the reverse effect, namely that the FASB tends to introduce such standards in industries that are experiencing capital growth. Although this is possible, the findings reported in Table 2 as well as in Figs. 1 and 2 showing no significant nor systematic trends in capital flows before the effective date of the standard are inconsistent with this possibility. In other words, if the FASB reacts to capital growth trends in particular industries, this likely manifests as an increase in capital flows before the effective date of an industry-specific standard. Even if the FASB's decision to initiate an industry-specific standard is influenced by trends in the industry, there typically is a time lag between the initiation year and the year a particular standard is ultimately implemented by firms. Because we use the implementation year and because there is variation in these time lags, it is unlikely that industry trends systematically affect inferences from our tests. Nonetheless, we conduct several tests to examine whether industry-specific trends affect our inferences.

First, we modify Equation (2) using the methodology of Bertrand and Mullainathan (2003) and Giroud (2013) by including an additional control variable that reflects industry trends in capital flows. Specifically, we include an additional control variable, *IndustryCapitalTrend* (*IndustryEquityTrend*), which is the median *CapitalGrowth* (*EquityIssuance*) of unaffected Canadian firms—i.e., firms listed on the Toronto, Montreal, or Vancouver Stock Exchange—in the same 2-digit industry in a given year.[12] To the extent that industry trends are similar across U.S. and Canadian firms, inclusion of this control variable should account for contemporaneous economic factors affecting specific industries. The results presented in Table 4, Columns (1) and (2) reveal that the industry trend coefficients are insignificant. More importantly, the *PostStandard* coefficients, 0.049 and 0.035, are significantly positive and of similar magnitude to those reported in Table 3.[13]

Second, following Christensen et al. (2016), we estimate Equation (2) by adding Canadian firms to our sample. This permits us to include *industry-by-year-by-cohort* fixed effects (at the 2-digit industry level) that absorb industry-specific changes in capital growth and equity issuances, respectively, and that otherwise could not be included when our sample is limited to U.S. firms, as is the case in Tables 2 and 3 However, this can only be done for those industries for which we have a sufficient number of Canadian firm-year observations. Therefore, we use the ($t-8$ to $t+8$) window to expand the number of available sample firm-years.[14] The results presented in Table 4, Columns (3) and (4) reveal that the *PostStandard* coefficients, 0.068 and 0.053, are significantly positive. This finding suggests that only those firms that are subject to a change in accounting standards (i.e., U.S. listed firms and not Canadian listed firms) exhibit increases in capital flows following implementation of such standards.

Third, we estimate Equation (2) using *CurrentLiabilities*, the log of current liabilities divided by lagged current liabilities, as the dependent variable. The implementation of industry-specific standards is unlikely to affect the growth in current liabilities, at least not in the short term, because these are the outcome of firm-level operating decisions rather than decisions by investors to provide capital. As such, *CurrentLiabilities* can be viewed as a placebo outcome variable. Consistent with this prediction, the findings in Table 4, Column (5) reveal that the *PostStandard* coefficient is insignificant (coefficient = 0.009, *t*-statistic = 0.73).

Fourth, we exploit differences in the length of the standard setting process to isolate situations where capital growth can take place regardless of any information effects of a new standard. If initiation of new industry standards reflects the FASB's reaction to industry trends, it is difficult to distinguish whether a positive *PostStandard* coefficient arises from such trends or is the result of investors acting

[11] We assess statistical significance of differences in *PostStandard* coefficients based on a full interaction model (treatment variable, control variables, and fixed effects) with an indicator for the respective outcome group (equity vs. debt) and a stacked sample of the two outcome groups. This procedure allows us to replicate the results in Columns (5) and (6), and we obtain a significant interaction term of 0.024 (*t*-statistic = 3.21, *p*-value = 0.002) for the difference between the *DebtIssuance* and *EquityIssuance* specifications. In addition, our inferences are unaffected when using a bootstrapping procedure with clustering at the 3-digit SIC level to test for differences across specifications.

[12] Our inferences are unaffected when we calculate *IndustryCapitalTrend* (*IndustryEquityTrend*) based on the 1-digit industry or when we use industry means instead of medians.

[13] The smaller sample sizes in Table 4, Column (1) and (2) specifications reflect the fact there is missing capital flow data for Canadian firms in several two-digit industries and sample years.

[14] As expected, when we use the shorter ($t-5$ to $t+5$) window, the *PostStandard* coefficients are somewhat smaller in magnitude and have lower levels of significance (*CapitalGrowth* and *EquityIssuance* coefficients = 0.045 and 0.032; *t*-statistics = 1.76 and 1.32), which is consistent with lower estimation power arising from the smaller number of observations.

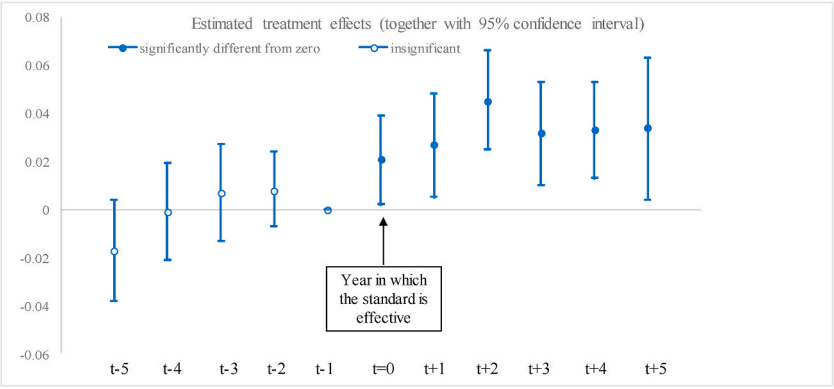


Fig. 2. Treatment Effects over Time for *EquityIssuance*
This figure plots point estimates together with 95% confidence intervals from a regression of equity issuances on yearly treatment indicators. We include year-by-cohort fixed effects as well as firm-by-cohort fixed effects, and we cluster standard errors at the three-digit industry level. The sample comprises 14 cohorts (i.e., affected and unaffected firms around implementation of industry-specific standards) based on an event window that begins in year $t-5$ and ends in year $t+5$ around the effective year of the standard, $t=0$. We use treatment indicator variables for all years of the estimation window except for the year before the standard is effective, $t-1$, which serves as the base year and is omitted from the regression. The dependent variable, *EquityIssuance*, is the sum of the proceeds raised from the sale of common and preferred stock, scaled by lagged total assets.

on better information provided by the new standards. Standards with a relatively short time lag between initiation and effective year—such as those illustrated in Fig. 3, Panel A—are particularly prone to such endogeneity concerns. In contrast, standards with a longer standard setting processes—such as those illustrated in Fig. 3, Panel B—exhibit a longer time lag between the initiation and effective year.

We therefore perform tests exploiting the subsample of standards with relatively long, i.e., above median, standard setting process: standards with three and more years between initiation and effective date.[15] Specifically, we estimate a version of Equation (2) replacing *PostStandard* with an indicator variable, *PostInitiation*, that equals one for affected industries after initiation of the standard, and we use a 3-year event window around the placebo initiation year. In doing so, we ensure that the standard is not yet effective in the post-initiation period, and thus, any capital flow effects cannot be attributed to the standard but to other factors or trends. For comparison, we also estimate Equation (2) for this subsample of standards using the 3-year event window around the true effective year as illustrated in Fig. 3, Panel B. The economically and statistically insignificant coefficient estimates for *PostInitiation* in the *CapitalGrowth* and *EquityIssuance* estimations, Columns 6 and 7 of Table 4, indicate no change in capital flows after the initiation year (coefficients $= -0.013$ and 0.006; t-statistics $= -0.91$ and 0.71). In contrast, the analogous significant coefficient estimates for *PostStandard* in Columns 8 and 9 of Table 4 indicate an increase in capital flows after the effective year (coefficients $= 0.033$ and 0.036; t-statistics $= 3.14$ and 4.49), compared to the post-initiation period. These findings are consistent with the implementation of the industry standards, but not other factors or trends, explaining the increase in capital for affected firms.[16]

4. Which type of industry standard helps investors in making decisions about providing capital to firms?

Although we find that implementation of industry-specific accounting standards results in an, on average, increase in capital growth for the affected industries, this average likely masks heterogeneity in the effects of different types of standards. Such heterogeneity can limit the informativeness (generalizability) of the estimated average effect. As discussed in Section 3.1, industry standards can differ along various dimensions such as the extent to which they provide new information or impose more uniform accounting. Guided by prior literature (Corona et al., 2023; Healy and Palepu, 2001; Schipper, 2003; Watts and Zimmermann, 1990), we identify common characteristics of the individual standards and group them according to these characteristics. This process yields four different partitions: *AICPA codification*, *Judgment and accounting choices*, *Explicit guidance*, and *Uniformity*. See Online Appendix D for details about (robustness of) the classification of standards into the different groups.

[15] The median (mean) time lag between initiation of the industry standard and its effective year is 2 (2.55) years. There is substantial variation in the standard-setting process across industry standards. For example, whereas nine years passed between initiation and effective date for SFAS 143, *Accounting for Asset Retirement Obligations*, SFAS 46, *Financial Reporting and Changing Prices: Motion Picture Films,* was both initiated and effective in 1981.

