THE LIQUIDITY, PRECISION, AND COMPARABILITY EFFECTS OF ASC 606: REVENUE FROM CONTRACTS WITH CUSTOMERS

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ABSTRACT

Petrus H. Ferreira: The Liquidity, Precision, and Comparability Effects of ASC 606: Revenue from Contracts with Customers
(Under the direction of Wayne Landsman)

I examine the liquidity effect of the implementation of ASC 606: Revenue from contracts with customers. I find that the implementation of ASC 606 increases liquidity. Next, I examine the channels through which the implementation of the standard affects liquidity. Theory suggests that the implementation of standards can affect liquidity through either the precision channel, i.e., the change in the accounting report's ability to reflect economic events, the comparability channel, i.e., the increase in comparability across reporting entities, or both. I find that the implementation of the new revenue standard is associated with increases in both precision and comparability, which are, in turn, associated with an increase in liquidity. I further show that firms that experience an increase in neither precision nor comparability do not experience an increase in liquidity.

Keywords: Revenue recognition standard, liquidity, comparability, ASC 606, ASU 2014-09, FASB

JEL Classification: D82, G00, G14, G18, M41

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LIST OF ABBREVIATIONS

ASC	Accounting Standard Codification
FASB	Financial Accounting Standards Board
FIC	Fixed Industry Classification
GAAP	Generally Accepted Accounting Principles
IFRS	International Financial Reporting Standards
SAB	Staff Accounting Bulletin

CHAPTER 1. INTRODUCTION

The objective of this study is to assess the capital market effects of the implementation of the new revenue standard, Accounting Standard Codification (ASC) 606, *Revenue from contracts with customers*.[1] More specifically, I examine whether implementation of the standard affects liquidity and the channels through which it occurs. Because the former revenue standard was used in conjunction with numerous pieces of authoritative industry- and transaction-specific guidance, firms often accounted differently for economically similar transactions, thereby reducing financial statement comparability among firms (Financial Accounting Standards Board (FASB) 2014). To improve comparability among firms, the FASB issued the new revenue recognition standard that requires all firms to follow the same five-step recognition process, regardless of industry or transaction type.

I examine the capital market effects of the new revenue standard for at least three reasons. First, revenue is an important measure of firm health and performance that is scrutinized by investors and regulators. Second, the new revenue standard had a large effect on financial reports of many firms and the potential capital market effects of the standard are, therefore, widespread. Third, because the cost of implementing the new revenue standard was substantial,

[1] ASC 606 is the codification of *Revenue from contracts with customers*, Accounting Standard Update (ASU) 2014-09. I refer to the ASU and its codification as the "new revenue standard" throughout the paper.

estimated to be $3.3 million on average for public and private companies, it is important to address whether investors benefited from the new revenue standard.[2]

Evidence suggests that liquidity is a key construct to assess the capital market effects (Bhattacharya et al. 2012; Lang et al. 2012; Holden et al. 2014). Furthermore, theory suggests that the implementation of standards can increase liquidity and that the increase in liquidity is associated with an increase in firm value and investment efficiency. Because of these reasons, I examine the liquidity effect of the new revenue standard, even if this is not an explicit objective of standard setter.

The new revenue standard is effective for firms with fiscal year-ends beginning after December 15, 2017, including interim periods. Because firms implemented the standard at different times, I use a staggered difference-in-differences design to examine the liquidity effect of the implementation of the standard. Using several proxies for liquidity, I find that the implementation of the new revenue standard increases liquidity. I also show that firms that implement the standard first experience an increase in liquidity relative to firms that implement the standard at later dates and that this liquidity difference decreases as more firms implement the standard. This liquidity difference is insignificantly different from zero once all firms implement the standard. I also perform additional analyses to increase confidence in the inference that the implementation of the standard increases liquidity.

Analytical work suggests that the implementation of accounting standards affects liquidity through two channels (Barth et al. 1999; Gao et al. 2019). The first is a change in precision, where precision is the accounting report's ability to reflect economic events. The

[2] Estimates of cost available at https://www.wsj.com/articles/revenue-recognition-compliance-costs-are-higher-than-expected-companies-say-11549406561?mod=djemCFO_h

second is a change in comparability, where an increase in comparability results in financial reports reflecting the same economic events more similarly to enable users to identify and understand similarities in, and differences among, recognized amounts. Because an increase (decrease) in precision or comparability can lead to an increase (decrease) in liquidity, the net effect of these two channels on liquidity is an empirical matter that depends on the relative effects of changes in precision and comparability. How these channels affect liquidity is potentially relevant to standard setters because creating a standard that increases comparability (precision), but decreases precision (comparability) could have undesirable capital market effects by decreasing liquidity.

First, I find that both precision and comparability increase after the implementation of the new revenue standard. I also show that both of these characteristics are positively correlated with liquidity. I also find no increase in either characteristic in the year prior to the implementation year and perform additional analyses, such as assessing the construct validity of the proxy for comparability and employing alternative estimation procedures when calculating the proxy for precision. Second, I show that firms that experience an increase in precision or comparability, or both, experience an increase in liquidity, but firms that experience an increase in neither do not. Third, using path analysis, I show that both the precision and comparability channels contribute to the increase in liquidity associated with implementation of the standard.

I contribute to the literature in various ways. First, I provide early evidence on the capital market effects of the implementation of the new revenue standard. Specifically, I show that liquidity increases when firms implement the standard. Second, I provide evidence that the precision and comparability channels are the channels through which the implementation of the standard increases liquidity. I also show that increases in precision or comparability are associated with increases in liquidity and that firms that experience an increase in neither precision nor comparability do not experience an increase in liquidity. Furthermore, to my knowledge, this is the first study to investigate empirically the interaction of the precision and comparability channels, and provide empirical evidence consistent with theory. Finally, I show that the proxies I use for precision and comparability are robust and can, therefore, be used in future research addressing standard-setting questions.

These findings should be of interest to standard setters as they show, consistent with theory, that new standards can not only increase comparability, but also change the precision with which accounting reports reflect economic events. In addition, the fact that both these constructs have liquidity effects provides insights potentially relevant to standard setters concerned with capital market effects.

The remainder of this paper is organized as follows. Section II discusses the institutional background, related research, and hypothesis development. Section III develops the research design and describes the empirical measures used for theoretical constructs. Section IV describes the sample, Section V reports the results, and Section VI concludes the study.

CHAPTER 2. INSTITUTIONAL BACKGROUND, RELATED RESEARCH, AND HYPOTHESIS DEVELOPMENT

Institutional background: Revenue standard (ASC 606)

The FASB issued ASC 606, *Revenue from contracts with customers*, in May 2014. The new revenue standard was the result of a 13-year joint project between the FASB and the International Accounting Standards Board. One objective of the project was to create a revenue standard that would improve the comparability of revenue between firms that apply International Financial Reporting Standards (IFRS) and US Generally Accepted Accounting Principles (GAAP). The new standard likely also improved comparability among firms that apply US GAAP. The FASB's former revenue standard, ASC 605, *Revenue recognition*, was used in conjunction with numerous pieces of authoritative industry- and transaction-specific guidance that often resulted in different accounting for economically similar transactions, reducing comparability (Financial Accounting Standards Board 2014).

To improve comparability, the FASB's new revenue standard standardizes how firms recognize revenue transactions by requiring all firms to follow a five-step recognition process, regardless of the industry or the transaction type, where comparability is "*the qualitative characteristic that enables users to identify and understand similarities in, and difference among, items"* (Financial Accounting Standards Board 2010). The new revenue standard also requires improved disclosures to help users understand the nature, amount, timing, and uncertainty of revenues and cash flow. These include quantitative and qualitative disclosures about contracts with customers, significant judgments and changes in judgments, and the cost of assets recognized to obtain or fulfill a contract.

Related research and hypothesis development

My study relates to accounting literature on the liquidity effect of the adoption of accounting standards. Whereas numerous studies investigate the liquidity effect of the adoption of IFRS, few investigate the liquidity effect of the change in accounting standards or the implementation of a single standard.[3] One example of the latter is Mohd (2005), which finds that implementing SFAS 86, *Accounting for costs of computer software to be sold, leased, or otherwise marketed*, decreases information asymmetry for software firms relative to that of other high-tech firms.

Fiechter, Landsman, Peasnell, and Renders (2019), which examines the economic consequences of the implementation of multiple industry-specific standards, finds that the implementation of industry-specific standards is associated with an increase in stock liquidity and greater capital flows to firms affected by the implementation of the standard. A contemporaneous study by Chung and Chuwonganant (2019) examines whether the implementation of the new revenue standard changed the effect that earnings announcements have on market quality and trading activities.[4] My study is one of the first to investigate the economic consequences of the implementation of the new revenue standard and the first, to my

[3] See De George, Li, and Shivakumar (2016) for a review of IFRS adoption studies.

[4] A key difference between that study and mine is that Chung and Chuwonganant (2019) addresses the question of whether the *change* in market quality and trading activities around earnings announcements changes after the implementation, whereas I address the question of whether the level of liquidity changes after the implementation. Furthermore, Chung and Chuwonganant (2019) examines the effect only at earnings announcements. Finally, in addition to addressing the question of whether liquidity increases after the implementation of the standard, I also examine the various theoretically motivated channels through which the standard affects liquidity.

knowledge, to investigate the channels through which the implementation of a standard affects liquidity.

Two analytical studies, Barth et al. (1999) and Gao et al. (2019), model the capital market effects of the adoption of accounting standards. Even though the model used in these studies relates to the adoption of a set of standards, for example, US GAAP and IFRS, Gao et al. (2019) notes that the model can be applied to the implementation of specific accounting standards (methods). The model shows that an increase (decrease) in either precision or comparability could lead to an increase (decrease) in price informativeness, and therefore liquidity. The net effect of the implementation of accounting standards on liquidity is, therefore, a function of the change in both precision and comparability.

The model shows that an increase (decrease) in precision improves (impairs) price informativeness and mitigates (increases) information asymmetry, which in turn leads to an increase (decrease) in liquidity. Precision is the accounting report's ability to reflect the firm's economic events or terminal cash flow.[5] In the context of my study, the accounting report is a firm's revenue and precision is the ability of revenue to reflect economic events during the period. It is *ex-ante* unclear whether precision will increase or decrease. On the one hand, the new five-step recognition process and the increase in disclosure requirements associated with the implementation of the new revenue standard could increase precision. On the other hand, prior revenue guidance might have reflected economic events more precisely than the new five-step recognition process, and the attempted increase in comparability might come at the cost of precision. Whether the new revenue standard increases or decreases precision is, therefore, an

[5] I assume that economic events will ultimately lead to terminal cash flow and use the term economic events throughout the study.

empirical question, and I have no directional prediction regarding the precision effect of the implementation of the standard.

