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ABSTRACT

We test and compare the effects of introduction of two new financial information technologies, EDGAR and XBRL, on well-known asset pricing anomalies often attributed to mispricing. EDGAR facilitates easier access to public accounting information about public firms; XBRL reduces the cost of processing such information. Using stacked difference-in-differences regressions, we find that both EDGAR and XBRL reduce mispricing for accounting-based anomalies but not for non-accounting-based anomalies. The economic magnitudes of the effects on accounting-based anomalies are similar for EDGAR and XBRL. These results suggest that both easier access to and less costly processing of public information enhance market efficiency.

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1 Introduction

In his classic formalization of the Efficient Markets Hypothesis (EMH), Fama (1970) offered a strict distinction between private information, known only to some, and public information, which is known costlessly to all. This dichotomy is reflected in results of the first event studies Ball and Brown (1968) and Fama et al. (1969), in which accounting or other information, viewed as public information, was hypothesized to be immediately and fully reflected in market prices.

This dichotomy blurs once it is recognized that there are pecuniary costs or other barriers to accessing and processing information—even information that is reported in business media. For example, when a firm in the 1960s announced a stock split, to discover such news promptly, investors generally had to pay to buy a business publication. Both then and now, only a subset of investors acquire any given information signal. Furthermore, time and attention is required to read and process financial disclosures or news about corporate events.

Nevertheless, the efficient markets hypothesis makes a strong claim: that if a signal is classified as "public" (i.e., when the cost of acquiring the signal is low), the signal is fully incorporated into price. Since Ball and Brown's classic study, earnings news has been classified as public information, so that post-announcement return predictability is anomalous from the perspective of the EMH.

In contrast, a signal that only a few investors can afford to buy is generally classified as private information. Even if returns are predictable after the arrival of such a signal, there is no violation of the EMH.

Formal modelling of settings with costly information acquisition does not support the idea that signals can meaningfully be classified in an absolute dichotomy of public versus private, with public signals fully incorporated into price and private signals only partly incorporated. Instead, the degree to which a signal is incorporated into price is decreasing with the cost of acquiring that signal. So, contra the EMH as applied in many event study tests, a reduction in the cost of acquiring information—even information that is commonly referred to as “public”—should cause that information to be more fully incorporated into price. This prediction follows immediately from a standard noisy rational expectations setting where ‘private’ signals could be ‘public’ information as reported on financial statements, since such information can be incorporated by investors only by incurring a cost (see Section 2).[1]

Furthermore, a similar prediction holds in a setting in which investors have limited attention, and therefore sometimes do not process available information signals. In such a setting, a technological change that makes it easier for investors to cognitively process a signal will cause it to be more fully incorporated into price (Hirshleifer and Teoh (2003)). For example, being able to obtain and manipulate an accounting number with a few clicks at any time rather than having it only in paper form reduces the cost of cognitive processing.

We perform here a test of the prediction that reducing the cost of acquiring a financial disclosure signal or of cognitively processing it causes the information in that signal to be incorporated more quickly and fully into price. To do so, we investigate the causal effect on anomaly mispricing of improved access to financial disclosure signals using two natural experiments: the staggered introduction of the EDGAR (Electronic Data Gathering, Analysis, and Retrieval) platform for corporate filings in 1993 and the mandate that requires corporate filings to be in an interactive data format known as XBRL (eXtensible Business Reporting Language) in 2009. The introduction of EDGAR made corporate filings electronically and readily available to investors. The mandatory adoption of XBRL lowered the processing cost of corporate filings by making them machine-readable. Therefore, EDGAR and XBRL can be viewed as an exogenous improvement in the degree of financial information access and the ease of financial information processing, respectively.

[1] In the model, investors incur a cost to obtain corporate filings, which contain accounting signals that are informative about future dividends. In equilibrium, some investors pay the cost and obtain corporate filings containing the relevant signals, while others do not pay the cost and remain uninformed. When both informed and uninformed investors are risk averse, the presence of uninformed investors leads to accounting information not being fully incorporated into price, resulting in observed return anomalies.

This reasoning suggests that the introduction of EDGAR and XBRL can weaken standard return anomalies that are widely attributed to mispricing. However, some relevant signals—for example, those based on market price—were easily available before the introduction of EDGAR or XBRL. In contrast, EDGAR and XBRL greatly improved the availability of other signals—those associated with financial statement information other than earnings.[2] Our first hypothesis is that the introduction of EDGAR weakens anomalies associated with signals that were troublesome to acquire and process before EDGAR for treated firms relative to control firms. For example, we expect the introduction of EDGAR to weaken the accrual anomaly, which requires financial statement information, for treated firms relative to control firms. Our second hypothesis is that treatment will not affect the strength of anomalies associated with signals that were easy to acquire and process even before EDGAR. For example, we do not expect the introduction of EDGAR to weaken the momentum anomaly, which does not require financial statement information, for treated firms relative to control firms. Similarly, we offer two additional hypotheses on the effect of mandatory XBRL adoption on anomalies. Our third hypothesis asserts that the introduction of XBRL weakens accounting anomalies. Our fourth hypothesis asserts that it does not reduce non-accounting anomalies.

Our tests are based on the most relevant subset of the 11 anomalies that are the focus of Stambaugh, Yu, and Yuan (2012), as they are often attributed to mispricing.[3] Among the remaining eight anomalies in the final set, five use annual accounting information, and we classify them as accounting anomalies. They include Accruals, Net Operating Assets, Investment to Assets, Asset Growth, and Gross Profitability. The other three anomalies use market information (such as stock returns, number of shares outstanding, and market capitalization), and we classify them as non-accounting anomalies. They include Momentum,

[2] *Barron*'s, a well-known financial periodical, regularly reports earnings information about firms.

[3] Consistent with mispricing, Chu, Hirshleifer, and Ma (2020) find that these anomalies become weaker once limits to arbitrage are alleviated. Also consistent with mispricing, Stambaugh, Yu, and Yuan (2012) find that these anomalies are stronger following times of higher investor sentiment. These anomalies are also the individual components that comprise the mispricing factors of Stambaugh and Yuan (2017). We omit two anomalies that use both accounting and market information (O-score and Failure Probability) and an anomaly that uses quarterly accounting information (Return on Assets).