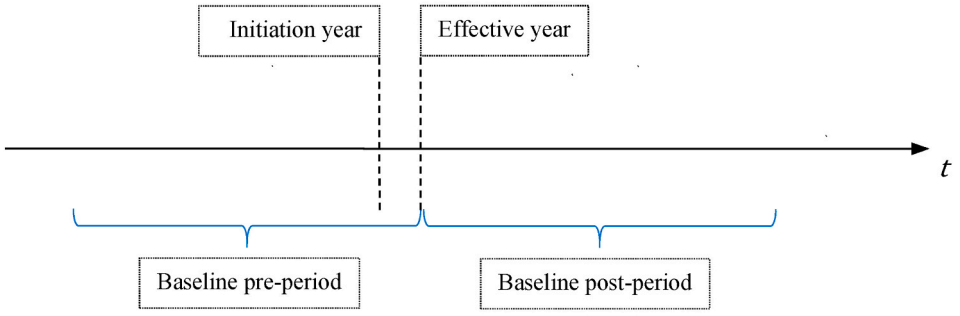
[16] Untabulated findings reveal that these inferences are the same when we exclude standards that mention other regulations or developments in the industry.

Table 4
Addressing the possibility of industry trends.

	Controlling for industry trends		Within-industry estimation		Placebo test	Subsample of standards with long standard setting process			
						Initiation year		Effective year	
Dependent variable:	*Capital Growth*	*Equity Issuance*	*Capital Growth*	*Equity Issuance*	*Current Liabilities*	*Capital Growth*	*Equity Issuance*	*Capital Growth*	*Equity Issuance*
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
PostStandard	0.049**	0.035***	0.068***	0.053**	0.009			0.033***	0.036***
	(2.44)	(3.11)	(2.67)	(2.20)	(0.73)			(3.14)	(4.49)
PostInitiation (placebo)						−0.013	0.006		
						(-0.91)	(0.71)		
IndustryCapitalTrend	−0.012								
	(-0.63)								
IndustryEquityTrend		0.018							
		(0.46)							
SalesGrowth	0.311***	0.140***	0.326***	0.143***	0.368***	0.307***	0.135***	0.292***	0.132***
	(13.63)	(7.14)	(13.75)	(9.03)	(24.19)	(14.66)	(9.48)	(15.29)	(9.54)
Q	0.004	0.011***	0.008*	0.012***	0.004*	−0.004	0.006*	0.001	0.009***
	(1.03)	(4.27)	(1.95)	(4.65)	(1.97)	(-0.76)	(1.87)	(0.19)	(2.99)
Cash	−0.215***	−0.379***	−0.252***	−0.321***	0.146***	−0.339***	−0.537***	−0.227***	−0.381***
	(-3.93)	(-7.20)	(-4.79)	(-6.30)	(5.68)	(-6.71)	(-8.42)	(-5.00)	(-9.31)
Size	−0.307***	−0.237***	−0.250***	−0.178***	−0.157***	−0.427***	−0.302***	−0.393***	−0.243***
	(-19.23)	(-14.15)	(-21.51)	(-13.10)	(-25.77)	(-21.92)	(-16.96)	(-22.89)	(-12.69)
Leverage	0.182***	0.108***	0.175***	0.117***	−0.511***	0.243***	0.185***	0.140***	0.089***
	(7.86)	(3.32)	(9.36)	(5.25)	(-15.86)	(8.60)	(5.84)	(4.86)	(3.15)
ROA	0.195***	−0.307***	0.218***	−0.291***	−0.227***	0.137***	−0.288***	0.133***	−0.308***
	(3.19)	(-13.88)	(4.39)	(-12.40)	(-17.89)	(3.27)	(-10.84)	(2.91)	(-13.43)
Year-cohort FE	Yes	Yes	No	No	Yes	Yes	Yes	Yes	Yes
Firm-cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year-cohort FE	No	No	**Yes**	**Yes**	No	No	No	No	No
Observations	170,298	124,333	351,922	336,915	216,177	69,350	67,959	73,850	72,436
R-squared	0.427	0.556	0.416	0.537	0.327	0.511	0.631	0.447	0.589

The table reports OLS coefficient estimates and, in parentheses, robust *t*-statistics clustered at the three-digit industry level. All regressions are based on a stacked event estimation with 14 cohorts. In Columns (1) and (2), we control for industry-specifc trends by including industry-year controls, which are calculated as the median *CapitalGrowth* and *EquityIssuance*, respectively, of unaffected Canadian firms (i.e., firms listed at Toronto, Montreal, or Vancouver Stock Exchange) in the same 2-digit industry in a given year. We use an event window that begins in year $t-5$ and ends in year $t+5$ around the effective year of the standard, $t=0$. We include year-by-cohort fixed effects as well as firm-by-cohort fixed effects. In Columns (3) and (4), we augment our sample by including unaffected Canadian firms, allowing us to impose additional industry-by-year-by-cohort fixed effects that absorb industry-specific shocks in capital growth and equity issuances, respectively. We use an event window that begins in year $t-8$ and ends in year $t+8$ around the effective year of the standard, $t=0$, and we include firm-by-cohort fixed effects as well as industry-by-year-by-cohort fixed effects at the 2-digit industry level. Column (5) is the baseline model (see column 1 of Table 3) with a placebo outcome variable, *CurrentLiabilities*, measured as the log of current liabilities divided by lagged current liabilities. In columns (6) through (9), we use a subsample of standards with above median standard setting process (i.e., three and more years between initiation and effective year) and an event window that that begins in year $t-3$ and ends in year $t+3$ around the initiation year (columns 6 and 7) and effective year (columns 8 and 9) of the standard. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent level (two-tailed).

A: Timeline for standards with relatively short standard setting process (below median = 0 to 2 years)



B: Timeline for standards with relatively long standard setting process (above median = 3 and more years)

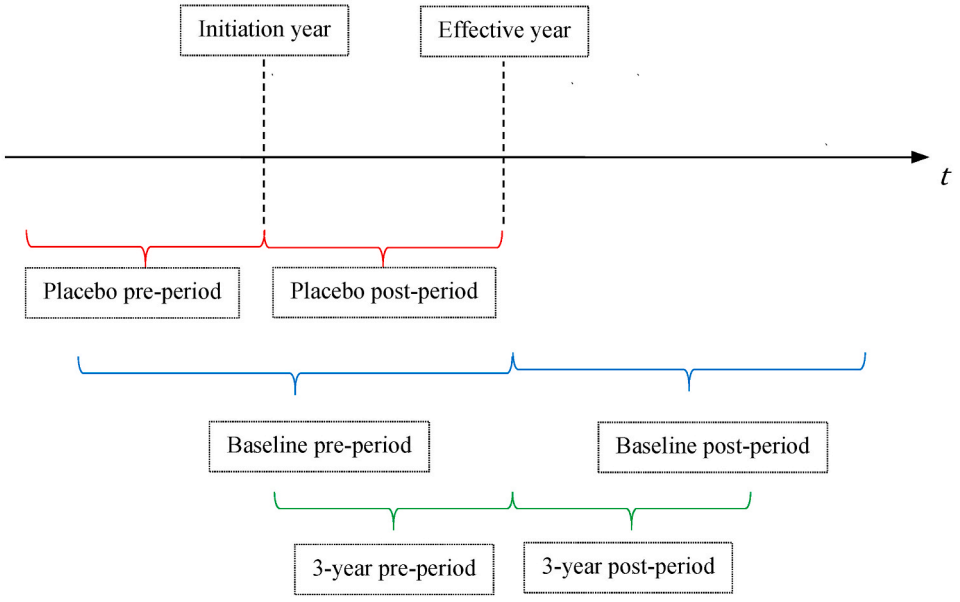


Fig. 3. Exploiting differences in the length of the standard setting process.
A: Timeline for standards with relatively short standard setting process (below median = 0–2 years) B: Timeline for standards with relatively long standard setting process (above median = 3 and more years).

4.1. AICPA codification

Some of our industry standards are simply a codification of existing AICPA industry guidance.[17] As such, the question arises whether these AICPA codification standards provide as much new information as another industry standard that makes substantial changes, e.g., in the way income is measured. On the one hand, codification of AICPA guidance into standards may reinforce and clarify application of the accounting guidance, as they are subject to the FASB's due process (FAS 32, para. 2) and can contain minor adjustments (FAS 66, para. 106; FAS 48, para. 22). On the other hand, the accounting changes are by definition not substantive (FAS 48, para. 24) and the incremental increase in transparency and comparability is limited if firms already apply these standards.

We thus test whether AICPA codification standards affect capital flows differently than other, non-AICPA codification standards. Specifically, we estimate Equation (2) by replacing *PostStandard* with two non-overlapping indicator variables, *PostStandard_AICPA* and *PostStandard_NoAICPA*. *PostStandard_AICPA* equals one for affected firms after implementation of industry-specific standards that codify existing industry guidance issued by the AICPA: SFAS 48, SFAS 50/51/53/63, SFAS 66/67, SFAS 139, and SFAS 152. *PostStandard_NoAICPA* equals one for affected firms after implementation of other, non-AICPA industry standards.

Columns (1) and (2) of Table 5 present the results. Whereas the coefficients on *PostStandard_AICPA* are insignificant, those on *PostStandard_NoAICPA* are significantly positive for both *CapitalGrowth* and *EquityIssuance*. In addition, the coefficients are significantly different from each other (p-values from F-tests are 0.048 and 0.017). These findings suggest that industry standards that simply codify existing AICPA pronouncement do not provide sufficient new information to investors that could alter their capital allocation decisions.