The model also shows that harmonization could decrease the average information processing cost per firm to the investor, which would lead to an increase in liquidity.[6] The authors model an equilibrium in which each investor decides to learn, at a cost, about the accounting standards applied by a firm. An investor's decision to invest in a specific firm is subject to an information processing cost constraint, where information processing cost is defined as the cost to learn how each individual firm or industry recognizes revenue.

Under former revenue guidance, an investor would need to incur an information processing cost to learn how each individual firm or industry recognizes revenue to be able to compare, i.e., identify similarities in, and differences among, firms' recognized revenue amounts. If the new revenue standard increases comparability, an investor would be able to apply knowledge of the new revenue standard to larger groups of firms or industries without incurring additional information processing cost. Because the investor does not need to incur additional information cost to compare more firms' recognized revenue amount, the average information processing cost per firm for the investor will decrease. An increase in comparability would there lead to a decrease in the average information processing cost per firm for the investor.

The model shows that information processing cost is negatively associated with the number of informed investors, and because an increase in comparability lowers information processing cost, the number of informed investors will increase if comparability increases.[7] In

[6] Gao et al. (2019) refers to the adoption of common standards, whereas Barth et al (1999) refers to harmonization. Furthermore, Gao et. (2019) refers to the decrease in information processing cost effect as the network effect, whereas Barth et al (1999) refers to it as the expertise acquisition effect.

[7] An informed investor is an investor who chooses to learn about the revenue recognition of a firm.

other words, an increase in comparability will lead to an increase in the number of informed investors and I, therefore, use the number of informed investors as a proxy for comparability.[8,9] As the number of informed investors increases, stock price becomes more informative, and greater stock price informativeness leads to higher liquidity. Given that one objective of the new revenue standard was to increase comparability, I predict that the new revenue standard will increase comparability.

Therefore, because the change in precision is ambiguous, the net effect of the implementation of the new revenue standard on liquidity is also ambiguous. The objectives of this study are to determine what the net liquidity effect of the implementation of the standard is and through which channels the implementation of the standard affects liquidity.

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[8] One caveat is that the informed investor will consider the change in precision because of the implementation of the new revenue standard when deciding whether to learn about the accounting standards applied by the firm. This is because an increase (decrease) in precision will decrease (increase) the profit for the informed investor and therefore decrease (increase) the number of informed investors in equilibrium. To address this matter I include a control for precision's effect on the number of informed investors in additional analyses.

[9] I also develop and use an alternative measure for comparability using the economic intuition as discussed in De Franco et al. (2011).

CHAPTER 3. RESEARCH DESIGN

Liquidity analysis

I use a staggered difference-in-differences design to examine the liquidity effect of the implementation of the new revenue standard. The standard is effective for fiscal year-ends beginning after December 15, 2017, including interim periods therein. The March 2018 quarterly reports of December fiscal year-end firms are the first reports that contain the effects of the standard.[10] January to November fiscal year-end firms implemented the standard for the first time in their April 2018 to February 2019 quarterly reports. Therefore, firms implement the standard at different dates depending on their fiscal year-ends, which provides the basis for exogenous variation in the implementation date.[11]

For example, firms with a December fiscal year-end will implement the standard for the first time in the March 2018 quarterly report. Firms with a March (June and September) fiscal year-end will release a March 2018 annual (quarterly) report, but the report will not yet include the effects of the new revenue standard.[12] The non-December fiscal year-end firms, therefore, serve as a control group. Similarly, the June 2018 quarterly reports of December and March fiscal year-end firms will include the effects of the new revenue standard, whereas the June

[10] Although firms could early adopt the new revenue standard, less than 1% of firms elected to do so (Peters 2018).

[11] I delete 27 firms that change their reporting periods during the sample period.

[12] One possible concern is that part of the control group includes both annual and quarterly reports. Another concern is that the standard affects firms with different fiscal periods in a different manner. I address these concern in robustness tests.

annual (quarterly) report of June (September) fiscal year-end firms will not. The staggered implementation allows me to compare the capital market effects of the new revenue standard, using the June and September fiscal year-end firms as a control group. One advantage of using a staggered difference-in-differences design is that any unobserved factor that might explain the outcome needs to coincide with each of the different implementation dates of the new revenue standard (Kraft et al. 2018).

I estimate Equation (1):

$$Liquidity\_variable_{it} = \beta_1 Treat \times Post_{it} + Controls_{it} + \mu_i + \gamma_t + \varepsilon_{it}, \quad (1)$$

where the *i* and *t* subscript refer to firm and month-year, and μ_i and γ_t are firm and month-year fixed effects.[13] I include firm fixed effects to control for time-invariant aspects of the firm and year-month fixed effects to control for common macroeconomic events. *Liquidity_variable* is one of three measures. The first is the natural log of the quarterly median of the daily Amihud (2002) measure of illiquidity, calculated using the unsigned stock return divided by USD trading volume (*Amihud*).[14] The second is the natural log of the quarterly median of daily quoted spreads, calculated using the daily closing bid and ask prices divided by the midpoint (*Bid_ask*) (Daske, Hail, Leuz, & Verdi 2008; Christensen et al. 2013; Glaeser 2018).

Because *Amihud* and *Bid_ask* are illiquidity measures, I take the negative of *Amihud* and *Bid_ask* to ease the interpretation of the results. Both of these measures are calculated using the daily data in the quarter following the implementation of the new revenue standard. For example,

[13] I include month-year fixed effects because quarterly reports are released every month, depending on the fiscal year-end of the firm. *Treat* and *Post* are omitted in the equation because *Treat* and *Post* are subsumed by the firm and month-year fixed effects.

[14] Similar to Christensen et al. (2013), I omit zero-return days from the computation of the quarterly median.

if a firm has a December fiscal year-end, the March 2018 quarterly report is the first report that includes the effects of the new revenue standard and the *Liquidity_variable* is calculated using daily data in the quarter following March 2018, i.e., the quarter ending June 2018. I use factor analysis to identify a common factor, *Liquidity*, that explains common variation between *Amihud* and *Bid_ask* (Daske et al. 2008; Christensen et al. 2013). *Treat*×*Post*, the variable of interest, equals one if the quarterly or annual report has been prepared using the new revenue standard, zero otherwise.

Controls includes a variety of firm-level variables identified by prior research as being associated with liquidity: the natural log of the standard deviation of returns ($SD\_returns_{t-4}$), the natural log of the market value of equity ($Size_{t-4}$), and the natural log of the quarterly median of daily turnover, calculated as volume of shares traded divided by number of shares outstanding ($Turnover_{t-4}$). I lag all variables by one year (Christensen et al. 2013). I also include controls that are standard in the disclosure literature (Guay et al. 2016; Glaeser 2018): income before extraordinary items scaled by assets (*ROA*), market value of equity to book value of equity (*MB*), book value of total debt to book value of total assets (*Leverage*), buy and hold returns over the quarter (*Q_return*), an indicator equal to one if the firm is a loss firm (*Loss*), special items scaled by total assets (*SI*), and number of analysts providing revenue forecasts (*Analyst*).[15]

The coefficient on the variable of interest, *Treat*×*Post*, reflects the liquidity effect of the implementation of the new revenue standard. I interpret a positive (negative) coefficient as the implementation of the new revenue standard increasing (decreasing) the liquidity of the average firm.

[15] Similar to *Amihud* and *Bid_ask*, *SD_returns*, *Turnover*, and *Q_return* are calculated in the three months following the fiscal quarter.

Channels through which the implementation of the standard can affect liquidity

In this section, I explore the channels through which the implementation of the new revenue standard could affect liquidity and I estimate the following equations to do so:

$$Precision_{it} = \lambda_1 Treat \times Post_{it} + \mu_i + \gamma_t + \varepsilon_{it} \quad (2)$$

$$Informed_{it} = \theta_1 Treat \times Post_{it} + \mu_i + \gamma_t + \varepsilon_{it}, \quad (3)$$

where *Precision* (*Informed*) is a proxy for precision (the number of informed investors) and is defined below. As explained in the previous section, the correlation between the implementation of the standard, *Treat*×*Post*, and precision, *Precision*, is ambiguous and a positive (negative) λ_1 coefficient is evidence of an increase (decrease) in precision. I predict a positive correlation between the implementation of common accounting standards, *Treat*×*Post*, and the number of informed investors, *Informed*, and I, therefore, predict a positive θ_1 coefficient.

I require proxies for precision and informed investors to estimate Equations (2) and (3). Prior analytical work models precision as the accounting report's ability to reflect the firm's fundamentals. More specifically, precision is modeled as the reciprocal of the standard deviation of the measurement error that is obtained when the economic events are mapped to the accounting report (Barth et al. 1999; Gao et al. 2019). Thus, precision is the reciprocal of σ_{ε_i}, where ε_i is the difference between r_i and $\alpha_i \times v_i$, from $r_i = \alpha_i \times v_i + \varepsilon_i$, r_i is the accounting report, α_i is the mapping of v_i to r_i, and v_i is the economic events during the period. In my setting, the accounting report is revenue. The coefficient on v_i, α_i, can be interpreted as the reciprocal of the revenue multiple in a valuation process. Precision is inherently difficult to measure because the economic events are unobservable to the researcher. Nevertheless, I calculate two proxies for precision and use factor analysis to identify a common factor that explains common variation between these proxies.

I use an empirical implementation of prior analytical and empirical work. Prior literature defines the accounting system as a mapping of economic events to the accounting report (De Franco et al. 2011; Yip and Young 2012) and uses returns as a proxy for economic events. Because the new revenue standard will affect *Revenue*, I use *Revenue* as a proxy for the accounting report. I, therefore, estimate the following equation:

$$Revenue_{it} = \beta_1 Returns_{it} + \mu_i + \gamma_t + \varepsilon_{it}, \tag{4}$$

where *Revenue* is revenue for the quarter and *Returns* is the quarterly return for the same quarter.[16] I estimate Equation (4) for the pre- and post-implementation periods separately to allow the β_1 coefficient to differ in the two periods because the accounting standard changed from the pre- to post-implementation period. β_1 reflects the mapping of economic events to the accounting report during the period and the residual from Equation (4), ε_{it}, reflects the measurement error in the mapping of economic events to the accounting report during the period. I use the residual from Equation (4) to calculate the standard deviation of the residual in the pre- and post-implementation periods. The natural log of the reciprocal of the standard deviation is the first proxy for precision, i.e., $Precision\_residual_{i\omega} = ln\left(\frac{1}{\sigma_{\varepsilon_{i\omega}}}\right)$, where ω represents either the pre- or post-implementation period.[17]

As discussed in the previous section, the new revenue standard also increases the amount of disclosure required. Equation (4) will not reflect the increase in disclosure and a possible

[16] I delete 207 observations where the absolute quarterly return is greater than 100%.