Net Stock Issuance, and Composite Equity Issuance.[4]

The implementation of EDGAR was adopted in a staggered way over ten phase-in waves ranging from April 1993 to May 1996. Similarly, the mandatory adoption of XBRL was also staggered over three phase-in groups for fiscal year ends ranging from 2009 to 2011. So, a possible testing approach is the standard staggered difference-in-differences (DiD) regression method (Bertrand and Mullainathan (2003)) to examine the effects of EDGAR implementation and XBRL adoption on anomaly returns. However, as highlighted by recent research (e.g., Goodman-Bacon (2021) and Baker, Larcker, and Wang (2022)), the standard staggered DiD regression approach suffers from the problem of negative weighting when already-treated observations are used as controls, which introduces biases in the DiD estimates. To avoid this problem, we follow Cengiz et al. (2019), Baker, Larcker, and Wang (2022), and Chang et al. (2022) in using the stacked DiD regression approach combined with the use of "clean" controls—controls that do not become treated themselves during the treatment window. As further discussed in Section 4, our sample consists of monthly returns of two stacked groups of treated and control firms for the sample period of July 1992 to June 1997 (July 2009 to June 2012) for the EDGAR (XBRL) sample.

Our research design allows us to examine the effect of EDGAR/XBRL implementation on each of the anomalies individually. A more powerful test is provided of the effects of EDGAR and XBRL on the five accounting anomalies in aggregate, and similarly, the three non-accounting anomalies in aggregate. To this end, we construct an aggregate mispricing score using the *Net* measure developed by Engelberg, McLean, and Pontiff (2018) and do so for the five accounting and three non-accounting anomalies separately.

Our findings are consistent with the four hypotheses. Consistent with the first hypothesis, the estimated effect of EDGAR implementation on anomaly returns is negative for all five accounting anomalies and statistically significant for four of them, Accruals, Net Operating

[4] *Barron*'s regularly publishes stock-market-related information including stock prices (returns) and equity issuance (in the "Equity Financing" table). This makes access to this information easy and cheap to acquire regardless of the availability of EDGAR.

Assets, Investment to Assets, and Asset Growth. In aggregate, the estimated DiD coefficient is -0.40% per month, statistically significant. Consistent with the second hypothesis, the estimated effect of EDGAR implementation on anomaly returns is small in magnitude and statistically insignificant for all three non-accounting anomalies, both individually and in aggregate.

The results using the mandatory XBRL adoption are similar. Consistent with the third hypothesis, the estimated effect of XBRL adoption on anomaly returns is negative for four accounting anomalies and statistically significant for two of them, Investment to Assets, and Gross Profitability. In aggregate, the estimated DiD coefficient is -0.45% per month, statistically significant. Consistent with the fourth hypothesis, the estimated effect of XBRL adoption on anomaly returns is small in magnitude and statistically insignificant for all three non-accounting anomalies, both individually and in aggregate.

A further key finding from our analysis is that the economic magnitudes of the effects of EDGAR and XBRL on accounting anomalies are similar. This suggests that advances to modern information technology have also been essential in improving market efficiency, and that easing the processing of information can be just as important as easing its availability.

Overall, these results support the prediction that new information technologies can alleviate mispricing and diminish anomalies. Our empirical tests that exploit the implementation of EDGAR and XBRL offer causal evidence consistent with the effects of costs of information acquisition and processing, and the effects of limited investor attention and limited investor processing power on market efficiency. As such, our paper contributes to several strands of literature.

First, our study is part of the literature that examines the effect of the implementation of EDGAR and XBRL. Gao and Huang (2020) uses the staggered adoption of EDGAR as a natural experiment and shows that it increases information production by corporate outsiders, including individual investors and sell-side analysts. Chang et al. (2022) find that the EDGAR implementation reduces investor disagreement and stock crash risk. Chang, Ljungqvist, and Tseng (2022) find that EDGAR implementation constrains strategic analyst behavior. Goldstein, Yang, and Zuo (2022) examine the real effects of EDGAR implementation and find that it decreases the cost of capital, increases equity financing and corporate investment, and reduces managerial learning from prices. Notable studies that examine the effect of mandatory XBRL adoption include Dong et al. (2016), who find XBRL adoption reduces firms' stock return synchronicity, Bhattacharya, Cho, and Kim (2018), who find that XBRL adoption increases stock picking performance and responsiveness to 10-K filings in the 10-K filing period for small institutions relative to large institutions, and Kim, Kim, and Lim (2019), who find that XBRL adoption constrains earnings management. Our study goes further to show that the implementation of EDGAR and XBRL facilitates the incorporation of value-relevant accounting information into prices and reduces accounting-related anomalies.

In independent work, Ivkovich et al. (2024) also study the effect of EDGAR introduction on asset pricing anomalies. Our study differs in important ways. In addition to examining the introduction of EDGAR, we examine the effect of the introduction of XBRL on the same set of return anomalies using a similar empirical design. This enables a direct comparison between the effects of these two information technologies on accounting or non-accounting based anomalies. Our finding that XBRL reduces accounting anomalies with an economic magnitude similar to EDGAR highlights the importance of continuous development of new technologies in enhancing market efficiency and supports the SEC's advocacy of their continuous efforts along this direction (SEC (2009)). Furthermore, in our tests on both EDGAR and XBRL, we adopt a stacked difference-in-differences regression approach. This mitigates biases present in standard staggered difference-in-differences regressions (Goodman-Bacon (2021) and Baker, Larcker, and Wang (2022)). Finally, we apply our tests to a set of anomalies that have been proposed to be potentially behavioral in origin (Stambaugh, Yu, and Yuan (2012) and Chu, Hirshleifer, and Ma (2020)), to focus on how information technology affects mispricing.

Second, our paper adds to the literature on limited attention and its asset pricing implications (see, e.g., Hirshleifer and Teoh (2003), Peng and Xiong (2006), DellaVigna and Pollet (2009), Huang et al. (2022), deHaan, Shevlin, and Thornock (2015), and Lee et al. (2024)). Related to our paper, Hirshleifer, Lim, and Teoh (2011) find that neglect of earnings components due to limited attention can cause the accrual anomaly. Along this line, Blankespoor, deHaan, and Marinovic (2020) highlight the role of disclosure processing costs in shaping equity market outcomes, including market efficiency. Our findings are consistent with limited investor cognitive processing power affecting market prices, and show that new financial information technologies help alleviate this constraint and make markets more efficient.