4.2. Judgment and accounting choices

The effect of more judgment and accounting choice on capital allocation decisions is *a priori* unclear. There is a longstanding debate about whether more accounting choice is "good" or "bad" (e.g., Healy and Palepu, 2001; Watts and Zimmermann, 1990). On the one hand, managers may use discretion to convey their private information. On the other hand, they might exercise discretion opportunistically. In our industry standards setting, allowing firms more judgment and choices, even if they are not exercised opportunistically, could emphasize firms' idiosyncrasies, in turn reducing comparability and making it more difficult for investors to direct capital to firms (Corona et al., 2023). We thus predict lower capital flows associated with the implementation of industry-specific standards that increase managerial judgment or accounting choices (or both) relative to other industry standards.[18]

To test this prediction, we replace *PostStandard* with two non-overlapping indicator variables, *PostStandard_Judgment* and *PostStandard_NoJudgment*. *PostStandard_Judgment* equals one for affected firms after implementation of industry-specific standards that increase managerial judgment (e.g., more estimates required) and/or increase accounting choices. Standards that increase managerial judgment are SFAS 39, SFAS 46, SFAS 54, SFAS 66/67, SFAS 80, and SFAS 86. *PostStandard_NoJudgment* equals one for affected firms after implementation of other, non-judgment industry-specific standards.

The results in Columns (3) and (4) of Table 5 reveal that the coefficients on *PostStandard_NoJudgment* are positive and significant, while the coefficients on *PostStandard_Judgment* are insignificant with a negative sign. The p-values (0.010 and 0.059) from the F-tests suggest that the coefficients are significantly different from each other. The findings suggest that industry-standards that increase managerial judgment and accounting choices are not helpful for investors in making decisions about providing capital to firms.

4.3. Explicit guidance

Accounting standards also differ in the extent to which they provide explicit implementation guidance such as illustrative examples or detailed disclosure guidance.[19] Schipper (2003) notes that detailed implementation guidance is intended to increase comparability. That is, accounting information is more comparable if firms closely follow the template for disclosures or account for transactions based on the illustrative examples provided by certain standards. However, if the guidance is overly strict, dissimilar transactions are forced to be accounted for in a similar way, in turn reducing comparability (Schipper, 2003). For industry-specific standards, the risk that detailed implementation guidance reduces comparability is likely lower, because firms and their transactions within an industry are plausibly more similar than firms across industries. Consistent with this reasoning, Fiechter et al. (2022) shows that the detailed disclosures required by SFAS 157 are useful to investors in assessing the impact of fair value measurements on earnings. Notably, the disclosure requirements were accompanied by a disclosure guidance template, FAS 157, para. 35A, which was consistently applied in the U.S banking industry (source: own data collection). We thus predict higher capital flows for industry standards providing more explicit guidance.

[17] For example, SFAS 66, *Accounting for Sales of Real Estate*, adopts the specialized profit recognition principles of the existing AICPA Industry Accounting Guides, *Accounting for Profit Recognition on Sales of Real Estate and Accounting for Retail Land Sales,* and AICPA Statements of Position 75-6 and 78-4.

[18] For example, in SFAS 46, *Financial Reporting and Changing Prices: Motion Picture Films*, the FASB concluded that additional flexibility should be provided for measurements of the effects of specific price changes on motion picture films. Likewise, SFAS 80, *Accounting for Futures Contracts*, allows for different ways to account for a change in the market value of the futures contract.

[19] For example, SFAS 90, *Regulated Enterprises—Accounting for Abandonments and Disallowances of Plant Costs,* and SFAS 92 *Regulated Enterprises—Accounting for Phase-in Plans,* include an appendix that provides specific examples on how to apply the standard.

Table 5
Which type of industry standard helps investors in making decisions about providing capital to firms?

	AICPA codification		Judgment and choice		Explicit guidance		Uniformity	
Dependent variable:	*Capital Growth*	*Equity Issuance*	*Capital Growth*	*Equity Issuance*	*Capital Growth*	*Equity Issuance*	*Capital Growth*	*Equity Issuance*
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PostStandard_AICPA	0.009	0.010						
	(0.49)	(1.09)						
PostStandard_NoAICPA	0.055***	0.039***						
	(3.26)	(4.21)						
PostStandard_Judgment			−0.031	−0.012				
			(-1.10)	(-0.51)				
PostStandard_NoJudgment			0.051***	0.037***				
			(3.48)	(4.32)				
PostStandard_Guidance					0.068***	0.048***		
					(3.49)	(4.71)		
PostStandard_NoGuidance					0.017	0.015		
					(1.09)	(1.56)		
PostStandard_Uniformity							0.055***	0.040***
							(3.56)	(4.71)
PostStandard_NoUniformity							−0.017	−0.010
							(-0.81)	(-0.57)
F-test [p-value]	[0.048]	[0.017]	[0.010]	[0.059]	[0.007]	[0.003]	[0.001]	[0.000]
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	221,477	215,473	221,477	215,473	221,477	215,473	221,477	215,473
R-squared	0.420	0.537	0.420	0.537	0.420	0.537	0.420	0.537

The table reports OLS coefficient estimates and, in parentheses, robust *t*-statistics clustered at the three-digit industry level (using the baseline specification). In columns (1) and (2), *PostStandard_AICPA* equals 1 for affected firms after implementation of industry-specific standards that codify existing industry guidance issued by the AICPA: SFAS 48, SFAS 50/51/53/63, SFAS 66/67, SFAS 139, and SFAS 152. *PostStandard_NoAICPA* equals 1 for affected firms after implementation of other (non-AICPA) industry-specific standards. In columns (3) and (4), *PostStandard_Judgment* equals 1 for affected firms after implementation of industry-specific standards that increase managerial judgment (e.g., more estimates required) and/or increase accounting choices: SFAS 39, SFAS 46, SFAS 54, SFAS 66/67, SFAS 80, SFAS 86. *PostStandard_NoJudgment* equals 1 for affected firms after implementation of other (non-judgment) industry-specific standards. In columns (5) and (6), *PostStandard_Guidance* equals 1 for affected firms after implementation of industry-specific standards that provide illustrative examples and additional disclosure guidance: SFAS 9, SFAS 50/51/53/63, SFAS 71, SFAS 80, SFAS 90, SFAS 92, SFAS 143. *PostStandard_NoGuidance* equals 1 for affected firms after implementation of other (non-guidance) industry-specific standards. In columns (7) and (8), *PostStandard_Uniformity* equals 1 for affected firms after implementation of industry-specific standards that increase standardization and uniformity of accounting (e.g., prescribing consistent recognition and measurement methods, reducing divergent accounting practices, or specifying accounting rules for certain transactions and circumstances): SFAS 9, SFAS 19; SFAS 50/51/53/63, SFAS 68, SFAS 71, SFAS 90, SFAS 92, SFAS 143, SFAS 152, SFAS 167. *PostStandard_NoUniformity* equals 1 for affected firms after implementation of other (non-uniform) industry-specific standards. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent level (two-tailed).

To test this prediction, we replace *PostStandard* with two non-overlapping indicator variables, *PostStandard_Guidance* and *PostStandard_NoGuidance*. *PostStandard_Guidance* equals one for affected firms after implementation of industry-specific standards that provide illustrative examples and additional disclosure guidance. Standards that provide guidance are SFAS 9, SFAS 50/51/53/63, SFAS 71, SFAS 80, SFAS 90, SFAS 92, and SFAS 143. *PostStandard_NoGuidance* equals one for affected firms after implementation of other, non-guidance industry-specific standards.

The results in Columns (5) and (6) of Table 5 show that the coefficients on *PostStandard_Guidance* are significant and positive for both *CapitalGrowth* and *EquityIssuance*, but the coefficients on *PostStandard_NoGuidance* are insignificant. The coefficients are significantly different from each other (p-values from F-tests are 0.007 and 0.003). These findings suggest that industry standards providing explicit guidance are helpful for investors in making their capital allocation decisions.

4.4. Uniformity

Heterogeneity in industry standards could also arise because some standards require more uniformity and leave less to the discretion of management compared to other standards. For example, some industry standards prescribe specific rules on how to account for certain transactions, and some other industry standards reduce the number of accounting choices available to firms.[20] Requiring more uniformity in accounting across more homogeneous firms—i.e., firms in the same industry—could enable investors to make better capital allocation decisions (Corona et al., 2023; Dye and Sridhar, 2008). Therefore, industry standards that increase uniformity of accounting practices within an industry should improve comparability, thereby increasing capital flows for firms in affected industries.

To test this prediction, we amend Equation (2) by replacing *PostStandard* with two non-overlapping indicator variables, *PostStandard_Uniformity* and *PostStandard_NoUniformity*. *PostStandard_Uniformity* equals one for affected firms after implementation of industry-specific standards that increase accounting uniformity; for example, prescribing consistent recognition and measurement methods, reducing divergent accounting practices, or specifying accounting rules for certain transactions and circumstances. Standards that increase uniformity are SFAS 9, SFAS 19; SFAS 50/51/53/63, SFAS 68, SFAS 71, SFAS 90, SFAS 92, SFAS 143, SFAS 152, and SFAS 167. *PostStandard_NoUniformity* equals one for affected firms after implementation of other, non-uniform industry-specific standards.

Findings in Columns (7) and (8) show that whereas the coefficients on *PostStandard_Uniformity* are significantly positive, those on *PostStandard_NoUniformity* are insignificant. In addition, the coefficients on *PostStandard_Uniformity* are significantly larger than those on *PostStandard_NoUniformity*, as indicated by the F-test (*p*-values of 0.001 and 0.000). These findings suggest that industry standards that increase accounting uniformity lead to more capital flows to firms.

4.5. Heterogeneity in standard-specific estimates

To gauge the extent to which the different, individual standards affect capital flows, we also estimate Equation (2) separately for each of our 20 industry standards. We note, however, that these separate estimations are more prone to estimation error (as compared to the grouping of standards in Table 5), that some standards are implemented concurrently with other standards (e.g., SFAS 44 and SFAS 46 in 1981), and that generalizability of these 20 "case studies" is inherently limited.