[17] Note that the standard deviation of the residual is divided by the mean of *Revenue*, i.e., I use the coefficient of variation. Not doing so would mechanically assign higher precision to firms with lower revenue.

increase in disclosure quality. If disclosure quality increases, the information risk will decrease, and a decrease in information risk will decrease the required rate of return. I use the standard deviation of daily returns over the quarter, *SD_returns*, as a proxy for the required rate of return and calculate the second proxy for precision as the natural log of the reciprocal of the standard deviation of daily returns over the quarter, i.e., $Precision\_returns_{it} = ln\left(\frac{1}{SD\_returns_{it}}\right)$. Finally, I use factor analysis to identify a common factor that explains common variation between the two precision proxies, *Precision*.

I use the natural log of the number of institutional investors, *Informed*, as a proxy for the number of informed investors. As discussed in the previous section, an increase in comparability will decrease the average information processing cost, which will increase the number of informed investors. The increase in the number of informed investors will, in turn, lead to an increase in liquidity because as the number of informed investors increases, the stock price becomes more informative and the greater stock price informativeness leads to higher liquidity.

Therefore, because an increase in comparability will lead to an increase in the number of informed investors, the number of informed investors is a proxy for comparability. I could investigate comparability using proxies for information processing cost, the number of informed investors, or stock price informativeness or by constructing a comparability measure. I choose to use a proxy for the number of informed investors for three reasons. First, using a proxy for the number of informed investors does not involve any calculations or estimations that might be subject to measurement error. Second, the analytical model provides the opportunity to test the construct validity of my proxy for the number of informed investors (see section V). Third, comparability is an unobservable characteristic that is difficult to measure, and I, therefore, choose to examine a consequence of an increase in comparability, i.e., an increase in the number of informed investors. Nevertheless, I develop and use an alternative measure for comparability in additional analyses.[18]

[18] I do not calculate comparability using the De Franco et al. (2011) the measure as this measure requires 16 quarters of data per firm.

CHAPTER 4. SAMPLE AND DATA

I begin by obtaining all firm observations from Compustat with fiscal quarters ending between March 1, 2017 and February 28, 2019 and the sample, therefore, includes one year of data before and after the implementation for December fiscal year-end firms. I end the sample period on February 28, 2019 to exclude the possible effects of the implementation of the new leasing standard (ASC 842) that is effective for annual periods, including interim periods therein, beginning after December 15, 2018, which is one year after the effective date of the new revenue standard. I obtain financial statement data from Compustat, market data from CRSP, and institutional holdings data from Thompson Reuters. I delete all observations where data are missing for the estimation of Equation (1) and require firms to have quarterly sales greater or equal to zero.[19] I delete all firms that changed their fiscal year-end during the sample period and require at least 20 days of daily CRSP data per quarter to calculate *Amihud* and *Bid_ask*. The final sample consists of 24,675 firm-quarter observations and 3,475 firms with an average of 7.1 quarters per firm over the two year sample period.

Table 1 reports the descriptive statistics, and Table 2 reports the Pearson correlation coefficients between variables used in Equation (1).[20] Because Table 1 shows that both liquidity measures are right-skewed, I use the natural log of the measures when estimating all equations

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[19] I assign an analyst following of zero to firms to firms missing analyst data. Inferences from estimating Equation (1) remain unchanged if I delete observations with missing analyst data. I include only common stock (CRSP shrcd 10 or 11) observations.

[20] Using the full sample. I winsorize all continuous variables at the 1st and 99th percentiles to mitigate the effect of outliers on my inferences.

(Christensen et al. 2013; Daske et al. 2013). Less than half the sample represents observations from the pre-implementation period (mean of *Treat*×*Post* is 0.446), which is expected because of the staggered difference-in-differences research design. The univariate correlations between *Treat*×*Post* and both *Amihud* and *Bid_ask* are positive and significant at the one percent level. The association between the implementation of the new revenue standard and the liquidity measures at a univariate level is suggestive of the implementation causing an increase in liquidity. As expected, the correlation between the liquidity measures, *Amihud* and *Bid_ask*, is positive and significant at the one percent level.

CHAPTER 5. RESULTS

Liquidity effect

Table 3 provides the results from estimating Equation (1). The first and second trio of columns provide results without and with controls included in the estimating equation. All standard errors are clustered by firm.[21] *Amihud* is positively associated with *Treat×Post* at the one percent level in both estimations, and the coefficients are 0.09 and 0.10 without and with controls. The inferences from using *Bid_ask* as the dependent variable are similar, where the associations between *Bid_ask* and *Treat×Post* are positive and significant at the one percent level, and the coefficients are 0.05, regardless of whether controls are included. The associations between *Liquidity* and *Treat×Post* are positive and significant at the one percent level, and the coefficients are 0.03, regardless of whether controls are included.[22] Focusing on the *Liquidity* coefficient estimated from Equation (1) with controls, *Treat×Post* is associated with a 3% increase in liquidity and the 95% confidence intervals of the coefficient are 0.02 and 0.05. Providing evidence that the increase in liquidity is caused by the implementation of the new revenue standard is the objective of the section below.

[21] Following Petersen (2009), I include month-year fixed effects but do not cluster standard errors by month-year because the number of clusters (month-years) is only eight per firm. However, I also estimate a version of Equation (1) clustering by firm and year-month; untabulated findings result in the same inferences as those based on findings in Table 3.

[22] I also estimate Equation (1) excluding firm fixed effects. Untabulated findings reveal the coefficient on *Liquidity* is 0.03 and significant at the 5% level. Furthermore, the adjusted R-squared of the equation is 0.89, indicating that firm fixed effects are not solely responsible for the model explanatory power.

Liquidity effect: Parallel trends assumption, reversal of the difference test, and placebo tests

One of the main assumptions of the difference-in-differences design is the parallel trends assumption. This assumption is inherently untestable because it assumes that the trends of the treated and control firms would have continued in the absence of the treatment. However, one can provide support for the parallel trends assumption by providing evidence that there was no trend before the treatment or that there were indistinguishable differences between the treatment and control group's dependent variable before the treatment. I follow Kraft et al. (2018) and Glaeser (2018) and estimate the following equation:

$$Liquidity_{it} = \beta_1 Before\_all_{it} + \beta_2\, After\_all_{it} + Controls_{it} + \mu_i + \gamma_t + \varepsilon_{it}, \qquad (5)$$

where *Before_all* is an indicator variable that equals one for all periods before the implementation of the new revenue standard, and *After_all* is an indicator variable that equals one for all periods after the implementation. The coefficients of Equation (5) have to be interpreted with the understanding that all firms are eventually treated during the sample period, and consequently, one period has to be used as the reference group. I use the period just before the implementation as the reference group. For example, if a firm has a December (June) fiscal year-end, then the December (June) reporting period is used as the reference group because the first treated report is the March (September) quarterly report. An insignificant β_1 coefficient is evidence of the absence of a trend before the implementation of the standard.

I also estimate an extended version of Equation (5), disaggregating the *Before_all* *(After_all)* variables into specific periods before (after) the implementation:

$$Liquidity_{it} = \beta_1 Before_{it-3} + \beta_2 Before_{it-2} + \beta_3 Before_{it-1} + \beta_4 After_{it+1} + \beta_5 After_{it+2} + \beta_6 After_{it+3} + Controls_{it} + \mu_i + \gamma_t + \varepsilon_{it}, \qquad (6)$$

where $Before_{it-n}$ ($After_{it+n}$) is the n periods before (after) the reference group used in Equation (4).[23] Similar to the interpretation of Equation (5), insignificant β_1, β_2, and β_3 coefficients are evidence of no trend before the implementation. Significant β_4, β_5, and β_6 coefficients are evidence that *Liquidity* in the post-implementation period is significantly different from that in the pre-implementation period.

Table 4 and Figure 1 provide support for the parallel trends assumption by providing evidence that there was no trend before the implementation. The results from estimating Equations (5) and (6) indicate that there were no significant differences in liquidity in the periods before the implementation, and no trend was, therefore, present before the implementation. The coefficient on *Before_all*, 0.00, is insignificant, which indicates that the liquidity in the period before the implementation is not significantly different from liquidity in all periods before the implementation combined. Furthermore, when I disaggregate *Before_all* into more granular periods in Equation (6), the $Before_{t-3}$, $Before_{it-2}$, and $Before_{it-1}$ coefficients, 0.00, 0.00, and –0.01, and are all insignificant. The results imply that there was no significant liquidity difference in any of the periods before the implementation, and no trend was, therefore, present, providing evidence in support of the parallel trends assumption. Contrary to the *Before_all* coefficient, the *After_all* coefficient, 0.03, is significant at the one percent level. Disaggregating the *After_all* into more granular periods, $After_{t+1}$, $After_{it+2}$, and $After_{it+3}$, reveals that all three coefficients are positive and significant. Taken together, Table 4 shows that there was no trend in the pre-implementation period and that *Liquidity* in the post-implementation period differs significantly from that of the pre-implementation period.

[23] If an observation is more than three periods before (after) the period in the reference group it is included with the observations in third period before (after) the reference group.

Visually it seems as though there is a positive trend after the implementation of the standard. This could happen because as more firms implement the standard, firms that have already implemented the standard experience a positive spillover effect (Gao et al. 2019). To test whether this the case, I estimate a version of Equation (5) in which I interact *After_all* with *Trend*, where *Trend* is the number of quarters relative to the reference period. Untabulated results show that there is a positive trend (coefficient = 0.01), but that the coefficient is insignificant. Therefore, even though a spill-over effect exists, the majority of the liquidity effect occurs at initial implementation.