Finally, our study also contributes to the literature on asset pricing anomalies. Several recent papers provide evidence consistent with mispricing as a source of many anomalies (see, e.g., the above-cited Stambaugh, Yu, and Yuan (2012) and Chu, Hirshleifer, and Ma (2020). McLean and Pontiff (2016) and Engelberg, McLean, and Pontiff (2018) find that many anomalies in aggregate become weaker after discovery and are concentrated on news days, consistent with mispricing. Our paper contributes to this literature by identifying one source of anomaly mispricing—the difficulty of accessing and processing publicly available accounting information.

The remainder of the paper is organized as follows. Section 2 presents a simple model of the effect of financial information technologies on anomaly mispricing. Section 3 provides background information on the implementation of EDGAR and XBRL. Section 4 discusses data, the sample, and anomalies. Section 5 presents the main empirical analysis and results. Section 6 concludes.

2 Model

We present a simple model of the effect of new financial information technologies on mispricing and anomalies.

We consider a one-period setup with two dates $t = \{0, 1\}$. There is a risk-free asset, and the risk-free rate is normalized to zero. There also exists a risky asset (stock), which liquidates at $t = 1$ and pays a dividend θ that follows a normal distribution: $\theta \sim \mathcal{N}(0, \sigma_\theta^2)$. The net supply of the risky asset is normalized to zero. There is a set of identical risk-averse investors, indexed by j and with initial wealth $W_{j0} > 0$. Each investor j selects his or her portfolio to maximize the expected negative exponential utility over his or the terminal wealth at $t = 1$, $\mathbb{E}_j[-e^{-\gamma W_{j1}}]$, where $\gamma > 0$ is the constant absolute risk aversion parameter. Denote the information set of investor j at $t = 0$ by I_j and denote the price of the risky asset at t by P_t. Intuitively, $P_1 = \theta$.

An anomaly variable s contains information about θ: $s = \theta + \epsilon$, where $\epsilon \sim \mathcal{N}(0, \sigma_\epsilon^2)$. This variable s is available at $t = 0$ in the corporate filings for the stock, which requires a cost of $c > 0$ to obtain or process. This cost could be a tangible resource opportunity cost or a psychological cost of allocating attention, in which case the anomaly has a behavioral source.[5] Denote the fraction of informed investors and that of uninformed investors in equilibrium by λ and $1 - \lambda$, respectively.

As shown in Appendix A.1, the expected return from $t = 0$ to $t = 1$ conditional on s is

$$\mathbb{E}[R_{0,1}|s] = [\frac{\sigma_\theta^2}{\sigma_\theta^2 + \sigma_\epsilon^2} - \frac{\frac{\lambda\sigma_\theta^2}{\sigma_\epsilon^2} + (1-\lambda)\frac{\sigma_\theta^2}{\sigma_\epsilon^2 + \frac{\gamma^2\sigma_\epsilon^4}{\lambda^2}\sigma_z^2}}{1 + \frac{\lambda\sigma_\theta^2}{\sigma_\epsilon^2} + (1-\lambda)\frac{\sigma_\theta^2}{\sigma_\epsilon^2 + \frac{\gamma^2\sigma_\epsilon^4}{\lambda^2}\sigma_z^2}}]s = f(\lambda)s, \qquad (1)$$

where $f(\lambda) = [\frac{\sigma_\theta^2}{\sigma_\theta^2+\sigma_\epsilon^2} - \frac{\frac{\lambda\sigma_\theta^2}{\sigma_\epsilon^2} + (1-\lambda)\frac{\sigma_\theta^2}{\sigma_\epsilon^2 + \frac{\gamma^2\sigma_\epsilon^4}{\lambda^2}\sigma_z^2}}{1 + \frac{\lambda\sigma_\theta^2}{\sigma_\epsilon^2} + (1-\lambda)\frac{\sigma_\theta^2}{\sigma_\epsilon^2 + \frac{\gamma^2\sigma_\epsilon^4}{\lambda^2}\sigma_z^2}}]$.

[5] On inattention as a source of anomalies, see the models of Hirshleifer and Teoh (2003) and Hirshleifer, Lim, and Teoh (2011).

It is easy to show that $\frac{\partial f(\lambda)}{\partial \lambda} < 0$ and $f(\lambda) > 0$ if $\lambda < 1$. This suggests that when some investors do not pay the cost of c and extract the signal s from corporate filings, there exists a return anomaly ex post.

It is straightforward to show that in equilibrium, a decrease in c induced by the new financial information technologies leads to an increase in λ. Given that the anomaly strength, $f(\lambda)$, decreases with λ, we have the following proposition.

PROPOSITION 1: *The new financial information technology reduces c, which leads to an increase in λ and, in turn, a decrease in anomaly strength.*

3 Background

3.1 Background on EDGAR Implementation

Before the implementation of EDGAR in 1993, firms subject to SEC registration were required to mail their mandatory filings in hardcopy to the SEC. To access these filings, investors had two options. First, they can physically visit one of the three SEC reference rooms, which are located in Chicago, New York, and Washington D.C., respectively.[6] Second, they can subscribe to commercial data vendors (such as Mead Data Central) at a substantial cost. Therefore, corporate filings (e.g., 10Ks) were costly to obtain at least for some investors although such filings might be viewed as "publicly available." The SEC launched the implementation of EDGAR in a phase-in program, with registered firms joining EDGAR in ten waves over three years starting on April 26, 1993, and ending on May 6, 1996. The SEC designates firms in these ten phase-in waves into groups labeled as "CF-01", "CF-02", ..., and "CF-10". The final phase-in dates for these ten groups are listed in the third column of Table 1.

[6] As discussed by a *New York Times* article in 1982 (https://www.nytimes.com/1982/05/19/business/sec-data-difficult-hunt.html), prior to the EDGAR implementation it was difficult and time-consuming to extract useful corporate filings information from the reference rooms.