Fig. 4 A (B) plots the coefficient estimates for each industry standard in our sample using *CapitalGrowth* (*EquityIssuance*) as dependent variable. Depending on the classification of the standard (i.e., *Uniformity*, *Guidance*, *Judgment*, *AICPA*, and *None*), we use a different color for the plotted coefficients. The Figure reveals substantial heterogeneity in the effects of the individual standards on capital flows. Some standards are associated with capital increases, others have no effect, and some standards have even a negative impact.[21] The graphical pattern is largely consistent with the notion that standards increasing uniformity and providing explicit guidance are associated with capital increases, whereas standards with more managerial judgment or accounting choice are associated with negative capital flows. Not surprisingly, the AICPA codification standards have essentially no effect on capital flows.

In addition to the graphical illustration of the standard-level heterogeneity, we also perform small sample (N = 20) regressions with the separate coefficient estimates as dependent variables and the different classifications as explanatory variables. Findings tabulated in Online Appendix E reveal two insights. First, our classifications of different types of standards explain a substantial part of the variation in capital flows across industry standards, as indicated by the R-squared values of 65.9% and 40.6% in the capital growth and equity issuance specifications. In the regressions for separate classifications (columns 2 to 5 and 7 to 10), *Uniformity* explains more variation in the dependent variables than the other explanatory variables. Second, the coefficient estimates for the different classifications are consistent with our inferences above, i.e., we obtain insignificant coefficients for *AICPA*, negative coefficients for *Judgment*, and positive coefficients for *Guidance* and *Uniformity*.

In sum, although we caution the reader not to put too much emphasis on the analyses from separate estimation of standards, the results reveal substantial heterogeneity of the effects of industry standards, and this variation is consistent with our analyses in Table 5 and our classification of the type of standards according to their underlying characteristics.

5. Which firms benefit from implementation of industry-standards?

The results from the preceding tests document an on average increase in capital flows after implementation of industry standards, and these effects are more pronounced for those standards that provide new information, provide explicit guidance, or increase uniformity of accounting practices. However, it is unclear which firms benefit from the implementation of these standards. Based on

[20] For example, SFAS 143 *Accounting for Asset Retirement Obligations*, eliminates different options to account for asset retirement obligations. As another example, SFAS 71 *Accounting for the Effects of Certain Types of Regulation*, requires consistent recognition of regulatory events by affected entities (e.g., capitalization of costs when the regulator assures that cost will be recovered).

[21] We note that the coefficient estimates for the SFAS 86 event of −0.129 and −0.154 are substantial in amount and negative. We thus test whether our inferences are affected by this potential "outlier" event. When excluding SFAS 86 from the regressions, our main inferences as well as those relating to the partitions in Table 5 are unaffected. For example, even though the coefficient of −0.19 on *PostStandard_Judgment* becomes less negative than the coefficient of −0.31 presented in Table 5, the difference between *PostStandard_Judgment* and *PostStandard_NoJudgment* is significant (p-value = 0.018).

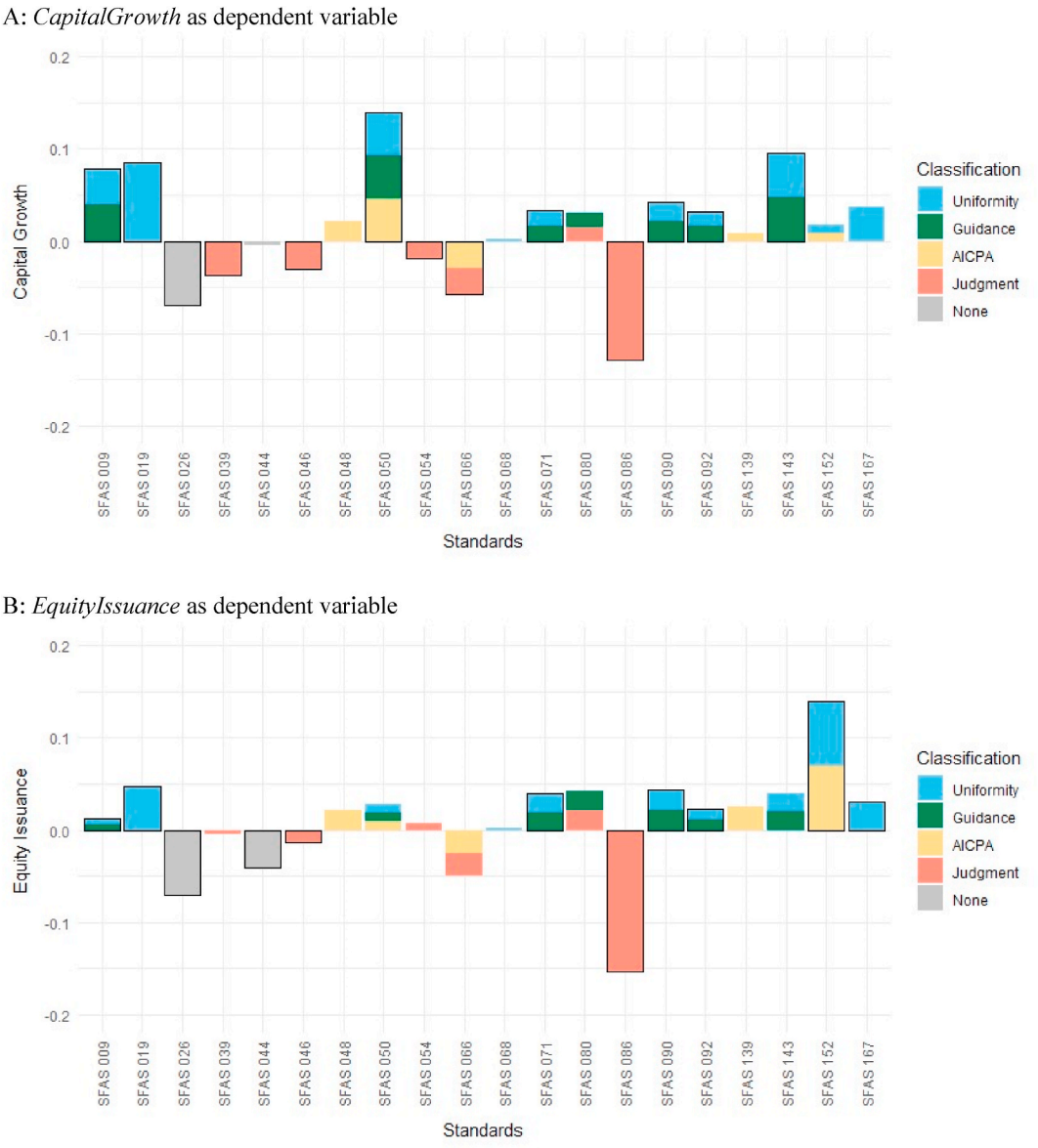


Fig. 4. Heterogeneity in standard-specific estimates. A: *CapitalGrowth* as dependent variable B: *EquityIssuance* as dependent variable
This figure plots the coefficients of estimating Equation (2) separately for each of the 20 industry standards in our sample for *CapitalGrowth* (A) and *EquityIssuance* (B) as dependent variables. The standards are classified as *AICPA codification*, *Judgment and accounting choices*, *Explicit guidance*, and *Uniformity* (see section 4 and online appendix D). A bar with a black outside border indicates statistical significance at the 10 percent level or lower (two-tailed).

our predictions in Section 3.1, we test whether small firms, firms with greater information asymmetry, and firms with capital constraints benefit more from implementation of industry-specific standards. Specifically, we estimate Equation (2) for the subsample of firms we identify before implementation of the standard as (1) being ex ante smaller, (2) having relatively high ex ante information asymmetry, and (3) having greater ex ante capital constraints. Because we expect these firms to benefit more from implementation of the new industry standards, we predict that the increase in capital flows is concentrated among these firms.

Regarding firm size, we designate ex ante small firms as firms with below median *Size* (i.e., log of total assets) averaged over the two years before the standard becomes effective. Regarding ex ante information asymmetry, we split the full sample (i.e., treatment and control groups) into firms with above (below) median Amihud (2002) illiquidity score averaged over the two years before the standard becomes effective. Regarding ex ante capital constraints, we split the full sample into firms we identify as capital constrained in one of the two years before the standard becomes effective. To identify capital constrained firms, we adopt the two-stage regression methodology from Biddle et al. (2009). In the first stage, we regress capital growth on the nonaccounting quality-based determinants of capital growth and year-cohort and firm-cohort fixed effects (i.e., model (1) of Table 3 without *PostStandard*) in the 5-year period

before implementation of the standard (i.e., $t-5$ to $t-1$). We define a firm as being relatively capital constrained if the residuals from the first-stage regression are in the bottom quartile.[22]

Findings in Panel A of Table 6 are consistent with our predictions. The findings reveal that the *PostStandard* coefficients in the capital growth and equity issuance estimations for the subsamples of small firms, firms with relatively high illiquidity, and firms with greater capital constraints are all significantly positive. In contrast, all but one of the corresponding coefficients for the subsample of large firms, firms with relatively low illiquidity, and firms with lower capital constraints are insignificant. In addition, untabulated findings from a full interaction model that includes a treatment variable, control variables, and fixed effects interacted with indicator variables for small and large firms reveal that the differences in the *PostStandard* coefficients between the two groups of firms are mostly statistically significant.[23] Therefore, small firms, firms with relatively high illiquidity, and firms with greater capital constraints benefit more from implementation of industry-specific standards.

Motivated by the heterogeneous effects of industry standards documented in Section 4, we also perform firm-level cross-sectional tests for different types of industry standards. Panels B and C of Table 6 reports estimation results for the *Uniformity* and *NoUniformity* partitions. For brevity, findings relating to estimations for the other partitions are tabulated in Online Appendix F. For the type of standards that we identify as being helpful for investors in making decisions about providing capital (e.g., explicit guidance, uniform accounting practices), we find that small firms, firms with relatively high illiquidity, and firms with greater capital constraints benefit more from implementation of such standards than large firms, firms with relatively low illiquidity, and firms with lower capital constraints. The differences in the *PostStandard* coefficients between the subsamples are significant. For other standards (e.g., AICPA codification, more judgment and accounting choices), we find no different effects across different firms.