The setting allows me to perform a reversal of differences test. As explained above, the December fiscal year-end firms will be the first firms to implement the new revenue standard in their March 2018 quarterly reports. If the implementation caused a liquidity difference between the December and non-December fiscal year-end firms, one would expect the difference to decrease as the non-December fiscal year-end firms implement the standard. To test whether this is the case, I estimate the following equation:

$$Liquidity_{it} = \beta_1 First\_adopters \times March2017_{it} + \beta_2 First\_adopters \times June2017_{it} + \beta_3 First\_adopters \times September2017_{it} + \beta_4 First\_adopters \times March2018_{it} + \beta_5 First\_adopters \times June2018_{it} + \beta_6 First\_adopters \times September2018_{it} + \beta_7 First\_adopters \times December2018_{it} + Controls_{it} + \mu_i + \gamma_t + \varepsilon_{it}, \quad (7)$$

where *First_adopters* are firms with a December fiscal year-end and were, therefore, the first to implement the standard.[24] *March2017* to *December2018* are indicator variables that equal one if the fiscal quarter ends in that month, and zero otherwise. Insignificant β_1, β_2, and β_3

[24] *First_adopters* and *March2017* to *December2018* are omitted in the equation because *First_adopters* is collinear with firm and *March2017* to *December2018* is collinear with month-year.

coefficients are evidence that there is no liquidity difference between the December and non-December fiscal year-end firms in each of the periods leading up to the implementation of the new revenue standard.[25]

If the implementation caused a liquidity difference between the December and non-December fiscal year-end firms, one would expect the difference to exist for the first period after the implementation, i.e., $\beta_4 > 0$, and to decrease and become insignificant for subsequent periods, i.e., $\beta_4 > \beta_5 > \beta_6 > \beta_7$. Theoretically, β_7 should not be significantly different from zero because all firms are treated at this stage, and there should be no liquidity difference between the December and non-December fiscal year-end firms.[26] An insignificant β_7 is evidence that the results from estimating Equation (1) are not because of an underlying trend, but because of the implementation of the standard. Therefore, any confounding factor that might explain the results from estimating Equation (1) would have to explain the reversal from estimating Equation (7).

Table 5, which presents findings from estimating Equation (7), and Figure 2 provide evidence on the differences between the *First_adopters* and the rest of the sample. The coefficients on *First_adopters×March2017*, *First_adopters×June2017*, and *First_adopters×September2017* are –0.005, –0.003, and –0.012, and are all insignificant, which implies that there was no liquidity difference between the *First_adopters* and the rest of the

[25] I use only firms with quarterly or annual reports that coincide with the December fiscal year-end firms' quarterly or annual reports. In other words, Equation (7) includes only March, June, September, and December fiscal year-end firms. I exclude the other fiscal year-end firms because I compare *Liquidity* at December fiscal year-end firms' reporting dates and only March, June, and September fiscal year-end firms have calculated *Liquidity* periods that coincide with December fiscal year-end firms' reporting dates. The firms included in Equation (7) represent more than 90% of the sample. Also, in additional analyses I assess the sensitivity of my inferences based on the Table 3 findings by using only March, June, September and December fiscal year-end firms.

[26] Similar to Equations (4) and (5), the reference (omitted) period is the period before the implementation. In this case it is quarterly and annual reports for the quarter ending December 2017.

sample in the pre-implementation period.[27] The coefficient on *First_adopters×March2018*, 0.031, is significant at the one percent level, which implies that a difference between the *First_adopters* and the rest of the sample originated when the *First_adopters* were treated, i.e., when they implemented the new revenue standard.

Table 5 and Figure 2 also show that the difference between the *First_adopters* and the rest of the sample decreases monotonically and becomes less significant as more firms implement the standard. More specifically, the difference is significant at the one percent level in the first period after the *First_adopters* implemented the standard (coefficient = 0.031), significant at the five percent level in the second period after the implementation (coefficient = 0.029), and insignificant in the third period after the implementation (coefficient = 0.026). In the fourth period after the implementation, once all firms have implemented the standard, the difference between the *First_adopters* and the rest of the sample is insignificant (coefficient = 0.011). Therefore, if a factor other than the implementation of the new revenue standard caused the decrease in liquidity, that factor should also systematically decrease the liquidity difference between December and non-December fiscal year-end firms as non-December fiscal year-end firms implement the standard.

I also perform placebo tests by leading the implementation of the new revenue standard by one and two year(s). If there is something inherently different about December fiscal year-end firms that causes liquidity to be different at certain times of the year, this difference should be present in the years before the implementation of the standard.[28] Therefore, I estimate modified

[27] By construction the coefficient on *First_adopters×December2017* is zero as it is the reference group.

[28] An alternative approach is to assign the implementation to the control groups. However, this cannot be done in this setting because all firms eventually implement the standard. The next best approach is randomizing the implementation date and this is the approach I follow by leading the implementation date.

versions of Equation (1) where I lead the implementation by one or two years.[29] Untabulated findings reveal that leading the implementation by one (two) year(s) results in an insignificant coefficient of –0.01 (–0.00) on *Treat*×*Post*. These results provide evidence that the observed effect is not attributable to systematic differences between December and non-December fiscal year-end firms that arise every year.

Precision and comparability effects

Table 6 reports the results from estimating Equation (2), i.e., the precision effect of the implementation of the new revenue standard. Column 1 reveals that *Precision* increases by 11% following the implementation of the standard.[30] Columns 2 and 3 show the results when estimating Equation (2) using each of the variables that comprise of the *Precision* factor, i.e., *Precision_residual* and *Precision_returns*. As expected, both these variables are positively related with *Treat*×*Post*. Columns 4 to 6 reveal the results when I calculate *Precision* by estimating alternative versions of Equation (4). Column 4 (5) shows the results when I use the share price of the firm (cash flow from operations) as a proxy for economic events instead of *Returns*. The *Treat*×*Post* coefficient using the share price of the firm (cash flow from operations) as a proxy for economic events is 0.10 (0.11) and is significant at the one percent level. I also estimate Equation (4) per industry, therefore allowing the slope coefficient to differ between industries when calculating *Precision*. The *Treat*×*Post* coefficient in column 6 is 0.10 and is significant at the one percent level, suggesting that the alternative estimation procedures do not alter the inferences based on the findings in columns 1 to 3.

[29] I also lead the sample period by one (two) years in the placebo tests. In other words, the sample period for leading the implementation date by one (two) year(s) is March 1, 2016 to February 28, 2018 (March 1, 2015 to February 28, 2017).

[30] The percentage increase is calculated as $e^{0.1}-1 = 0.11$.

Investors might not incorporate the effect of the new revenue standard immediately because they do not initially understand how the economic events map into the changed accounting report. Column 7 indicates that the inferences remain unchanged when I exclude the first period after the implementation of the standard, but, as expected, the λ_1 coefficient, i.e., the increase in precision, increases from 0.10 to 0.20. In column 8 I estimate a modified version of Equation (2) where I lead the implementation by one year. The findings in column 8 reveal that leading the implementation by one year results in an insignificant coefficient of 0.01 on *Treat*×*Post*. Overall, the evidence in Table 6 suggests that the implementation of the new recognition standard increases precision.

Table 7 provides evidence on the increase in the number of informed investors, i.e., the comparability effect of the implementation of the standard. Column 1 reveals that the implementation of the new revenue standard is associated with a 3% increase in the number of informed investors. I again exclude the first period after the implementation of the standard and, as expected, find that the coefficient on *Treat*×*Post* increases from 0.03 to 0.04. I also lead the implementation by one year (column 3) and show that there was no comparability effect in the prior year.

Theory predicts that higher (lower) precision will attract fewer (more) informed investors (Gao et al. 2019). Furthermore, the increase in informed investors could increase liquidity, and the higher liquidity could subsequently attract more informed investors in subsequent periods. Therefore, I estimate Equation (3) but control for *Precision* and the level of *Liquidity* in the previous period. The coefficient of interest in column 4, i.e., the coefficient on *Treat*×*Post*, is 0.02 and is significant at the one percent level. As predicted by theory, the coefficient on

Precision is negative, but it is insignificant. I explore the various paths in further detail in the path analysis section below.

I study the construct validity of *Informed* by investigating whether the increase in *Informed* is a function of the relative size of the firm. Gao et al. (2019) shows that the increase in the number of informed investors in firm *i* will be a function of the total size of all other firms using the accounting standard relative to the size of firm *i*, where size refers to the number of liquidity traders in the firm. Therefore, according to the model, a firm that has relatively fewer liquidity traders should experience a larger increase in the number of informed traders. Because the number of liquidity traders in a firm is unobservable, I use the number of shareholders in a firm, *Num_sh*, as a proxy for the number of liquidity traders and therefore assume a positive association between *Num_sh* and the number of liquidity traders. I calculate the relative size of firm *i* as the total size of all other firms divided by the size of firm *i*, i.e,: $Relative\_size_i = \frac{\sum_{i=1}^{n} Num\_sh_{-i}}{Num\_sh_i}$.[31,32] Therefore, I estimate a version of Equation (3) where I interact *Treat*×*Post* with *Relative_size*. As expected, the coefficient on the interaction is positive and significant at the one percent level which adds to the construct validity of using *Informed* as a proxy for the number of informed investors.

Cross-sectional analyses

If *Precision* and *Informed* are the channels through which the implementation of the new revenue standard affects liquidity, then the coefficient on *Treat*×*Post* should decrease or become

[31] I use the number of shareholders in the period before the implementation of the standard.

[32] *Relative_size* is standardized to have mean zero and a standardized deviation of one to ease interpretation.

insignificant once I control for the channels, i.e., *Precision* and *Informed*. Therefore, I estimate the following equation:

$$Liquidity_{it} = \beta_1 Treat \times Post_{it} + \beta_2 Precision_{it} + \beta_3 Informed_{it} + Controls_{it} + \mu_i + \gamma_t + \varepsilon_{it}. \tag{8}$$

Column 1 in Table 8 reports the results from estimation of Equation (8). The insignificant *Treat*×*Post* coefficient provides evidence that *Precision* and *Informed* are the channels through which the implementation of the standard affects liquidity. Another important observation is that the proxies used for precision and the number of informed investors are positively associated with liquidity, as predicted by theory. The *Precision* and *Informed* coefficients, 0.08 and 0.35, are positive and significant at the one percent level. The fact that the associations are as predicted by theory bolsters the construct validity of the proxies used for precision and the number of informed investors.

Column 2 (3) of Table 8 shows the results from the estimation of a version of Equation (1) that includes firms for which *Precision* decreases (increases). The difference in *Precision* is the difference between the mean *Precision* before and after the implementation of the revenue standard, and I demean *Precision* by month-year before calculating the difference to control for month-year fixed effects. Furthermore, I control for *Informed* because I am interested in the liquidity effect of an increase in *Precision*. As expected, the firms that experience an increase in *Precision* also experience an increase in liquidity, whereas the firms that experience a decrease in *Precision* experience no increase in liquidity. I follow a similar process and estimate a version of Equation (1) for those firms that experience a decrease and increase in *Informed*. Column 4 shows firms that experience a decrease in *Informed* do not experience an increase in liquidity, whereas the firms that experience an increase *Informed* do experience an increase in liquidity.