For our analysis, the shock to the cost of acquiring corporate accounting information occurred when investors could easily access firms' filings on EDGAR via the internet, which did not happen immediately after the phase-in dates for firms in phase-in waves 1 through 4. Instead, for these firms, the date of shock is January 17, 1994, when through a project funded by the National Science Foundation, firms' EDGAR filings became available online, hosted by New York University (NYU), for free. For firms in phase-in waves 5 through 10, the dates of shock are the same as their phase-in dates. The dates of shock for these ten groups are listed in the fourth column of Table 1. As discussed in Chang et al. (2022), the assignment of firms into phase-in waves is random conditional on size with larger firms assigned to earlier EDGAR phase-in waves. This makes it important in empirical tests to select control firms matched to treated firms by size.

A natural question is whether investors had access to corporate information via other venues, such as media or press releases, to the extent the implementation of EDGAR did not play a crucial role in improving investors' information sets. For example, as discussed in DellaVigna and Pollet (2009), earnings announcements are (quickly) disseminated by the *Wall Street Journal*. However, it was not the case that the *Wall Street Journal* quickly accessed the hard copy of corporate filings and published relevant accounting information. For example, it was costly for regular investors to access information embedded in corporate filings in 1992, even one year before the phase-in implementation of EDGAR, according to a 1992 petition to the SEC signed by academics, librarians, and journalists.[7]

3.2 Background on XBRL Implementation

The SEC initiated the voluntary XBRL financial reporting program in 2005 (SEC (2005)) to assess the feasibility of XBRL financial reporting by U.S. firms. In the voluntary program, firms were allowed to decide whether and when to submit interactive financial reporting

[7]A table contained in the petition letter showed that the cost of acquiring corporate filings information via Mead Data Central costed "a fee of $125 per month, plus a connect charge of $39 an hour, plus a charge of 2.5 cents per line of data plus search charges which range from $6 to $51 per search."

data in XBRL format. Therefore, in this setting, firms' choice of adopting XBRL financial reporting could be endogenous, making it less ideal to study the potential causal effect of information technology on asset pricing anomalies.

Thus motivated, our empirical analysis builds on the mandatory program of XBRL implemented by the SEC in 2009 (SEC (2009)). In this final ruling, U.S. firms are required to submit financial reporting filings that are tagged according to standardized taxonomies in XBRL format.

Specifically, XBRL utilizes a set of standardized tags that map to various items within financial statements. This list of tags functions like a dictionary, where each tag represents a "word" and its corresponding economic definition. As a result, an XBRL filing transforms a financial statement into a format where each item is identified using these standard tags, making the document machine-readable.

The advantage of XBRL mandatory adoption lies in its standardization, making financial statements machine-readable. Consequently, market participants can avoid the inefficiencies associated with manual data collection and more efficiently process various data items directly through computer software. Consequently, the mandatory adoption of XBRL adoption aims to lower the information-processing costs for market participants (SEC (2009)). As discussed further in SEC (2009), requiring firms to file their statements using interactive data (XBRL) format will enable market participants to process information more quickly than using the same financial information provided in a static format. After the XBRL mandate, any investor with a computer and internet access will be able to obtain and download interactive financial data that were typically accessible only to large institutional users.

The XBRL mandatory program was implemented in a staggered phased-in program starting in 2009 with three phase-in groups (cohorts). As with the EDGAR program, larger firms were required to comply with the XBRL format earlier,[8] which makes it important to se-

[8] Specifically, firms with public float above $5 billion are required to submit XBRL filings for fiscal periods ending on or after June 15, 2009. Other large accelerated filers are required to submit XBRL filings for periods ending on or after June 15, 2010. Finally, all remaining filers are required to submit XBRL filings for periods

lect control firms matched to treated firms by size for empirical analysis using the XBRL mandatory adoption setting.

4 Data and Sample

4.1 Sample of Treated and Control Firms: EDGAR

We obtain the list of firms assigned to the ten phase-in waves from SEC Release No. 33-7122 and merge this list of firms with data from Compustat and the Center for Research in Security Prices (CRSP) by CIK to construct our initial sample. As standard in the literature, we keep common stocks traded on NYSE, Amex and NASDAQ, as of the month prior to the phase-in date. Table 1 presents the distribution of firms across the ten phase-in waves in this initial sample and the average market capitalization for each phase-in wave.

In our analysis, the shock to the public availability of a firm's accounting information occurs when the firm's filings become available to investors via online access to EDGAR. The third column of Table 1 lists the dates of shock for firms in different phase-in waves. For firms in phase-in waves 5 to 10, the date of shock is the same as the phase-in date. For firms in phase-in waves 1 to 4, the date of shock is January 17, 1994, when online access to EDGAR went live, hosted by NYU.

It is noteworthy that firms in the same phase-in wave can have different treatment statuses even on the same date if their fiscal year-ends are in different months. For example, consider two firms in phase-in wave 8 and their filings associated with fiscal year-ends in 1995. Firm A has a fiscal year-end on June 30, 1995, and firm B has a fiscal year-end on September 30, 1995. Since the phase-in date for both of these two firms is August 7, 1995, firm A's 1995 filing is not subject to EDGAR requirement, while firm B's 1995 filing is. At some point in 1996, when investors formed potential anomaly trading strategies using annual accounting

ending on or after June 15, 2011 (SEC (2009)).

information in 1995, investors had access to digital accounting information for firm B but not for firm A. To avoid this layer of complexity in analysis and for a cleaner test, we focus on annual accounting information and keep only observations with fiscal year-ends in December.[9]

For firms in each phase-in wave, Table 2 shows whether their accounting information in a particular year is available on EDGAR based on their dates of shock. For firms in phase-in waves 1 through 4, their electronic filings via EDGAR did not become publicly available until January 17, 1994, when online access to EDGAR went live. Therefore, these four groups of firms belong to the first cohort, for which the effective treatment date is January 1994. For this cohort, filings with fiscal year-ends in 1992 and before are not available on EDGAR, while filings with fiscal year-ends in 1993 and after are.