6. Is implementation of industry-specific standards associated with increases in liquidity and comparability?

In Section 3, we propose that increases in transparency or comparability or both explain increases in capital flows after implementation of a new industry-specific accounting standard. Therefore, in this section, we shed light on these two channels and test whether industry standards (i) reduce information asymmetry, which should manifest as an increase in stock price liquidity, and (ii) increase financial statement comparability.

Specifically, we test whether implementation of industry-specific standards on average increases stock price liquidity by using the Amihud (2002) illiquidity index as a proxy for information asymmetry.[24] Following prior literature (e.g., Balakrishnan et al., 2014), we define the dependent variable, *AmihudIlliquidity*, as the log of the yearly average of a firm's daily Amihud index, i.e., ratio of absolute stock return to dollar volume (see Appendix II for a more detailed variable description). Because the measure is constructed such that higher values imply greater illiquidity, we predict that the *PostStandard* coefficient is negative. We amend Equation (2) by adding control variables prior research (Balakrishnan et al., 2014; Christensen et al., 2016) identifies as being associated with stock price liquidity: the natural logarithm of equity market capitalization, *LogMarketCap*, the natural logarithm of share turnover, *LogTurnover*, and the natural logarithm of equity volatility, *LogVolatility*.

We test whether implementation of industry-specific standards on average increases financial statement comparability by using the De Franco et al. (2011) measure, which is based on a time-series approach. We construct the measure by first estimating a regression of a firm's earnings on its own stock return and then compare the fitted value of earnings based on this regression to those obtained using the regression coefficients of other firms in the firm's industry. We adjust the measure to ensure that there is no overlap in the time series around the treatment events. Specifically, we use the 16 previous quarters to estimate the earnings-returns regressions in the period before the industry-specific standard is implemented and the 16 subsequent quarters in the period after the standard is implemented. This avoids any overlap in the time series between the pre- and post-period. We use the same set of controls as for the liquidity regressions. To the extent that comparability increases after implementation of industry-specific standards, we predict a positive coefficient for *PostStandard*.

When we conduct our tests for increases in liquidity and comparability, we also estimate Equation (2) replacing *PostStandard* with the different types of standards: *AICPA codification, Judgment and accounting choices*, *Explicit guidance*, and *Uniformity*. If, for example, standards providing more guidance help investors in making better comparisons across affected firms, we expect a more pronounced increase in financial statement comparability for *PostStandard_Guidance* than for *PostStandard_NoGuidance*.

Table 7, Columns (1) to (5), present findings from the liquidity specifications. Consistent with our predictions, Column (1) reveals that the *PostStandard* coefficient, −0.083, is significantly negative (t-statistic = −2.72), which indicates that, on average, firms experience an increase in stock price liquidity following implementation of an industry-specific standard. The findings in Columns (2) to (5) reveal significantly negative coefficients only on *PostStandard_NoAICPA, PostStandard_NoJudgment, PostStandard_Guidance,* and *PostStandard_Uniformity*; those on *PostStandard_AICPA PostStandard_Judgment, PostStandard_NoGuidance,* and *PostStandard_NoUniformity* are not statistically significant. Although the F-test for difference in coefficients is statistically significant in only one out of four specifications (i.e., the guidance specification), the results provide some (weak) evidence that liquidity benefits are

[22] Chen et al. (2018) not only caution against using residuals as dependent variables, but also mention that the use of residuals as partitioning variables (as we do in these tests) can be problematic (Chen et al., 2018, p. 756). We thus follow one of the suggestions by Chen et al. (2018) and include the same control variables as in the first stage regression.

[23] These inferences are unchanged when we test for coefficient differences using a bootstrapping procedure with clustering at the 3-digit SIC level.

[24] We focus on the Amihud (2002) illiquidity measure mainly because of data availability for the sample period. For example, bid-ask spreads are unavailable for events before 1985 and are not well populated in the database before 1992.

Table 6
Which firms benefit more from industry-standards?

Panel A: All industry standards

Dependent variable:	*CapitalGrowth*						*EquityIssuance*					
Subsamples:	Small Firm		Information Asymmetry		Capital Constraints		Small Firm		Information Asymmetry		Capital Constraints	
	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
PostStandard	0.057**	0.020	0.055***	−0.003	0.074***	0.020	0.037**	0.009	0.029***	0.001	0.040***	0.018*
	(2.14)	(1.62)	(3.01)	(-0.15)	(3.36)	(1.27)	(2.24)	(1.65)	(2.74)	(0.12)	(2.69)	(1.91)
t-stat from full interaction model [*p*-value]	1.57 [0.118]		2.81 [0.005]		2.69 [0.008]		1.82 [0.070]		1.73 [0.086]		1.47 [0.144]	
SalesGrowth	0.280***	0.425***	0.347***	0.302***	0.327***	0.296***	0.134***	0.121***	0.134***	0.133***	0.138***	0.127***
	(14.26)	(14.12)	(13.34)	(13.25)	(17.84)	(14.48)	(8.76)	(7.88)	(10.09)	(6.97)	(10.45)	(8.42)
Q	0.003	0.009**	0.019***	−0.002	0.010***	−0.000	0.003	0.049***	0.021***	0.011***	0.018***	0.005*
	(0.71)	(2.02)	(6.08)	(-0.59)	(3.31)	(-0.09)	(0.89)	(16.65)	(6.36)	(4.04)	(5.51)	(1.84)
Cash	−0.280***	−0.030	−0.109**	−0.322***	−0.165***	−0.264***	−0.369***	−0.149***	−0.165***	−0.444***	−0.298***	−0.324***
	(-4.25)	(-0.78)	(-2.09)	(-6.12)	(-2.61)	(-4.94)	(-6.82)	(-5.07)	(-3.26)	(-10.36)	(-5.42)	(-6.97)
Size	−0.328***	−0.214***	−0.234***	−0.359***	−0.302***	−0.314***	−0.235***	−0.124***	−0.137***	−0.266***	−0.189***	−0.227***
	(-23.94)	(-10.59)	(-15.62)	(-23.75)	(-15.31)	(-23.74)	(-14.34)	(-7.55)	(-10.24)	(-13.84)	(-9.55)	(-13.81)
Leverage	0.193***	0.051	0.241***	0.202***	0.121***	0.246***	0.118***	0.114***	0.160***	0.162***	0.052**	0.203***
	(6.80)	(1.23)	(6.74)	(8.85)	(5.33)	(10.43)	(4.14)	(3.86)	(6.09)	(6.45)	(2.56)	(7.27)
ROA	0.182***	0.459***	0.371***	0.190***	0.146***	0.283***	−0.302***	−0.251***	−0.370***	−0.264***	−0.323***	−0.261***
	(4.07)	(3.07)	(4.72)	(3.41)	(2.92)	(6.12)	(-13.81)	(-5.26)	(-12.46)	(-8.69)	(-15.56)	(-11.13)
Year-cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	125,409	96,068	62,980	113,128	65,183	156,294	122,339	93,134	61,613	109,856	63,637	151,836
R-squared	0.431	0.422	0.370	0.495	0.390	0.469	0.553	0.495	0.466	0.594	0.514	0.570

Panel B: *Uniformity* standards

Dependent variable:	*CapitalGrowth*						*EquityIssuance*					
Subsamples:	Small Firm		Information Asymmetry		Capital Constraints		Small Firm		Information Asymmetry		Capital Constraints	
	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
PostStandard	0.092***	0.024*	0.077***	−0.003	0.094***	0.028	0.061***	0.010*	0.041***	0.001	0.050***	0.022**
	(3.36)	(1.94)	(4.44)	(-0.17)	(3.73)	(1.64)	(3.49)	(1.92)	(4.08)	(0.07)	(3.03)	(2.28)
t-stat from full interaction model [*p*-value]	2.82 [0.005]		4.03 [0.000]		2.85 [0.005]		3.07 [0.002]		2.37 [0.019]		1.61 [0.109]	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	79,882	66,397	39,589	75,433	42,833	103,446	77,906	64,519	38,743	73,332	41,843	100,582
R-squared	0.421	0.415	0.367	0.485	0.374	0.462	0.550	0.406	0.480	0.586	0.512	0.562

Panel C: *NoUniformity* standards

(*continued on next page*)

Table 6 (*continued*)

Dependent variable:	*CapitalGrowth*						*EquityIssuance*					
Subsamples:	Small Firm		Information Asymmetry		Capital Constraints		Small Firm		Information Asymmetry		Capital Constraints	
	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
PostStandard	−0.043	−0.007	−0.015	0.021	0.022	−0.018	−0.054	0.009	−0.033	0.022	0.008	−0.006
	(-0.80)	(-0.22)	(-0.38)	(0.43)	(0.50)	(-0.49)	(-1.21)	(0.45)	(-0.89)	(0.55)	(0.20)	(-0.19)
t-stat from full interaction model [*p*-value]	−0.56 [0.573]		−0.69 [0.491]		0.96 [0.336]		−1.37 [0.172]		−1.08 [0.280]		0.40 [0.690]	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	45,527	29,671	23,391	37,695	22,350	52,848	44,433	28,615	22,870	36,524	21,794	51,254
R-squared	0.451	0.450	0.377	0.513	0.422	0.484	0.560	0.541	0.451	0.610	0.522	0.587

The table reports OLS coefficient estimates and, in parentheses, robust *t*-statistics clustered at the three-digit industry level. Panels A, B, and C show the results for all industry standards, *Uniformity* standards, and *NoUniformity* standards, respectively (see section 4 and online appendix D for the classification of standards). *SmallFirm* equals 1 (0) for all firms with above (below) median *Size* averaged over the two years before the standard becomes effective. *InformationAsymmetry* equals 1 (0) for all firms with above (below) median Amihud illiquidity score averaged over the two years before the standard becomes effective. *CapitalConstraints* equals 1 (0) for all firms we identify as capital constraint in one of the two years before the standard becomes effective. To identify capital constrained firms, we borrow the two-stage regression methodology from Biddle et al. (2009). In the first stage, we regress capital growth on the nonaccounting quality-based determinants of capital growth and year-cohort and firm-cohort fixed effects (i.e., model (1) of Table 3 without *PostStandard*) in the 5-year period before introduction of the standard ($t-5$ to $t-1$). In the second stage, we assign residuals from the first stage to quartiles, with those in the bottom quartile being defined as capital constrained. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent level (two-tailed).