I also estimate Equation (1) for those firms that experience a decrease in both *Precision* and *Informed.* As expected, column 6 shows that these firms do not experience an increase in liquidity. Finally, I estimate Equation (1) for those firms that experience an increase in both *Precision* and *Informed*. Column 7 reveals that this subset of firms experiences the greatest increase in liquidity, i.e., the coefficient is 0.074, whereas the coefficient for those firms that experience an increase in *Precision* (*Informed*) is 0.031 (0.047). Therefore, the results seem to suggest that those firms that experience an increase in both *Precision* and *Informed* experience the greatest liquidity increase and that firms that experience an increase in neither *Precision* nor *Informed* do not experience an increase in liquidity. The results, therefore, suggest that both precision and comparability are the channels through which the implementation of the standard affects liquidity.

Path analysis

Next, I use path analysis to provide additional support for the inferences I draw from the cross-sectional analysis. Using path analysis, I disaggregate the correlation between the implementation of the new revenue standard and liquidity into the channels identified by theory. Doing so provides evidence on the existence and relative importance of the channels between the implementation of the standard and liquidity (Bhattacharya et al. 2012; Landsman et al. 2012). The two indirect, or mediated, channels are the two channels identified by theory: the precision and comparability channels. The direct channel is the correlation that is not explained by the two indirect channels. Figure 3 shows the path diagram. The path arrows represent the assumed relations among variables and the proxies for the theoretical constructs are indicated in parenthesis.

I implement path analysis using the following system of equations:

$$Precision_{it} = \lambda_1 Treat \times Post_{it} + \mu_i + \gamma_t + \varepsilon_{it} \tag{2}$$

$$Informed_{it} = \theta_1 Treat \times Post_{it} + \mu_i + \gamma_t + \varepsilon_{it} \tag{3}$$

$$Liquidity_{it} = \beta_1 Treat \times Post_{it} + \beta_2 Precision_{it} + \beta_3 Informed_{it} + Controls_{it} + \mu_i + \gamma_t + \varepsilon_{it}. \tag{8}$$

The total effect of each channel is calculated as the product of the two coefficients leading to and from each of the mediating variables, i.e., *Precision* and *Informed.* Regarding the precision (comparability) channel, the λ_1 (θ_1) coefficient, i.e., the correlation between *Treat*×*Post* and *Precision* (*Informed*), is 0.107 (0.030) and significant at the one percent level, indicating that the implementation of the revenue standard is associated with an 11% (3%) increase in precision (the number of informed investors). The β_2 (β_3) coefficient, i.e., the correlation between *Precision* (*Informed*) and *Liquidity*, is 0.084 (0.36) and is significant at the one percent level. The total precision (comparability) channel effect that is obtained by multiplying the λ_1 and β_2 (θ_1 and β_3) coefficients is 0.009 (0.011).

The direct effect, i.e., the correlation between *Treat*×*Post* and *Liquidity* is insignificant. The direct channel's insignificant β_1 coefficient, 0.009, adds to the construct validity of the proxies used for precision and number of informed investors because the *Precision* and *Informed* channels account for all of the significant correlation between *Treat*×*Post* and *Liquidity*. Focusing only on the significant channels reveals that the precision channel accounts for 45% of the increase in liquidity, whereas the comparability channel accounts for 55% of the increase in liquidity. More importantly, the inferences are consistent with the inferences based on Table 7 findings, i.e., both the precision and comparability channel contribute to the increase in liquidity.

In addition to the path analysis as outlined in Figure 3, I also estimate a version that includes paths from *Precision* to *Informed*, *Informed* to *Precision*, and *Liquidity* to *Informed* (see figure 4). Inferences from estimating the alternative path analysis remain unchanged.

Specifically, the total effect of the two channels, i.e., total indirect effect, accounts for 0.014 of the increase and is significant at the one percent level. The direct effect is significant using the alternative path analysis, but only at the 10 percent level (coefficient = 0.014).

Liquidity effect: Additional analyses

Firms in specific industries might choose the same fiscal year-end as their peers, which suggests that the liquidity implementation effect is concentrated in industries where firms cluster their fiscal year-ends. To address this concern, I re-estimate Equation (1) using industries whose firms' fiscal year-ends are less clustered. To do so, I calculate the number of firms per industry that are expected to have a December fiscal year-end.[33] I calculate the expectation by multiplying the number of firms in an industry by the percentage of all firms with a December fiscal year-end, irrespective of industry. I then delete all industries where the actual number of December fiscal year-end firms is 2/1 (1/2) times more (less) than the expected number of December fiscal year-end firms and re-estimate Equation (1). Untabulated findings reveal the inferences are the same as those based on Table 3 findings.[34]

I also re-estimate Equation (1) by including fiscal quarter fixed effects, where fiscal quarter number refers to the first, second, third, or fourth fiscal quarter. Untabulated findings based on the modified equation yields the same inferences as those based on Table 3 findings. I also delete fourth fiscal quarters (annual reports) and re-estimate Equation (1). Untabulated findings reveal the inferences are the same as those based on Table 3 findings.

[33] Unless noted otherwise, industry refers to the Fama-French 48 industry classification.

[34] Choosing to delete all industries where the actual number of December fiscal year-end firms is 3/2 (2/3), 4/3 (3/4), 5/4 (4/5), or 6/5 (5/6) times more (less) than the expected number of December fiscal year-end firms does not change the inferences.

I estimate a version of Equation (1) in which I interact *Treat*×*Post* with *NonDec*, where *NonDec* is an indicator variable that equals one if the firm has a non-December fiscal year-end, and zero otherwise. Untabulated findings reveal that the coefficient on the interacted variable is insignificantly different from zero, indicating that the liquidity effect of the implementation is present in December and non-December fiscal year-end firms and that the effect does not differ between December and non-December fiscal year-end firms.

I also use data from Compustat to identify firms that were substantively affected by the implementation of the new revenue standard. Compustat defines substantively affected as "accounting changes that have a substantive impact on the measurement and presentation of financial data, or which require significant new disclosures." I estimate a version of Equation (1) in which I interact *Treat*×*Post* with *Material*, where *Material* is an indicator variable that equals one if the firm was substantively affected by the implementation of the standard. Untabulated findings reveal that, as expected, the coefficient on the interacted variable is positive and significant at the one percent level. Furthermore, the twenty-fifth (fiftieth) [seventy-fifth] percentile of the mean number of firms substantively affected per industry is 45% (54%) [64%], alluding to the fact that the majority of industries were substantively affected by the implementation of the standard. This reinforces the notion that the implementation of the standard affected a wide variety of firms across all industries. In addition, inferences from Table 3 findings remain unchanged when I eliminate all firms that were substantively affected by the implementation of other standards at the same time as when the new revenue standard was implemented.

Furthermore, in accordance with SEC Staff Accounting Bulletin No. 74 (SAB 74), *Disclosure of the impact that recently issued accounting standards will have on the financial* *statements of the registrant when adopted in a future period*, it is possible that firms could disclose certain information before the implementation date of the standard. This is, however, not a concern for three reasons. First, the evidence seems to suggest that less than 12% of firms disclosed the effect of the new revenue standard on revenue before the implementation of the standard (Coleman and Usvyatsky 2019). Second, even if firms disclose the effect of the implementation of the standard on revenue, firms will still disclose additional information once they implement the standard. Third, the disclosure in accordance with SAB 74 would bias against finding any significant association between the implementation of the standard and liquidity because the liquidity effects of SAB 74 would occur before the implementation of the standard, therefore biasing the coefficient between the implementation of the standard and liquidity downwards.

I also conduct several other analyses to assess the sensitivity of my inferences to various sample characteristics. In particular, I delete bank observations, use the quarterly means (instead of median) of the daily Amihud (2002) measure and daily quoted spreads, create a *Liquidity* factor that includes the proportion of zero-returns trading days, only include firms that have no missing observations during the sample period, only include firms that have reporting periods that end on March, June, September, and December, and calculate the liquidity variables using daily data between the earnings announcement date and the end of the following fiscal quarter . All inferences from re-estimating Equation (1) using the different samples are the same as those based on Table 3 findings.

Alternative measure of comparability

I develop an alternative measure for comparability using the economic intuition as discussed in De Franco et al. (2011). As mentioned earlier, in Equation (4) ($Revenue_{it} = \beta_1 Returns_{it} + \mu_i + \gamma_t + \varepsilon_{it}$) *Revenue* is a proxy for the accounting report, *Returns* is a proxy for economic events, and the β_1 coefficient reflects the mapping of economic events to the accounting report during the period. If the new revenue standard causes firms in different industries to account for the same economic events in a similar manner, then the difference in mapping between industries should decrease after the implementation of the standard. To empirically test whether this is the case, I first estimate Equation (4) per industry in the pre-and post-implementation period, respectively. This allows me to obtain a mapping (β_1) per industry for the pre- and post-implementation period.

Next, I determine how much a specific industry's mapping differs from other industries' mappings. I do so by calculating the average absolute difference between industry *i*'s and industry *j*'s mapping, i.e., $Alt\_Comp_{i\omega} = (\frac{1}{J} \times \sum_{j=1}^{J} |\beta_{1i\omega} - \beta_{1j\omega}|)$, where $\beta_{1i\omega}$ ($\beta_{1j\omega}$) is the coefficient on *Returns* from estimating Equation (4) for industry *i* (*j*) in period *ω*, *J* is the number of industries excluding industry *i*, and *ω* represents one of two distinct periods, i.e., the pre- or post-implementation period. I use the negative of the natural logarithm of *Alt_Comp* to ease interpretation, i.e., the higher *Alt_Comp*, the higher comparability.

I estimate the following equation to determine whether the implementation of the new revenue standard is associated with an increase in the alternative comparability measure:

$$Alt\_Comp_{i\omega} = \beta_1 Treat \times Post_{it} + \mu_p + \gamma_t + \varepsilon_{it}, \tag{9}$$

where the *p* subscript refers to industry. I use industry fixed effects and cluster standard errors by industry because the dependent variable, *Alt_Comp*, is calculated per industry.

Untabulated results reveal that comparability increases after the implementation of the new revenue standard. Furthermore, the standard deviation of the industry-specific β_1's decrease after the implementation of the standard. This alludes to the comparability effect of the standard, i.e., that the difference in mapping between industries decreases. I omit the intercept when estimating Equation (4) to allow β_1 to reflect the total mapping of economic events to the accounting report. Two alternative approaches are to estimate Equation (4) with an intercept and assume the mapping is the sum of the intercept and β_1 or ignore the intercept. Inferences are unchanged when using *Alt_Comp* that is calculated using the alternative estimation procedures. Furthermore, inferences are unchanged when I use cash flow from operations as a proxy for economic events instead of *Returns*. I also use the Hoberg-Phillips Fixed Industry Classification (FIC) data to calculate *Alt_Comp* (Hoberg and Phillips 2010; Hoberg and Phillips 2016).[35] Inferences are unchanged when I use FIC 50 or FIC 100.