For firms in phase-in waves 5 through 9, filings with fiscal year-ends in 1994 and before are not available on EDGAR, while filings with fiscal year-ends in 1995 and after are. These five groups of firms belong to the second cohort, for which the effective treatment date is December 1995. For firms in phase-in wave 10, filings with fiscal year-ends in 1995 and before are not available on EDGAR, while filings with fiscal year-ends in 1996 and after are. This group of firms belongs to the third cohort.

As discussed in the introduction, to mitigate bias in the traditional staggered DiD regression approach, we follow the method of Cengiz et al. (2019), Baker, Larcker, and Wang (2022), and Chang et al. (2022) by combining with "clean" controls. For each cohort, we follow two fiscal years before treatment and two fiscal years after treatment.[10] Accordingly, the candidate controls for firms in the first cohort (phase-in waves 1 through 4) come from firms in phase waves 5 through 10. For this first set of treated and control firms, we follow

[9]This corresponds to the majority of the observations. In the sample of common stocks traded on NYSE, AMEX, and Nasdaq and included in the EDGAR phase-in program, more than 57% of observations have fiscal year-ends in December from 1993 to 1996, when the EDGAR phase-in program took place.

[10]The choice of following two years before and after treatment follows the standard in the literature, e.g., in Gao and Huang (2020) and Chang et al. (2022), which also study the effect of the EDGAR implementation with either a standard staggered DiD approach or a stacked DiD approach.

them over the fiscal years of 1991 to 1994. For firms in the second cohort (phase-in waves 5 through 9), their controls come from firms in phase wave 10, and they are followed only for one fiscal year (1995) after treatment because the control firms became treated as well in the second fiscal year (1996). For this second set of treated and control firms, we follow them over the fiscal years of 1994 to 1996.

We are then left with two effective treatment event dates: January 1994 for firms in phase-in waves 1 through 4 and December 1995 for firms in phase-in waves 5 through 9. As discussed in Chang et al. (2022), the assignment of firms to EDGAR phase-in firms is random conditional on size. We therefore choose control firms that are similar in size to treated firms.

At each of the two treatment dates, we estimate a nearest-neighbor propensity score model to find control firms that match treated firms on equity market capitalization in both levels and logs. We consider only matches in the common support to be valid using a caliper of 0.05. As discussed above, we keep only firms with fiscal year ends in December in this propensity matching procedure. Also as standard in the literature, we exclude financial and utility firms.

Panel A of Table 3 lists the distribution of firms in the final matched EDGAR sample. There are 691 treated firms and 691 control firms in the final sample, which are used in subsequent empirical analysis. As discussed earlier, the fiscal years for the sample firms range from 1991 through 1996. We lag accounting information by six months as is standard in the literature (e.g., Fama and French (1992)). Therefore, the EDGAR sample contains a panel of monthly returns for the two sets of treated and control firms, and the return sample period for the main analysis is from July 1992 to June 1997.

4.2 Sample of Treated and Control Firms: XBRL

We download the SEC XBRL index files and use them to identify 10-K filings that are in XBRL format. By doing so, we obtain the timing information of the first-time (initial) XBRL 10-K filing for each firm, including information on the initial filing date and corresponding fiscal year end. Our empirical design focuses on the mandatory XBRL program; therefore, we exclude firms with fiscal year ends in their initial XBRL 10-K filings before June 15, 2009, as these come from the voluntary filers. After this process, we obtain a list of firms in the mandatory XBRL program with the timing information of their initial XBRL filings.

We then merge this list of firms with data from Compustat and CRSP by CIK to construct our initial sample and keep common stocks traded on NYSE, Amex, and NASDAQ. Consistent with the EDGAR sample, we focus on annual accounting information and only include observations with fiscal year ends in December. We then partition this sample into three groups: Group 1 with fiscal year ends in December 2009, Group 2 with fiscal year ends in December 2010, and Group 3 with fiscal year ends in December 2011.

Since Group 3 is the last group to comply with XBRL reporting, there are no control firms for them. As a result, there are essentially two treatment events, December 2009 and December 2010, corresponding to two cohorts of treated and control firms. Similar to the EDGAR sample, we select "clean" control firms for each treated cohort from the set of future treated firms. Specifically, control firms for treated firms in Group 1 come from Group 2 and Group 3, while control firms for treated firms in Group 2 come from Group 3. At each of the two treatment dates, we estimate a nearest-neighbor propensity score model to find control firms that match treated firms on equity market capitalization in both levels and logs, and we exclude financial and utility firms in this process.

Panel B of Table 3 lists the distribution of firms in the final matched XBRL sample. There are 252 treated firms and 252 control firms in the final sample, which are used in subsequent empirical analysis. Since the second treatment date is one year after the first one, we keep one fiscal year before and after the treatment date for each cohort to keep controls "clean", i.e., not becoming treated later in the sample. As a result, the fiscal year ends for the sample firms range from December 2008 to December 2010. With a six-month lag for accounting information, the XBRL sample contains a panel of monthly returns for the two sets of treated and control firms, covering the sample period from July 2009 to June 2012.

4.3 Anomalies

As stated in the introduction, we choose a set of anomalies that are widely attributed to mispricing and start with the 11 anomalies in Stambaugh, Yu, and Yuan (2012), but omit two anomalies that use both accounting and market information (O-score and failure probability). As outlined in Section 4.1, we focus on annual accounting information with fiscal year-ends in December, so we also drop an anomaly (return on assets) that uses quarterly accounting information. Out of the remaining eight anomalies, five—Net Operating Assets, Accruals, Investment to Assets, Asset Growth, and Gross Profitability—use accounting information, and we classify them as accounting anomalies. The other three anomalies—Momentum, Net Stock Issues, and Composite Equity Issues—use market information easily obtained, e.g., from *Barron*'s, such as stock returns, number of shares outstanding, and market capitalization, and we classify them as non-accounting anomalies.

Table 4 lists these eight anomalies, and Appendix A.2 provides the details on the firm characteristics used to construct the return predictors. All anomaly return predictors are signed (using the signs in the third column of Table 4) such that a higher anomaly variable is associated with higher subsequent average returns according to the original publication. Here we investigate the average anomaly returns in the EDGAR and XBRL samples. The purpose of this analysis is to verify the existence of anomalies in the sample and provide a benchmark for the effects of EDGAR implementation and XBRL adoption on anomalies.

A caveat is that both samples are relatively short, so the point estimates of mean anomaly returns are noisy.