Table 7
The role of liquidity and comparability.

Dependent variable:	*AmihudIlliquidity*					*DeFrancoComparability*				
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PostStandard	−0.083***					0.191**				
	(-2.72)					(2.42)				
PostStandard_AICPA		−0.026					−0.249*			
		(-0.34)					(-1.66)			
PostStandard_NoAICPA		−0.101***					0.308***			
		(-3.71)					(3.57)			
PostStandard_Judgment			−0.070					−0.423		
			(-0.84)					(-0.95)		
PostStandard_NoJudgment			−0.085***					0.224***		
			(-2.63)					(2.78)		
								[0.154]		
PostStandard_Guidance				−0.145***					0.304***	
				(-5.05)					(3.22)	
PostStandard_NoGuidance				−0.015					0.026	
				(-0.36)					(0.27)	
PostStandard_Uniformity					−0.086**					0.220***
					(-2.55)					(2.67)
PostStandard_NoUniformity					−0.069					−0.158
					(-1.13)					(-0.51)
F-test [p-value]		[0.345]	[0.871]	[0.006]	[0.809]		[0.001]	[0.154]	[0.026]	[0.243]
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	175,502	175,502	175,502	175,502	175,502	85,870	85,870	85,870	85,870	85,870
R-squared	0.975	0.975	0.975	0.975	0.975	0.778	0.778	0.778	0.778	0.778

The table reports OLS coefficient estimates and, in parentheses, robust *t*-statistics clustered at three-digit industry level. All regressions are based on a stacked DiD with 14 cohorts, using an event window that begins in year $t-5$ and ends in year $t+5$ around the effective year of the standard, $t=0$. We include year-by-cohort fixed effects as well as firm-by-cohort fixed effects. In columns (1) to (5), the dependent variable, *AmihudIlliquidity*, is the log of the yearly average of a firm's daily Amihud index (i.e., ratio of absolute stock return to dollar volume). In columns (6) to (10), the dependent variable, *DeFrancoComparability*, is based on the comparability measure of DeFranco, Kothari, and Verdi (2011), which we adjust to ensure that there is no overlap around the treatment events. In addition to the control variables of the baseline model (see Table 3, column 1), we control for *LogMarketCap* measured as the log of the market capitalization (i.e., fiscal year-end share price times number of outstanding shares), *LogTurnover* measured as the log of yearly average of the daily turnover (i.e., US$ trading volume divided by the market value at the end of each trading day), and *LogVolatility* measured as the log of the standard deviation of daily returns during a fiscal year. See Section 4 and online appendix D for classification of standards into different groups. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent level (two-tailed).

more pronounced for standards that are not merely AICPA codifications and standards that do not increase managerial judgment but instead provide more guidance and uniformity.

Table 7, Columns (6) to (10), present findings from the comparability specifications. Consistent with our predictions, Column (6) reveals that the *PostStandard* coefficient, 1.91, is significantly positive (*t*-statistic = 2.42), which indicates that, on average, firms experience an increase in comparability following implementation of an industry-specific standard. The findings in Columns (7) to ((10) reveal significantly positive coefficients only on *PostStandard_NoAICPA, PostStandard_NoJudgment, PostStandard_Guidance,* and *PostStandard_Uniformity*; those on *PostStandard_AICPA PostStandard_Judgment, PostStandard_NoGuidance* and *PostStandard_NoUniformity* are insignificant or even negative. Although the p-values from the F-tests for differences in coefficients are significant in only two out of four specifications, these findings provide some support that industry standards that reveal new information, provide explicit guidance, or increase uniformity generate positive comparability effects.

7. Conclusion

This study examines whether implementation of industry-specific accounting standards helps capital market participants in making decisions about providing capital to firms. Specifically, we predict and find that implementation of industry-specific standards between 1975 and 2011 leads to greater capital growth for firms in these industries. We also find that the increase in capital growth arises primarily from equity issuances. Findings from additional tests mitigate the possibility that our inferences are affected by industry-specific trends or other economic events unrelated to industry-specific standards.

We predict and find a more pronounced effect for industry standards that provide new information, include explicit guidance, or increase uniformity of accounting practices within an industry relative to other standards. In addition, we predict and find evidence of heterogeneity in the effects of standards on firms' capital flows depending on firm characteristics. In particular, we find that the increase in capital flows is concentrated among small firms, firms with greater information asymmetry, and firms with greater capital constraints before implementation of the standards. Finally, we test whether industry standards increase capital flows by reducing information asymmetry, which should manifest as an increase in stock price liquidity, or by increasing financial statement comparability, or both. Findings from these tests reveal that both channels are sources of increases in capital flows.

Our study is subject to limitations. First, our study faces the common tradeoff between credible identification and generalizability (see e.g., discussion of Glaeser and Guay, 2017). Accordingly, we note that our inferences might not generalize to other (broader) settings such as general accounting standards or cross-country accounting standards. According to theory (Corona et al., 2023; Dye and Sridhar, 2008), uniformity is a desirable feature of accounting standards if affected firms' transactions are homogeneous, which is plausibly more the case in our industry-specific setting, as compared to a cross-industry or cross-country setting. Relatedly, the average effect we observe likely masks differential effects of the standards arising from their substantial heterogeneity. Indeed, we find predictable differences in the effects of particular standards depending on the degree of uniformity, judgment, and guidance that industry standards provide. Second, although we perform various tests that collectively increase confidence that the documented increase in capital flows is attributable to the implementation of industry-specific standards, we cannot completely rule out potential confounding events. In particular, we note that the FASB's decision to introduce industry standards is not exogenous. Third, because of data restrictions and identification issues, we can neither trace the source of the increase in capital flows nor separate intra-industry from inter-industry capital flows. Consistent with predictions (and theory), we document an on average inflow of capital into affected industries, and this capital is then directed to firms that arguably benefit more from increased transparency and comparability (i.e., small firms, firms with greater information asymmetry, and capital constrained firms). This capital could come from other unaffected industries, other countries, and/or other asset classes (e.g., sale of government bonds). Yet, we note that (empirically) disentangling these different sources of capital supply is beyond the scope of this paper.

Setting aside these limitations, our study's findings provide support for the proposition that implementation of industry-specific accounting standards can help capital market participants in making decisions about providing capital to firms, in particular those that increase uniformity. This evidence is not only of interest to standard setters in shaping future accounting standards, but also enhances our understanding about whether, and if so, which type of industry standards can be helpful for capital allocation decisions.

Data availability

Data are available from the public sources cited in the text.

Acknowledgements

For helpful comments and suggestions, we thank Wayne Guay (editor), an anonymous referee, and Caspar David, Andrei Filip, Martin Jacob, Nico Lehmann, Maximilian Müller, Zoltán Novotny-Farkas, Patrick Vorst, Steve Zeff, seminar participants at the Academic Panel of the UK Financial Reporting Council, Maastricht University, Florida State University, Juan Carlos III University of Madrid, the University of Notre Dame, the University of Tennessee, ESSEC Business School, WHU Otto Beisheim School of Management, WU Vienna University, Erasmus University Rotterdam, Open University Netherlands, and BI Norwegian Business School. Annelies Renders acknowledges financial support from the NWO, Netherlands (Vidi Research Grant 016.Vidi.185.235).

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jacceco.2023.101670.

APPENDIX I. Industry-Specific Standards and their Implementation

Cohort	Standard	Title	Treated firms (industries)	Affected SIC codes	Initiation year	Effective year	AICPA codification	Judgment and choice	Explicit guidance	Uniformity
1	SFAS 009	Accounting for Income Taxes: Oil and Gas Producing Companies	Oil and gas firms	130-133; 137; 138; 290; 291; 299	1975	1976	no	no	yes	yes
2	SFAS 019	Financial Accounting and Reporting by Oil and Gas Producing Companies	Oil and gas firms	130-133; 137; 138; 290; 291; 299	1977	1979	no	no	no	yes
3	SFAS 026	Profit Recognition on Sales-Type Leases of Real Estate	Lessors of real estate	6517, 6519	1978	1980	no	no	no	no
4	SFAS 039	Financial Reporting and Changing Prices: Specialized Assets—Mining and Oil and Gas	Mining firms	100-103, 105-111, 140-149	1980	1981	no	yes	no	no
4	SFAS 044	Accounting for Intangible Assets of Motor Carriers	Motor Carriers	4210, 4213, 4214, 4231	1980	1981	no	no	no	no
4	SFAS 046	Financial Reporting and Changing Prices: Motion Picture Films	Motion picture firms	78	1981	1981	no	yes	no	no
5	SFAS 048	Revenue Recognition When Right of Return Exists	Retail firms	520-523; 525-527; 530-534; 539-546; 549-557; 559-573; 575-579; 590-599	1981	1982	yes	no	no	no
5	SFAS 050, 051, 053 and 063	Financial Reporting in the Record and Music Industry (50), Financial Reporting by Cable Television Companies (51), by Producers and Distributors of Motion Picture Films (53), by Broadcasters (63)	Licensors and licensees in the music and record industry; cable television companies; firms in motion picture production and distribution; and radio and television broadcasting stations	5735, 5736, 6794, 4841, 781, 782, 483	1981	1982	yes	no	yes	yes
5	SFAS 054	Financial Reporting and Changing Prices: Investment Companies	Firms categorized as ‘‘holding and other investment offices’’	67	1981	1982	no	yes	no	no
6	SFAS 066 and 067	Accounting for Sales of Real Estate (66), Accounting for Costs and Initial Rental Operations of Real Estate Projects (67)	Real estate industry	65	1981	1983	yes	yes	no	no
6	SFAS 068	Research and Development Arrangements	Firms in the chemicals and allied products industry as well as firms in the laboratory apparatus and furniture industry	28; 38	1982	1983	no	no	no	yes
7	SFAS 071	Accounting for the Effects of Certain Types of Regulation	Utilities industry	490-494	1979	1984	no	no	yes	yes