Untabulated results show that the higher the increase in *Alt_Comp*, the greater the increase in *Informed*. This adds to the construct validity of *Alt_Comp* and alludes to the theoretical channel between comparability and the number of informed investors. Furthermore, I change the unit of analysis to be an industry instead of a firm. *Treat*×*Post* in this specification is the mean of all firms' *Treat*×*Post* in an industry quarter. Even though the number of observations decreases substantially (914 observations), the inferences in column 1 remain unchanged when using the alternative industry quarter sample. Specifically, the coefficient on *Treat*×*Post* is 0.54 and is significant at the one percent level. Finally, I re-estimate columns 2 and 3 of Table 8 using *Alt_Comp* instead of *Informed*. Untabulated results show that inferences remain unchanged, i.e., the increase in liquidity is concentrated in the sample of firms that experience an increase in *Alt_Comp*. In summary, the evidence suggests that using an alternative proxy for comparability does not alter the main inference, i.e., that the implementation of the new revenue standard is associated with an increase in comparability and the increase in comparability is associated with an increase in liquidity.

[35] TNIC industry data are available from the Hoberg-Phillips data library.

Effect on revenue attributes

It is plausible that the new revenue standard could affect particular revenue attributes because the timing and amount of revenue recognition changes after the implementation of the standard. An increase in particular revenue attributes provides additional evidence that the implementation of the standard is associated with an increase in liquidity because prior literature finds a positive association between particular earnings attributes and liquidity (Affleck-Graves et al. 2002; Bhattacharya et al. 2012; Lang et al. 2012).

I examine three revenue attributes: persistence, predictability, and smoothness (Francis et al. 2004). *Persistence* is the coefficient on $Revenue_{t-1}$ when *Revenue* is regressed on $Revenue_{t-1}$. A larger (smaller) coefficient implies more (less) persistence. I interact *Treat*×*Post* with $Revenue_{t-1}$ to determine the incremental effect of the new revenue standard on persistence. *Predictability* is the negative of the natural log of the standard deviation of the residual from the aforementioned equation. Large (small) values of predictability imply more (less) predictable revenue. *Smoothness* is the negative of the natural log of the standard deviation of *Revenue* divided by the standard deviation of cash flow from operations. Large (small) values of smoothness imply more (less) revenue smoothness.

Untabulated results show that revenue persistence, predictability, and smoothness increase after the implementation of the new revenue standard. Specifically, the coefficient on the $Treat \times Post \times Revenue_{t-1}$ is 0.02 and is significant at the one percent level. The coefficient on *Treat*×*Post* when *Predictability* (*Smoothness*) is regressed on *Treat*×*Post* is 0.25 (0.11) and is significant at the one percent level. Therefore, the evidence suggests that the implementation of the standard is associated with an increase in the persistence, predictability, and smoothness of revenue.

CHAPTER 6. CONCLUSION

I investigate the liquidity effect of the implementation of the new revenue recognition standard using a staggered difference-in-differences design. Analytical work shows that the implementation of standards can affect liquidity through either the precision channel, i.e., the change in the accounting report's ability to reflect economic events, the comparability channel, i.e., the increase in comparability across reporting entities, or both. Analytical work also shows that an increase (decrease) in either precision or comparability will lead to an increase (decrease) in liquidity, and therefore the net effect on liquidity is dependent on the change of both precision and comparability (Barth et al. 1999; Gao et al. 2019).

I show that the implementation of the new revenue standard increases liquidity. I perform various analyses to mitigate concerns that other factors cause the increase in liquidity. I then show that firms that implement the standard first experience an increase in liquidity relative to firms that implement the standard at a later stage and that this liquidity difference decreases as more firms implement the standard. This liquidity difference is insignificantly different from zero once all firms implement the standard.

I further show that both precision and comparability increase after the implementation of the new revenue standard. I also find that firms that experience an increase in either precision or comparability experience an increase in liquidity, but that firms that experience an increase in neither precision nor comparability do not experience an increase in liquidity. I use path analysis to show that the increase in liquidity is attributable to both the precision and comparability channel. Lastly, I use an alternative proxy for comparability and show that my inferences remain unchanged when I use the alternative proxy.

I contribute to the literature by providing early evidence of the capital market effects of the implementation of the new revenue standard. I provide evidence on the theoretically motivated channels through which the implementation of the accounting standard affects liquidity. I also show that increases in precision or comparability are associated with increases in liquidity and that some firms do not experience an increase in liquidity. The interaction between the precision and comparability channel could be of particular interest to standard setters as it shows, consistent with theory, that new standards can not only increase comparability, but also change the precision with which accounting reports reflect economic events. In addition, the fact that both these constructs have liquidity effects provides insights potentially relevant to standard setters concerned with capital market effects.