As discussed above, some firm-month observations can appear repeatedly (in different cohorts) in both EDGAR and XBRL samples. In this analysis, we only keep one instance of these firm-month observations. For both EDGAR and XBRL samples, at the beginning of each month t, we sort all (treated and control) firms into quintiles according to a particular anomaly variable and define an aggregate anomaly variable Net_{it}, which is the sum of the numbers of long-side and short-side anomaly quintile portfolios firm i belongs to at the beginning of month t, as in Engelberg, McLean, and Pontiff (2018). We calculate Net_{it} for accounting and non-accounting anomalies separately. We then run the following set of pooled panel regressions

$$R_{it} = \beta_1 Net_{it} + \gamma_t + \epsilon_{it}, \tag{2}$$

where R_{it} is monthly return of firm i in month t and γ_t represents time (month) fixed effects.

In these panel regressions, we keep all observations for control firms but only observations before EDGAR implementation or XBRL adoption for treated firms. The β_1 estimate is a measure of the average anomaly return in aggregate before firms' filings are available on EDGAR or required to be in XBRL format, i.e., prior to treatment. We cluster standard errors by firm.

Columns (1) and (2) in Table 5 show the results for accounting and non-accounting anomalies using the EDGAR sample. The coefficient on Net is positive and significant for both accounting and non-accounting anomalies. In terms of magnitude, an increase of one in Net score increases subsequent monthly stock returns by 0.36% (0.34%) for accounting (non-accounting) anomalies.

Columns (3) and (4) of Table 5 report the results for accounting and non-accounting anomalies using the XBRL sample. The coefficient on Net is positive and statistically significant for both accounting and non-accounting anomalies. In terms of magnitude, an increase of one in the Net score increases subsequent monthly stock returns by 0.25% (0.22%) for accounting (non-accounting) anomalies.

5 Empirical Analysis

As described in the introduction and based in part on Proposition 1, our hypotheses are as follows.

HYPOTHESIS 1: *For mispricing-driven accounting anomalies, the implementation of EDGAR reduces the anomaly returns for treated stocks relative to control stocks.*

HYPOTHESIS 2: *For mispricing-driven non-accounting anomalies, the implementation of EDGAR does not affect the anomaly returns for treated stocks relative to control stocks.*

HYPOTHESIS 3: *For mispricing-driven accounting anomalies, mandatory adoption of XBRL reduces the anomaly returns for treated stocks relative to control stocks.*

HYPOTHESIS 4: *For mispricing-driven non-accounting anomalies, mandatory adoption of XBRL does not affect the anomaly returns for treated stocks relative to control stocks.*

5.1 Results from EDGAR

Since there are two treatment events, there are two cohorts of treated firms, firms in phase-in waves 1 through 4 and firms in phase-in waves 5 through 9. We select "clean" control firms for each treated cohort from the set of future treated firms. For each cohort g of treated and control firms, we create a dataset that contains data for these firms. We then stack the cohort-specific datasets together. For each cohort g, at the beginning of each month t, we sort all (treated and control) firms into quintiles according to a particular anomaly variable and denote the quintile firm i from cohort g belongs to by Qtl_{igt}. Similar to Section 4.3, we also define an aggregate anomaly variable Net_{igt}, which is the sum of the numbers of long-side and short-side anomaly quintile portfolios firm i from cohort g belongs to at the beginning of month t. We calculate Net_{igt} for accounting and non-accounting anomalies separately.

We estimate the following set of stacked DiD regressions

$$
\begin{aligned}
R_{igt} =& \beta_1 Qtl_{igt} \times POST\_EDGAR_{igt} + \beta_2 POST\_EDGAR_{igt} + \beta_3 Qtl_{igt} \times Treated_{ig} \\
& + \beta_4 Treated_{ig} + Qtl_{igt} \times TimeFE + TimeFE + \epsilon_{igt}, \qquad (3) \\
R_{igt} =& \beta_1 Net_{igt} \times POST\_EDGAR_{igt} + \beta_2 POST\_EDGAR_{igt} + \beta_3 Net_{igt} \times Treated_{ig} \\
& + \beta_4 Treated_{ig} + Net_{igt} \times TimeFE + TimeFE + \epsilon_{igt}, \qquad (4)
\end{aligned}
$$

where R_{igt} is the return of firm i from cohort g in month t, $POST\_EDGAR_{igt}$ is a dummy variable that equals one if the corresponding annual accounting information for R_{igt} is available to investors on EDGAR via online access,[11] $Treated_{ig}$ is a dummy variable that equals one if firm i is a treated firm in cohort g, and $TimeFE$ denotes time (month) fixed effects, which capture the common factors (and/or macroeconomic variables) that drive stock returns for all firms. The interaction terms $Qtl \times TimeFE$ and $Net \times TimeFE$ control for the potential variations of anomaly magnitudes (individually and in aggregate) over time.[12] The stand-alone terms of Qtl and Net are subsumed by the two interaction terms $Qtl \times TimeFE$ and $Net \times TimeFE$, respectively. They are, therefore, dropped from the regressions. In these regressions, the unit of analysis is a firm-month observation. We cluster standard errors by firm, given that the implementation of EDGAR is a firm-level shock.

The DiD coefficient associated with $Qtl \times POST\_EDGAR$ in equations (3) and (4), β_1,

[11] For example, consider two firms in the first cohort. One is from phase-in wave 4 (CF-04), and the other is from phase-in wave 5 (CF-05). The returns of these two firms in July 1995 correspond to their annual corporate filings with fiscal year-ends in December 1994. For the former (latter) firm, $POST\_EDGAR = 1$ ($POST\_EDGAR = 0$) as its corporate filing is (not) available to investors on EDGAR via online access.

[12] For example, it is well known that momentum profits vary over time (see, e.g., Daniel and Moskowitz (2016) and Ma (2022)) and the momentum strategy sometimes crashes (Daniel and Moskowitz (2016)). The time-varying variables associated with ex ante fluctuations in anomaly returns are controlled for by these interaction terms.

is the main coefficient of interest and measures the difference in changes in anomaly strength for treated versus control firms due to EDGAR implementation. Hypothesis 1 predicts that β_1 is negative for accounting anomalies, and Hypothesis 2 predicts that β_1 is around zero for non-accounting anomalies.