Cohort	Standard	Title	Treated firms (industries)	Affected SIC codes	Initiation year	Effective year	AICPA codification	Judgment and choice	Explicit guidance	Uniformity
8	SFAS 080	Accounting for Futures Contracts	Precious metals, airlines, and steel industry	3911; 5094; 4512; 3312–3325	1983	1985	no	yes	yes	no
9	SFAS 086	Accounting for the Costs of Computer Software to be Sold, Leased, or Otherwise Marketed	Business of selling prepackaged software	7372	1984	1986	no	yes	no	no
10	SFAS 090	Regulated Enterprises—Accounting for Abandonments and Disallowances of Plant Costs	Utilities industry	490-494	1985	1988	no	no	yes	yes
10	SFAS 092	Regulated Enterprises—Accounting for Phase-In Plans	Electric services industry	491	1985	1988	no	no	yes	yes
11	SFAS 139	Rescission of FASB Statement No. 53 and Amendments to FASB Statement Nos. 63, 89, and 121	Motion picture production and distribution	781; 782	1998	2001	yes	no	no	no
12	SFAS 143	Accounting for Asset Retirement Obligations	Mining and public utilities industries	100-103, 105-111, 140-149, 490-499	1994	2003	no	no	yes	yes
13	SFAS 152	Accounting for Real Estate Time-Sharing Transactions	Real estate industry	650-655; 659; 661	2003	2006	yes	no	no	yes
14	SFAS 167	Accounting for Transfers of Financial Assets (166), Amendments to FASB Interpretation No. 46(R) (167)	Construction, machinery, utilities, transportation, and retail industries	490-491; 630-633; 635-637; 639-641; 351-356; 358; 359; 150-154; 160-179; 520-523; 525-527; 530-534; 539-546; 549-557; 559-573;575-579; 590-599; 400; 401; 404; 410-415; 417; 419-421; 423; 424; 440-474; 478						

This table provides an overview of the industry-specific standards and their implementation. As a starting point, we use Table 2 of Khan et al. (2018) to identify industry-specific accounting standards and the affected industries. We then eliminate industry-specific standards that only affect the financial industry, and we retain only those industry standards that affect specific SIC codes or Fama-French 49 industries. This approach allows us to define treated industries (identified by SIC codes or Fama-French 49) and control industries (all other SIC codes or Fama-French 49); see Equation (1). We then collect the effective date for each industry-specific standard, allowing us to identify the fiscal year for which the industry-specific standard is effective. We obtain the initiation year of the standard (i.e., start date of the standard setting process) from Table 1 of Khan et al. (2018). Finally, we classify standards into the following categories: AICPA codification, Judgment and choice, Explicit guidance, and Uniformity (see section 4 and online appendix D for more details about the classification of industry-standards).

. (*continued*).

APPENDIX II. Variable Definitions

Variable	Description	Data source
Main dependent variables:		
CapitalGrowth	Log (invested capital$_t$ ÷ invested capital$_{t-1}$): invested capital is equity and long-term debt, Compustat item #37.	Compustat
EquityIssuance	Sum of the proceeds raised from the sale of common and preferred stock (Compustat item #108) ÷ total assets$_{t-1}$ Compustat item #6)	Compustat
Treatment variables:		
Standard$_k$	Indicator variable that equals one for all years of the event window that begins in $t-5$ and ends in $t+5$ around the effective year, $t=0$, except for the year before, $t-1$, for firms from industries that are subject to an industry-specific standard during the sample period, and zero otherwise.	Constructed
PostStandard	Indicator variable that equals one for firms from industries that are subject to an industry-specific standard after the implementation of the standards, and zero otherwise.	Constructed
Control variables:		

(*continued on next page*)

(*continued*)

Variable	Description	Data source
SalesGrowth	Log (sales$_t$ ÷ sales$_{t-1}$): sales is Compustat item #12.	Compustat
Q	([common shares*price close] + total liabilities)$_t$ ÷ total assets$_t$: common shares outstanding, price close, total liabilities, and total assets are Compustat items #25, #199, #181, and #6, respectively.	Compustat
Cash	Cash$_{t-1}$ ÷ total assets$_{t-1}$: cash and total assets are Compustat items #1 and #6, respectively.	Compustat
Size	Log (total assets)$_{t-1}$: total assets is Compustat item #6.	Compustat
Leverage	Total liabilities$_{t-1}$ ÷ total assets$_{t-1}$: total liabilities and total assets are Compustat items #181 and #6, respectively.	Compustat
ROA	Net income$_t$ ÷ total assets$_{t-1}$: net income and total assets are Compustat items #172 and #6, respectively.	Compustat
Alternative dependent variables:		
AllEquity	Log ([invested capital – long-term debt]$_t$ ÷ [invested capital – long-term debt]$_{t-1}$): invested capital and long-term debt are Compustat items #37 and #9, respectively.	Compustat
LTDebt	Log (long-term debt$_t$ ÷ long-term debt$_{t-1}$): long-term debt is Compustat item #9.	Compustat
CoreCapital	Log ([common stock + capital surplus]$_t$ ÷ [common stock + capital surplus]$_{t-1}$): common stock and capital surplus are Compustat items #85 and #210, respectively.	Compustat
DebtIssuance	Long-term debt issuance (Compustat item #111) − long-term debt reduction (Compustat item #114) ÷ total assets$_{t-1}$ (Compustat item #6)	Compustat
CurrentLiabilities (placebo)	Log (current liabilities$_t$ ÷ current liabilities$_{t-1}$): current liabilities is Compustat item #5.	Compustat
Standard partitioning variables:		
PostStandard_AICPA	Indicator variable that equals one for affected firms after implementation of industry-specific standards that are codification of AICPA pronouncements (i.e., SFAS 48, SFAS 50/51/53/63, SFAS 66/67, SFAS 139, and SFAS 152), and zero otherwise.	FASB
PostStandard_NoAICPA	Indicator variable that equals one for affected firms after implementation of other, non-AICPA industry-specific standards, and zero otherwise.	FASB
PostStandard_Judgment	Indicator variable that equals one for affected firms after implementation of industry-specific standards that increase managerial judgment and/or increase accounting choices (i.e., SFAS 39, SFAS 46, SFAS 54, SFAS 66/67, SFAS 80, and SFAS 86), and zero otherwise.	FASB
PostStandard_NoJudgment	Indicator variable that equals one for affected firms after implementation of other, non-judgment industry-specific standards, and zero otherwise.	FASB
PostStandard_Guidance	Indicator variable that equals one for affected firms after implementation of industry-specific standards that provide illustrative examples and additional disclosures (i.e., SFAS 9, SFAS 50/51/53/63, SFAS 71, SFAS 80, SFAS 90, SFAS 92, and SFAS 143), and zero otherwise.	FASB
PostStandard_NoGuidance	Indicator variable that equals one for affected firms after implementation of other, non-guidance industry-specific standards, and zero otherwise.	FASB
PostStandard_Uniformity	Indicator variable that equals one for affected firms after implementation of industry-specific standards that increase uniformity of accounting practices (i.e., SFAS 9, SFAS 19, SFAS 50/51/53/63, SFAS 68, SFAS 71, SFAS 90, SFAS 92, SFAS 143, SFAS 152, and SFAS 167), and zero otherwise.	FASB
PostStandard_NoUniformity	Indicator variable that equals one for affected firms after implementation of other, non-uniform industry-specific standards, and zero otherwise.	FASB
Other variables:		
IndustryCapitalTrend	The median *CapitalGrowth* of unaffected Canadian firms (i.e., firms listed at Toronto, Montreal, or Vancouver Stock Exchange) in the same 2-digit industry in a given year *t*.	Compustat
IndustryEquityTrend	The median *EquityIssuance* of unaffected Canadian firms (i.e., firms listed at Toronto, Montreal, or Vancouver Stock Exchange) in the same 2-digit industry in a given year *t*.	Compustat
AmihudIlliquidity	Log of the yearly average of a firm's daily Amihud index. The Amihud index uses daily CRSP data (ret, prc, and vol) and is calculated as the ratio of absolute stock return to dollar volume: [10,000,000 × |ret| ÷ (prc × vol)].	CRSP
DeFrancoComparability	Average yearly comparability of all firm-pairs within an industry using the 16 previous quarters to estimate comparability in the period before the standard is implemented and the 16 subsequent quarters to estimate comparability in the period after the standard is implemented.	Compustat
LogMarketCap	Log of the market capitalization (i.e., fiscal year-end share price times number of outstanding shares [prc × shr]).	CRSP
LogTurnover	Log of yearly mean of the daily turnover (i.e., dollar trading volume divided by the market value at the end of each trading day [(prc × vol) ÷ (prc × shr]).	CRSP
LogVolatility	Log of the standard deviation of daily returns during a fiscal year [sd(ret)].	CRSP
SmallFirm	Indicator variable that equals 1 (0) for all firms with above (below) median *Size* averaged over the two years before the standard becomes effective	Compustat
InformationAsymmetry	Indicator variable that equals 1 (0) for all firms with above (below) median Amihud illiquidity score averaged over the two years before the standard becomes effective.	CRSP
CapitalConstraints	Indicator variable that equals 1 (0) for all firms we identify as capital constrained in one of the two years before the standard becomes effective. To identify capital constrained firms, we borrow the two-stage regression methodology from Biddle et al. (2009). In the first stage, we regress capital growth on the nonaccounting quality-based determinants of capital growth and year-cohort and firm-cohort fixed effects (i.e., model (1) of Table 3 without *PostStandard*) in the 5-year period before implementation of the standard ($t-5$ to $t-1$). In the second stage, we assign residuals from the first stage to quartiles, with those in the bottom quartile being defined as capital constrained.	Compustat

All continuous variables are winsorized at the 1st and 99th percentiles.