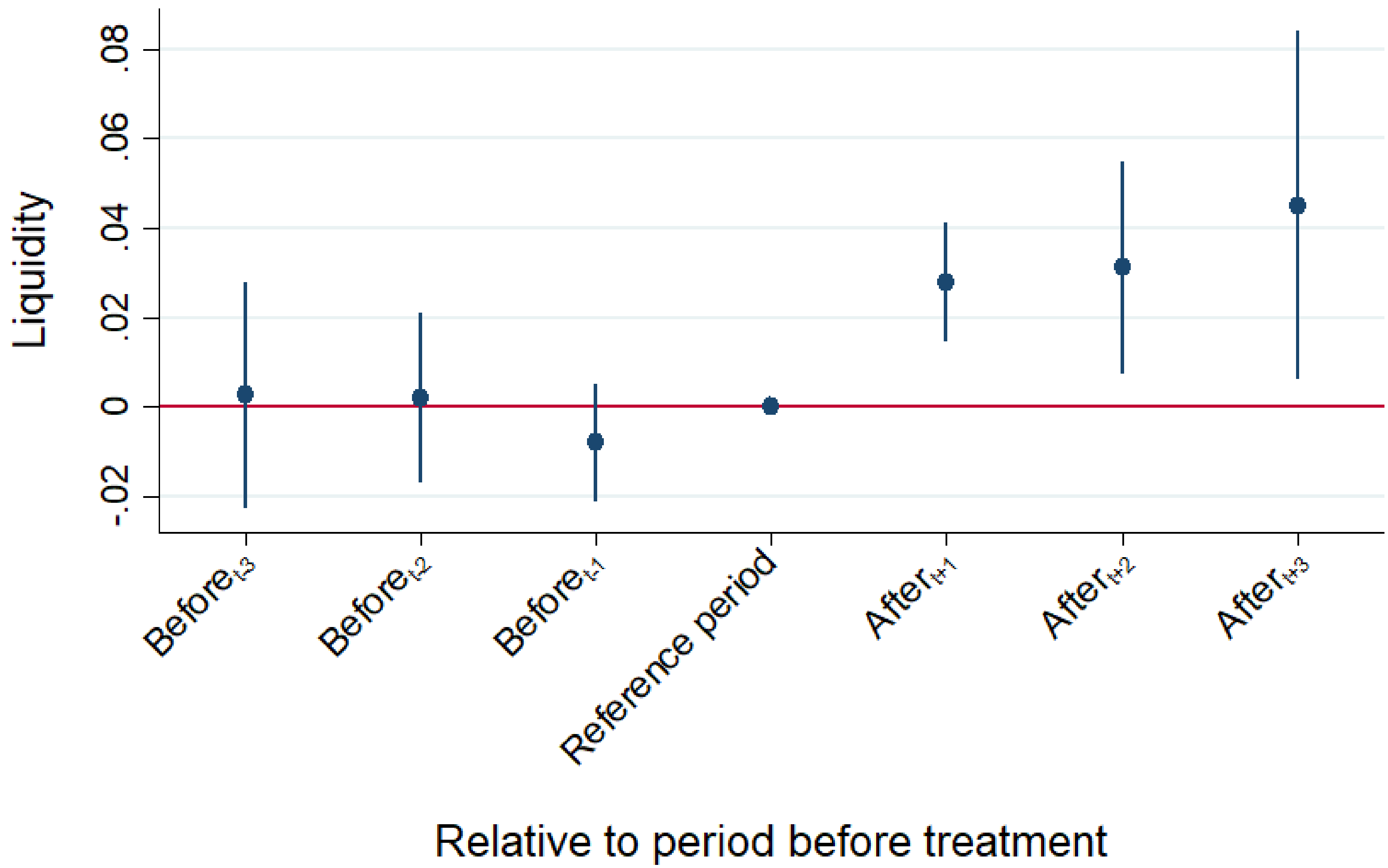

Figure 1. Parallel trends assumption

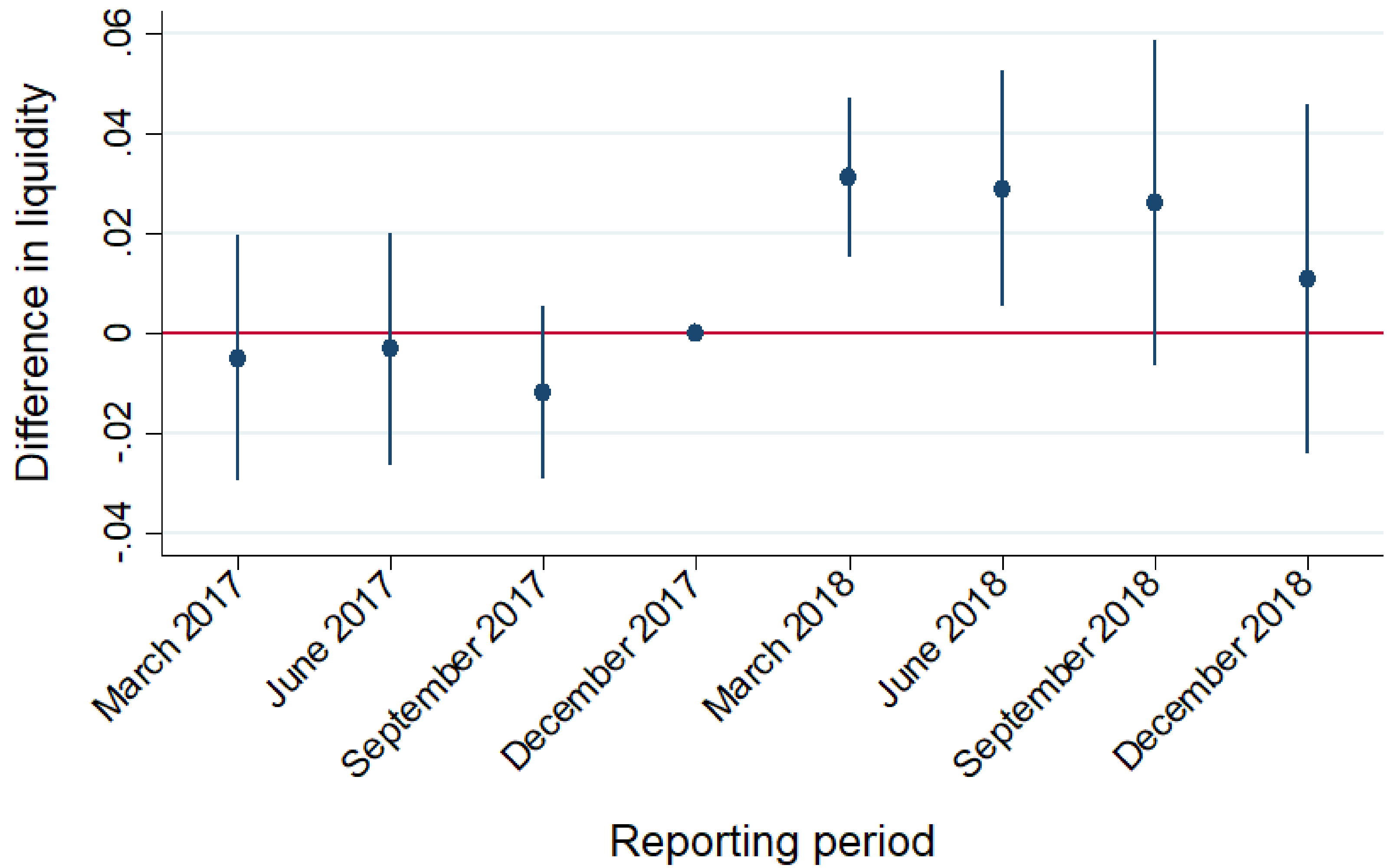

Figure 2. Reversal of difference

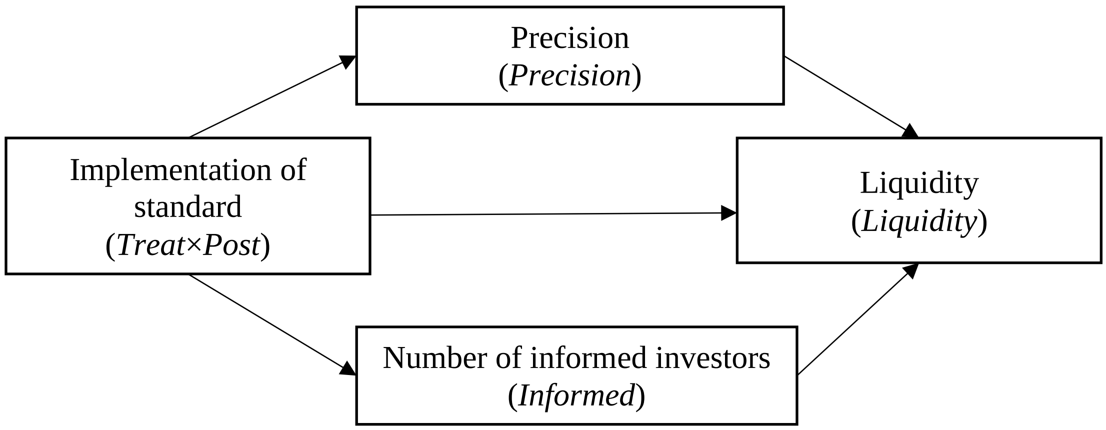

Figure 3. Path analysis

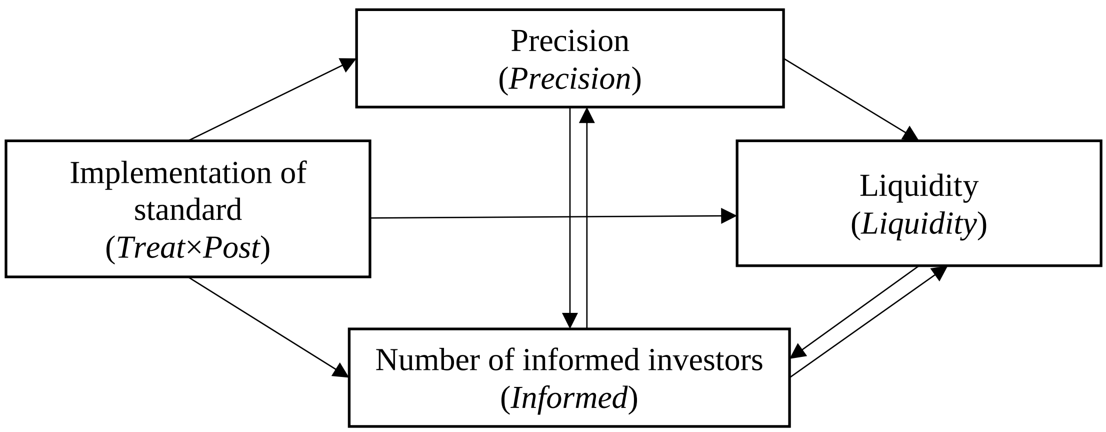

Figure 4. Path analysis: Additional paths

Table 1. Descriptive statistics

Variable	*N*	*Mean*	*Bottom Quartile*	*Median*	*Top Quartile*	*SD*
Treat×Post	24,675	0.446	0	0	1	0.497
Amihud	24,675	1.100	0.003	0.024	0.330	3.450
Bid_ask	24,675	0.504	0.031	0.109	0.563	0.849
SD_returns$_{t\text{-}4}$	24,675	-3.806	-4.203	-3.865	-3.441	0.555
Size$_{t\text{-}4}$	24,675	6.616	5.040	6.620	8.086	2.150
Turnover$_{t\text{-}4}$	24,675	-5.498	-6.035	-5.307	-4.777	1.131
ROA	24,675	-0.021	-0.014	0.003	0.016	0.087
MB	24,675	3.354	1.248	2.054	3.995	7.361
Leverage	24,675	0.241	0.040	0.189	0.370	0.238
Q_return	24,675	0.016	-0.106	0.012	0.123	0.240
Loss	24,675	0.360	0.000	0.000	1.000	0.480
SI	24,675	-0.003	-0.002	0.000	0.000	0.013
Analyst	24,675	5.566	1	4	8	5.778

Table 1 presents descriptive statistics for variables used in the analyses. All variables are defined in the Appendix. Note that Amihud and Bid_ask are not yet multiplied by negative one and are the variables before taking the natural log. Amihud (Bid_ask) is multiplied by 10^7 (100) in this Table for ease of interpretation. All continuous variables are winsorized at the 1st and 99th percentiles.

Table 2. Correlation matrix

Variable	*Treat×Post*	*Amihud*	*Bid_ask*	*SD_returns*$_{t-4}$	*Size*$_{t-4}$	*Turnover*$_{t-4}$	*ROA*	*MB*	*Leverage*	*Q_return*	*Loss*	*SI*	*Analyst*
Treat×Post	1.00												
Amihud	0.03*	1.00											
Bid_ask	0.02*	0.93*	1.00										
SD_returns$_{t-4}$	-0.01*	-0.48*	-0.49*	1.00									
Size$_{t-4}$	0.03*	0.93*	0.87*	-0.54*	1.00								
Turnover$_{t-4}$	0.05*	0.60*	0.56*	0.12*	0.47*	1.00							
ROA	-0.01*	0.33*	0.35*	-0.46*	0.35*	-0.03*	1.00						
MB	0.01	0.15*	0.14*	-0.01*	0.13*	0.08*	-0.01*	1.00					
Leverage	0.01	0.17*	0.14*	0.01*	0.19*	0.19*	-0.01	-0.06*	1.00				
Q_return	-0.01*	0.10*	0.09*	-0.05*	0.04*	-0.02*	0.12*	0.01	-0.03*	1.00			
Loss	-0.00	-0.36*	-0.37*	0.52*	-0.36*	0.05*	-0.58*	0.02*	0.03*	-0.11*	1.00		
SI	-0.00	0.05*	0.05*	-0.11*	0.05*	-0.06*	0.32*	0.01	-0.06*	0.03*	-0.24*	1.00	
Analyst	0.01	0.70*	0.63*	-0.26*	0.71*	0.46*	0.19*	0.15*	0.13*	0.04*	-0.17*	0.02*	1.00

Table 2 presents the correlation matrix for the variables used in the analyses. * indicates a significance level at one percent.

Table 3. Liquidity effect of implementation

$$Liquidity\_variable_{it} = \beta_1 Treat \times Post_{it} + Controls_{it} + \mu_i + \gamma_t + \varepsilon_{it} \quad (1)$$

Variables	*Amihud*	*Bid_Ask*	*Liquidity*	*Amihud*	*Bid_Ask*	*Liquidity*
Treat×Post	0.09***	0.05***	0.03***	0.10***	0.05***	0.03***
	(3.86)	(3.56)	(4.18)	(4.13)	(3.56)	(4.31)
$SD\_returns_{t-4}$				0.06***	0.01	0.01**
				(3.20)	(0.57)	(2.05)
$Size_{t-4}$				0.03	0.01	0.01
				(1.00)	(0.40)	(0.75)
$Turnover_{t-4}$				-0.02	0.02**	0.00
				(-1.50)	(2.00)	(0.44)
ROA				0.43**	0.20*	0.13**
				(2.38)	(1.95)	(2.37)
MB				0.01***	0.00***	0.00***
				(5.50)	(4.64)	(5.45)
Leverage				-0.89***	-0.44***	-0.28***
				(-6.52)	(-4.87)	(-6.02)
Q_return				0.15***	0.03*	0.03***
				(6.31)	(1.82)	(4.33)
Loss				-0.11***	-0.04***	-0.03***
				(-7.26)	(-3.51)	(-5.68)
SI				-0.10	0.51	0.14
				(-0.18)	(1.41)	(0.75)
Analyst				0.02***	0.01***	0.01***
				(4.76)	(4.44)	(5.24)
Observations	24,675	24,675	24,675	24,675	24,675	24,675
R^2	0.972	0.950	0.970	0.973	0.950	0.971
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm	Firm	Firm