Table 6 reports estimation results for accounting anomalies. The estimated coefficient β_1 associated with $Qtl \times POST\_EDGAR$ is negative for all five anomalies and significant for four of them: Accruals, Net Operating Assets, Investment to Assets, and Asset Growth. For these anomalies, the magnitude of β_1 is economically sizable. It ranges from 0.31% for Net Operating Assets and Investment to Assets to 0.51% for Asset Growth. These magnitudes are comparable to the magnitude of mean anomaly returns before treatment in Panel A of Table 5. In aggregate, the estimated coefficient β_1 associated with $Net \times POST\_EDGAR$ is negative and both statistically and economically significant. The magnitude of β_1 is 0.40% per month, also comparable to that of the mean aggregate anomaly returns before treatment in Panel A of Table 5. These results show that accounting anomalies became substantially weaker for treated firms than control firms after the implementation of EDGAR. This supports Hypothesis 1 and shows that modern financial information technology alleviates accounting-related anomaly mispricing.

Table 7 reports estimation results for non-accounting anomalies. The estimated coefficient β_1 associated with $Qtl \times POST\_EDGAR$ is small in magnitude for all three anomalies and has mixed signs. These coefficients are also statistically insignificant. In aggregate, the estimated coefficient β_1 associated with $Net \times POST\_EDGAR$, 0.09% ($t = 0.48$), is also small in magnitude and statistically insignificant. These results show that non-accounting anomalies are not significantly affected by the implementation of EDGAR. This supports Hypothesis 2 and shows that modern financial information technology does not affect non-accounting-related anomaly mispricing. This test also serves as a placebo test, alleviating concerns that the DiD results for accounting anomalies can be driven by other unobservable shocks (than the implementation of EDGAR), which affect treated and control firms differently. If unobservable shocks affect non-accounting anomalies similarly to their effect on accounting anomalies, then we would expect to see a similar effect of unobservable shocks for the non-accounting anomalies. Such an effect is not observed.

5.2 Results from XBRL

We conduct a stacked difference-in-differences analysis using the mandatory XBRL program similar to that performed for the EDGAR analysis. For each cohort g of treated and control firms, we create a dataset that contains data for these firms. We then stack the cohort-specific datasets together. For each cohort g, at the beginning of each month t, we sort all (treated and control) firms into quintiles according to a particular anomaly variable and denote the quintile firm i from cohort g belongs to by Qtl_{igt}. We also define an aggregate anomaly variable Net_{igt}, which is the sum of the numbers of long-side and short-side anomaly quintile portfolios firm i from cohort g belongs to at the beginning of month t. We calculate Net_{igt} for accounting and non-accounting anomalies separately.

We estimate the following set of stacked DiD regressions

$$\begin{aligned} R_{igt} = &\beta_1 Qtl_{igt} \times POST\_XBRL_{igt} + \beta_2 POST\_XBRL_{igt} + \beta_3 Qtl_{igt} \times Treated_{ig} \\ &+ \beta_4 Treated_{ig} + Qtl_{igt} \times TimeFE + TimeFE + \epsilon_{igt}, \end{aligned} \tag{5}$$

$$\begin{aligned} R_{igt} = &\beta_1 Net_{igt} \times POST\_XBRL_{igt} + \beta_2 POST\_XBRL_{igt} + \beta_3 Net_{igt} \times Treated_{ig} \\ &+ \beta_4 Treated_{ig} + Net_{igt} \times TimeFE + TimeFE + \epsilon_{igt}, \end{aligned} \tag{6}$$

where R_{igt} is the return of firm i from cohort g in month t, $POST\_XBRL_{igt}$ is a dummy variable that equals one if the corresponding annual accounting information for R_{igt} is from a 10-K filing in XBRL format, $Treated_{ig}$ is a dummy variable that equals one if firm i is a treated firm in cohort g, and $TimeFE$ denotes time (month) fixed effects, which capture the common factors (and/or macroeconomic variables) that drive stock returns for all firms. The interaction terms $Qtl \times TimeFE$ and $Net \times TimeFE$ control for the potential variations of anomaly magnitudes (individually and in aggregate) over time. The stand-alone terms of Qtl and Net are subsumed by the two interaction terms $Qtl \times TimeFE$ and $Net \times TimeFE$, respectively. They are, therefore, dropped from the regressions. In these regressions, the unit of analysis is a firm-month observation. We cluster standard errors by firm, given that the implementation of XBRL is a firm-level shock.

The DiD coefficient associated with $Qtl \times POST\_XBRL$ in equations (5) and (6), β_1, is the main coefficient of interest and measures the difference in changes in anomaly strength for treated versus control firms due to XBRL adoption. Hypothesis 3 predicts that β_1 is negative for accounting anomalies, and Hypothesis 4 predicts that β_1 is around zero for non-accounting anomalies.

Table 8 reports estimation results for accounting anomalies. The estimated coefficient β_1 associated with $Qtl \times POST\_XBRL$ is negative for four anomalies and significant for two of them: Investment to Assets, and Gross Profitability. For these anomalies, the magnitude of β_1 is economically sizable. In aggregate, the estimated coefficient β_1 associated with $Net \times POST\_XBRL$ is negative and both statistically and economically significant. The magnitude of β_1 is 0.45% per month. These results show that accounting anomalies became substantially weaker for treated firms than control firms after the implementation of XBRL. This supports Hypothesis 3 and shows that modern financial information technology alleviates accounting-related anomaly mispricing.

Table 9 reports estimation results for non-accounting anomalies. The estimated coefficient β_1 associated with $Qtl \times POST\_XBRL$ is small in magnitude for all three anomalies and has mixed signs. These coefficients are also statistically insignificant. In aggregate, the estimated coefficient β_1 associated with $Net \times POST\_XBRL$, -0.07% ($t = -0.33$), is also small in magnitude and statistically insignificant. These results show that non-accounting anomalies are not significantly affected by the implementation of XBRL. This supports Hypothesis 4 and shows that modern financial information technology does not affect non-accounting-related anomaly mispricing.