References

Amihud, Y., 2002. Illiquidity and stock returns: cross-section and time-series effects. J. Financ. Mark. 5 (1), 31–56.
Angrist, J.D., Pischke, J.-S., 2009. Mostly Harmless Econometrics: an Empiricist's Companion. Princeton University Press, Princeton, NJ.
Armstrong, C., Kepler, D., Samuels, D., Taylor, D., 2022. The evolution of empirical methods in accounting research and the growth of quasi-experiments. J. Account. Econ. 74 (2–3), 101521.
Badertscher, B., Shroff, N., White, H.D., 2013. Externalities of public firm presence: evidence from private firms' investment decisions. J. Financ. Econ. 109 (3), 682–706.
Baker, A.C., Larcker, D.F., Wang, C.C., 2022. How much should we trust staggered difference-in-differences estimates? J. Financ. Econ. 144 (2), 370–395.
Balakrishnan, K., Billings, M.B., Kelly, B., Ljungqvist, A., 2014. Shaping liquidity: on the causal effects of voluntary disclosure. J. Finance 69 (5), 2237–2278.
Barrios, J.M., 2021. Staggeringly Problematic: A Primer on Staggered DiD for Accounting Researchers. Available at: SSRN 3794859.
Beatty, A., Liao, S., Weber, J., 2009. The effect of private information and monitoring on the role of accounting quality in investment decisions. Contemp. Account. Res. 27 (1), 17–47.
Bertrand, M., Mullainathan, S., 2003. Enjoying the quiet life? Corporate governance and managerial preferences. J. Polit. Econ. 111 (5), 1043–1075.
Bharath, S.T., Sunder, J., Sunder, S.V., 2008. Accounting quality and debt contracting. Account. Rev. 83 (1), 1–28.
Biddle, G.C., Hilary, G., 2006. Accounting quality and firm-level capital investment. Account. Rev. 81 (5), 963–982.
Biddle, G.C., Hilary, G., Verdi, R.S., 2009. How does financial reporting quality relate to investment efficiency? J. Account. Econ. 48 (2–3), 112–131.
Bird, A., Ertan, A., Karolyi, S.A., Ruchti, T.G., 2020. Information, Financing, and Investment. SSRN Working Paper.
Blankespoor, E., deHaan, E., Marinovic, I., 2020. Disclosure processing costs, investors' information choice, and equity market outcomes: a review. J. Account. Econ. 70 (2–3), 101344.
Cengiz, D., Dube, A., Lindner, A., Zipperer, B., 2019. The effect of minimum wages on low-wage jobs. Q. J. Econ. 134, 1405–1454.
Chen, F., Hope, O.K., Li, Q., Wang, X., 2011. Financial reporting quality and investment efficiency of private firms in emerging markets. Account. Rev. 86 (4), 1255–1288.
Chen, W., Hribar, P., Melessa, S., 2018. Incorrect inferences when using residuals as dependent variables. J. Account. Res. 56 (3), 751–796.
Cho, Y.J., 2015. Segment disclosure transparency and internal capital market efficiency: evidence from SFAS No. 131. J. Account. Res. 53 (4), 669–723.
Christensen, H.B., Hail, L., Leuz, C., 2016. Capital-market effects of securities regulation: prior conditions, implementation, and enforcement. Rev. Financ. Stud. 29 (11), 2885–2924.
Cohn, M., 2018. Companies still find it difficult to comply with revenue recognition changes. Account. Today.
Collins, D.W., Dent, W.T., 1979. The proposed elimination of full cost accounting in the extractive petroleum industry: an empirical assessment of the market consequences. J. Account. Econ. 1, 3–44.
Conley, T., Goncalves, S., Hansen, C., 2018. Inference with dependent data in accounting and finance applications. J. Account. Res. 56 (4), 1139–1203.
Corona, C., Huang, Z., Hwang, H., 2023. Accounting uniformity, comparability, and resource allocation efficiency. Account. Rev. (forthcoming).
De Franco, G., Kothari, S.P., Verdi, R.S., 2011. The benefits of financial statement comparability. J. Account. Res. 49 (4), 895–931.
Diamond, D., Verrecchia, R., 1991. Disclosure, liquidity and the cost of capital. J. Finance 46, 1325–1359.
Dyckman, T.R., Smith, A.J., 1979. Financial accounting and reporting by oil and gas producing companies. J. Account. Econ. 1, 45–75.
Dye, R., Sridhar, S., 2008. A positive theory of flexibility in accounting standard. J. Account. Econ. 46, 312–333.
Fiechter, P., Novotny-Farkas, Z., Renders, A., 2022. Are level 3 fair value remeasurements useful? Evidence from ASC 820 rollforward disclosures. Account. Rev. 97, 301–323.
Financial Accounting Standards Board (FASB), 2018. Conceptual Framework for Financial Reporting, Statements of Financial Accounting Concepts No. 8, FASB. FASB, Norwalk, CT.
Giroud, X., 2013. Proximity and investment: evidence from plant-level data. Q. J. Econ. 128 (2), 861–915.
Glaeser, S., Guay, W.R., 2017. Identification and generalizability in accounting research: a discussion of Christensen, Floyd, Liu, and Maffett (2017). J. Account. Econ. 64 (2–3), 305–312.
Goodman-Bacon, A., 2021. Difference-in-differences with variation in treatment timing. J. Econom. 225, 254–277.
Gormley, T.A., Matsa, D.A., 2011. Growing out of trouble? Corporate responses to liability risk. Rev. Financ. Stud. 24, 2781–2821.
Healy, P.M., Palepu, K.G., 2001. Information asymmetry, corporate disclosure, and the capital markets: a review of the empirical disclosure literature. J. Account. Econ. 31 (1–3), 405–440.
Henderson, B.J., Jegadeesh, N., Weisbach, M.S., 2006. World markets for raising new capital. J. Financ. Econ. 82 (1), 63–101.
Jiang, J., Wang, I., Wangerin, D., 2018. How does the FASB make decisions? A descriptive study of agenda-setting and the role of individual board members. Account. Org. Soc. 71, 1–17.
Khan, U., Li, B., Rajgopal, S., Venkatachalam, M., 2018. Do the FASB's standards add shareholder value? Account. Rev. 93 (2), 209–247.
Lambert, R., Leuz, C., Verrecchia, R.E., 2007. Accounting information, disclosure, and the cost of capital. J. Account. Res. 45 (2), 385–420.
Leone, A.J., Minutti-Meza, M., Wasley, C.E., 2019. Influential observations and inference in accounting research. Account. Rev. 94 (6), 337–364.
Leuz, C., Wysocki, P., 2016. The economics of disclosure and financial reporting regulation: evidence and suggestions for future research. J. Account. Res. 54, 525–622.
Martin, L.A., Subramaniam, C., Vigeland, R.L., 2000. The effects of SFAS no. 90 on nuclear electric utilities. Account. Horiz. 14 (2), 191–209.
Myers, S.C., Majluf, N.S., 1984. Corporate financing and investment decisions when firms have information that investors do not have. J. Financ. Econ. 13 (2), 187–221.
Plumlee, M., Xie, Y., Yan, M., 2015. Bank loan spread and private information: pending approval patents. Rev. Account. Stud. 20 (4), 593–638.
Ritter, J.R., Welch, I., 2002. A review of IPO activity, pricing, and allocations. J. Finance 57 (4), 1795–1828.
Roychowdhury, S., Shroff, N., Verdi, R.S., 2019. The effects of financial reporting and disclosure on corporate investment: a review. J. Account. Econ. 68 (1), 1–27.
Schipper, K., 2003. Principles-based accounting standards. Account. Horiz. 17 (1), 61–72.
Schipper, K., Thompson, R., Weil, R.L., 1987. Disentangling interrelated effects of regulatory changes on shareholder wealth: the case of motor carrier deregulation. J. Law Econ. 30 (1), 67–100.
Shroff, N., 2017. Corporate investment and changes in GAAP. Rev. Account. Stud. 22 (1), 1–63.
Shroff, N., 2020. Real effects of PCAOB international inspections. Account. Rev. 95 (5), 399–433.
Watts, R., Zimmermann, J., 1990. Positive accounting theory: a ten-year perspective. Account. Rev. 65, 131–156.
Whited, R.L., Swanquist, Q.T., Shipman, J.E., Moon, J.R., 2022. Out of control: the (over) use of controls in accounting research. Account. Rev. 97 (3), 395–413.
Zeff, S., 2007. Some obstacles to global financial reporting comparability and convergence at a high level of quality. Br. Account. Rev. 39 (4), 290–302.