This table presents regression summary statistics for the estimation of Equation (1). *t*-statistics are presented in parentheses. All continuous variables are winsorized at the 1st and 99th percentiles. The superscript asterisks *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 4. Parallel trends assumption

$$Liquidity_{it} = \beta_1 Before\_all_{it} + \beta_2\ After\_all_{it} + Controls_{it} + \mu_i + \gamma_t + \varepsilon_{it} \quad (5)$$

$$Liquidity_{it} = \beta_1 Before_{it-3} + \beta_2 Before_{it-2} + \beta_3 Before_{it-1} + \beta_4 After_{it+1} + \beta_5 After_{it+2} + \beta_6 After_{it+3} + Controls_{it} + \mu_i + \gamma_t + \varepsilon_{it} \quad (6)$$

Variables	*Liquidity*	*Liquidity*
Before_all	0.00	
	(0.20)	
After_all	0.03***	
	(4.34)	
Before$_{t-3}$		0.00
		(0.20)
Before$_{t-2}$		0.00
		(0.22)
Before$_{t-1}$		-0.01
		(-1.21)
After$_{t+1}$		0.03***
		(4.08)
After$_{t+2}$		0.03**
		(2.58)
After$_{t+3}$		0.05**
		(2.28)
Observations	24,675	24,675
R^2	0.971	0.971
Firm FE	Yes	Yes
Month-year FE	Yes	Yes
Cluster	Firm	Firm
Controls	Yes	Yes

This table presents regression summary statistics for the estimation of Equations (5) and (6). *t*-statistics are presented in parentheses. All continuous variables are winsorized at the 1st and 99th percentiles. The superscript asterisks *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5. Reversal of differences

$$\begin{aligned} Liquidity_{it} = {} & \beta_1 First\_adopters \times March2017_{it} + \beta_2 First\_adopters \times June2017_{it} + \\ & \beta_3 First\_adopters \times September2017_{it} + \beta_4 First\_adopters \times March2018_{it} + \\ & \beta_5 First\_adopters \times June2018_{it} + \beta_6 First\_adopters \times September2018_{it} + \\ & \beta_7 First\_adopters \times December2018_{it} + Controls_{it} + \mu_i + \gamma_t + \varepsilon_{it} \qquad (7) \end{aligned}$$

Variable	*Liquidity*
First_adopters×March2017	-0.005
	(-0.40)
First_adopters×June2017	-0.003
	(-0.27)
First_adopters×September2017	-0.012
	(-1.36)
First_adopters×March2018	0.031***
	(3.84)
First_adopters×June2018	0.029**
	(2.41)
First_adopters×September2018	0.026
	(1.59)
First_adopters×December2018	0.011
	(0.61)
Observations	22,411
R^2	0.970
Firm FE	Yes
Month-year FE	Yes
Cluster	Firm
Controls	Yes

This table presents regression summary statistics for the estimation of Equation (7). *t*-statistics are presented in parentheses. All continuous variables are winsorized at the 1st and 99th percentiles. The superscript asterisks *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 6. Precision analysis

$$Precision_{it} = \lambda_1 Treat \times Post_{it} + \mu_i + \gamma_t + \varepsilon_{it} \qquad (2)$$

Variables	*Precision*	*Precision_residual*	*Precision_returns*	*Precision*	*Precision*	*Precision*	*Precision*	*Precision*
Column number	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat×Post	0.10***	0.29***	0.02**	0.10***	0.11***	0.10***	0.20***	0.01
	(7.53)	(14.41)	(2.13)	(7.39)	(8.22)	(7.59)	(11.84)	(0.83)
Observations	23,111	23,111	24,461	23,303	23,285	22,920	20,183	23,708
R^2	0.921	0.974	0.793	0.920	0.920	0.920	0.915	0.920
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm
Alternate economic events proxy				Price	Cash flow			
Alternate estimation						Industry		
Sample							Excluding first period	Placebo period

This table presents regression summary statistics for the estimation of Equation (2) and various versions thereof. *t*-statistics are presented in parentheses. All continuous variables are winsorized at the 1st and 99th percentiles. The superscript asterisks *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 7. Informed investors analysis

$$Informed_{it} = \theta_1 Treat \times Post_{it} + \mu_i + \gamma_t + \varepsilon_{it} \quad (3)$$

Variables	*Informed*	*Informed*	*Informed*	*Informed*	*Informed*
Column number	(1)	(2)	(3)	(4)	(5)
Treat×Post	0.03***	0.04***	-0.00	0.02***	0.03***
	(3.54)	(2.94)	(-0.49)	(3.04)	(3.51)
Precision				-0.01	
				(-1.18)	
Liq_{t-1}				0.34***	
				(23.26)	
Treat×Post×Relative_size					0.01**
					(2.05)
Observations	21,704	18,944	21,381	20,251	18,967
R^2	0.983	0.982	0.977	0.985	0.982
Firm FE	Yes	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm	Firm
Sample		Excluding first period	Placebo period		

This table presents regression summary statistics for the estimation of Equation (3) and various versions thereof. *t*-statistics are presented in parentheses. All continuous variables are winsorized at the 1st and 99th percentiles. The superscript asterisks *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 8. Cross-sectional analysis

$$Liquidity_{it} = \beta_1 Treat \times Post_{it} + \beta_2 Precision_{it} + \beta_3 Informed_{it} + Controls_{it} + \mu_i + \gamma_t + \varepsilon_{it} \quad (8)$$

Variable	*Liquidity*	*Liquidity*	*Liquidity*	*Liquidity*	*Liquidity*	*Liquidity*	*Liquidity*
Subsample	*NA*	*Decrease in Precision*	*Increase in Precision*	*Decrease in Informed*	*Increase in Informed*	*Decrease in Precision and Informed*	*Increase in Precision and Informed*
Column number	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat×Post	0.01	0.01	0.03***	0.00	0.05***	-0.01	0.07***
	(1.39)	(0.40)	(2.96)	(0.01)	(3.71)	(-0.50)	(4.65)
Precision	0.08***			0.09***	0.06***		
	(11.83)			(9.51)	(5.26)		
Informed	0.35***	0.42***	0.50***				
	(8.35)	(5.50)	(6.49)				
Observations	20,571	8,199	9,552	9,132	8,720	4,433	4,931
R^2	0.978	0.981	0.973	0.978	0.970	0.980	0.965
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm	Firm	Firm	Firm
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table presents regression summary statistics for the estimation of Equation (8) and various versions thereof. *t*-statistics are presented in parentheses. All continuous variables are winsorized at the 1st and 99th percentiles. The superscript asterisks *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 9. Path analysis

Variable	*Liquidity*	
Precision Channel		
λ_1[Treat×Post, Precision]	0.107***	
	(7.70)	
β_2[Precision, Liquidity]	0.084***	
	(11.87)	
Total precision channel	0.009	
Precision channel percentage		45%
Comparability Channel		
θ_1[Treat×Post, Informed]	0.030***	
	(3.60)	
β_3[Informed, Liquidity]	0.360***	
	(8.64)	
Total comparability channel	0.011	
Comparability channel percentage		55%
Direct channel	0.009	
β_1[Treat×Post, Liquidity]	(1.13)	
Sum of all significant channels	0.020	
Total percentage		100%
Observations	20,591	
Firm FE	Yes	
Month-year FE	Yes	
Cluster	Firm	

This table presents regression summary statistics for the path analysis estimation (Equations (2), (3), and (8)). *t*-statistics are presented in parentheses. All continuous variables are winsorized at the 1st and 99th percentiles. The superscript asterisks *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

APPENDIX 1. VARIABLE DEFINITIONS

Amihud	The negative of the natural log of the quarterly median of the daily Amihud (2002) measure of illiquidity, calculated using the unsigned stock return divided by USD trading volume.
Bid_ask	The negative of the natural log of the quarterly median of the daily quoted spreads, calculated using the daily closing bid and ask prices divided by the midpoint.
Liquidity	A common factor that explains common variation between *Amihud* and *Bid_ask*.
Liquidity_variable	Either *Amihud*, *Bid_ask*, or *Liquidity*.
Treat×Post	Equals one if the quarterly or annual report has been prepared using the new revenue standard, zero otherwise.
$SD\_returns_{t-4}$	The natural log of the quarterly standard deviation of daily returns, lagged by one year.
$Size_{t-4}$	The natural log of the market value of equity, lagged by one year.
$Turnover_{t-4}$	The natural log of the quarterly median of daily turnover, calculated as the volume of shares traded divided by the number of shares outstanding, lagged by one year.
ROA	Income before extraordinary items scaled by assets.
MB	Market value of equity to book value of equity.
Leverage	Book value of total debt to book value of total assets.
Q_return	Buy and hold returns over the quarter.
Loss	An indicator equal to one if the firm is a loss firm.
SI	Special items scaled by total assets.
Analyst	The number of analysts providing revenue forecasts.
Precision_residual	The natural log of the reciprocal of the standard deviation of the residual from Equation (4).
Precision_returns	The natural log of the reciprocal of the standard deviation of daily returns over the quarter.
Precision	The common factor that explains common variation between *Precision_residual* and *Precision_returns*.

Informed	The natural log of the number of institutional investors.
Revenue	Revenue for the quarter.
Returns	Buy and hold over the same period as *Revenue*.
Before_all	An indicator variable equal to one for all periods before the implementation of the new revenue standard.
After_all	An indicator variable equal to one for all periods after the implementation of the new revenue standard.
$Before_{it-n}$ $(After_{it+n})$	The n periods before (after) the fiscal year-end.
Trend	The number of quarters relative to the reference period, i.e., the period before the implementation of the standard.
First_adopters	Firms with a December fiscal year-end and were, therefore, the first to implement the standard.
March2017 to December2018	Indicator variables that equal one if the fiscal quarter ends on that specific date.
NonDec	An indicator variable that equals one if the firm has a non-December fiscal year-end.
Material	An indicator variable that equals one if the firm was substantively affected by the implementation of the new revenue standard.
Num_sh	The number of shareholders.
Relative_size	The total size of the other firms relative to the size of firm *i*: $\frac{\sum_{i=1}^{n} Num\_sh_{-i}}{Num\_sh_i}$.
Alt_Comp	An alternative measure of comparability calculated as $\frac{1}{J} \times \sum_{j=1}^{J}\left|\beta_{1i\omega} - \beta_{1j\omega}\right|$, where $\beta_{1i\omega}$ $(\beta_{1j\omega})$ is the coefficient on *Returns* from estimating Equation (4) for industry *i* (*j*) in period ω, *J* is the number of industries excluding industry *i*, and ω represents one of two distinct periods, i.e., the pre- or post-implementation period.
Persistence	The coefficient on $Revenue_{t-1}$ when *Revenue* is regressed on $Revenue_{t-1}$.
Predictability	The negative of the natural log of the standard deviation of the residual from the *Persistence* equation.

Smoothness	The negative of the natural log of the standard deviation of *Revenue* divided by the standard deviation of cash flow from operations.

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