As discussed earlier, the test on non-accounting anomalies also serves as a placebo test, alleviating concerns that the DiD results for accounting anomalies can be driven by unobserved shocks which affect treated and control firms differently.

6 Conclusion

We examine and compare the effects of the introduction of two modern financial information technologies on anomaly mispricing. We use the staggered implementation of the EDGAR platform, which made accounting information readily accessible for treated firms, and the staggered mandatory adoption of XBRL financial reporting, which lowered the information-processing costs, as two natural experiments. Accounting information was publicly available prior to the introduction of EDGAR, but was not costlessly available. Similarly, most of accounting information was not machine-readable until after the mandatory adoption of XBRL.

We provide here a test of the effects of increasing information availability and ease of processing on prices. As such, we provide a test for the effects of models of costly information acquisition and processing and of limited investor attention. We study a set of accounting-based and non-accounting anomalies that are widely attributed to mispricing.

Using similar empirical designs based on stacked difference-in-differences regressions, we find that both EDGAR implementation and XBRL adoption reduced anomaly returns substantially for accounting anomalies but did not significantly weaken non-accounting anomalies. This evidence is consistent with the hypothesis that better information technologies increase information availability and ease of processing public information, thereby making markets more efficient. Furthermore, the economic magnitudes of the effects of accounting-based anomalies are similar for EDGAR and XBRL. This underscores the importance of the continuing evolution of modern information technologies in enhancing market efficiency.

A Appendix

A.1 Details of the Model

Given investor j's demand $x_j(I_j)$, his or her terminal wealth at $t = 1$ is

$$W_{j1} = W_{j0} - P_0 x_j(I_j) + \theta x_j(I_j). \tag{A.1}$$

Investor j's optimization problem is then

$$\begin{aligned} Max_{x_j} - \mathbb{E}_j[e^{-\gamma W_{j1}}] =& Max_{x_j} - \mathbb{E}[e^{-\gamma(W_{j0} - P_0 x_j(I_j) + \theta x_j(I_j))}] \\ =& Max_{x_j} - [e^{-\gamma(W_{j0} - P_0 x_j(I_j)) - \gamma x_j(I_j)\mathbb{E}[\theta|I_j] + \frac{1}{2}\gamma^2 x_j^2(I_j) Var(\theta|I_j)}], \end{aligned} \tag{A.2}$$

where the last equality is due to θ following a normal distribution.

Taking the first-order condition of equation (A.2) with respect to x_j results in the following equation for investor j's demand of the risky asset at $t = 0$ given his or her information set I_j:

$$x_j(I_j) = \frac{\mathbb{E}[\theta|I_j] - P_0}{\gamma Var(\theta|I_j)}. \tag{A.3}$$

For (informed) investors that pay the cost of c to obtain the anomaly variable signal s, their information set I_j is $\{s\}$. By the standard rule of Bayesian updating for normal variables, we have $\mathbb{E}[\theta|s] = \frac{\sigma_\theta^2}{\sigma_\theta^2+\sigma_\epsilon^2} s$ and $Var(\theta|s) = \frac{\sigma_\theta^2 \sigma_\epsilon^2}{\sigma_\theta^2+\sigma_\epsilon^2}$. Their demand is then $x_I(s) = \frac{\frac{\sigma_\theta^2}{\sigma_\theta^2+\sigma_\epsilon^2} s - P_0}{\gamma \frac{\sigma_\theta^2 \sigma_\epsilon^2}{\sigma_\theta^2+\sigma_\epsilon^2}}$.

For (uninformed) investors that do not pay the cost of c, they can learn about θ from the asset price. Denote their demand by $x_U(P_0)$. However, there exists a random noise-trading demand $z \sim \mathcal{N}(0, \sigma_z^2)$, which prevents the asset price from fully revealing.

We use the standard technique of conjecturing a linear pricing function $P_0 = bs + b\frac{\gamma\sigma_\epsilon^2}{\lambda} z$, which can be rearranged as $\frac{P_0}{b} = \theta + \epsilon + \frac{\gamma\sigma_\epsilon^2}{\lambda} z$. Thus, the price signal $\frac{P_0}{b}$ is θ plus a noise term with variance $\sigma_\epsilon^2 + \frac{\gamma^2\sigma_\epsilon^4}{\lambda^2}\sigma_z^2$. The uninformed investors' demand is then

$$x_U(P_0) = \frac{\mathbb{E}[\theta|P_0] - P_0}{\gamma Var(\theta|P_0)} = \frac{\frac{1}{b}\frac{\sigma_\theta^2}{\sigma_\theta^2+\sigma_\epsilon^2+\frac{\gamma^2\sigma_\epsilon^4}{\lambda^2}\sigma_z^2}P_0 - P_0}{\gamma\frac{\sigma_\theta^2(\sigma_\epsilon^2+\frac{\gamma^2\sigma_\epsilon^4}{\lambda^2}\sigma_z^2)}{\sigma_\theta^2+\sigma_\epsilon^2+\frac{\gamma^2\sigma_\epsilon^4}{\lambda^2}\sigma_z^2}}. \tag{A.4}$$

Using the market-clearing condition $\lambda x_I(s) + (1-\lambda)x_U(P_0) + z = 0$ and by matching coefficients, we have

$$P_0 = \frac{\frac{\lambda\sigma_\theta^2}{\sigma_\epsilon^2} + (1-\lambda)\frac{\sigma_\theta^2}{\sigma_\epsilon^2+\frac{\gamma^2\sigma_\epsilon^4}{\lambda^2}\sigma_z^2}}{1+\frac{\lambda\sigma_\theta^2}{\sigma_\epsilon^2} + (1-\lambda)\frac{\sigma_\theta^2}{\sigma_\epsilon^2+\frac{\gamma^2\sigma_\epsilon^4}{\lambda^2}\sigma_z^2}}(s+\frac{\gamma\sigma_\epsilon^2}{\lambda}z). \tag{A.5}$$

The expected return from $t=0$ to $t=1$ conditional on s is

$$\mathbb{E}[R_{0,1}] = \mathbb{E}[\theta - P_0|s]. \tag{A.6}$$

Substituting $\mathbb{E}[\theta|s = \frac{\sigma_\eta^2}{\sigma_\eta^2+\sigma_\theta^2}s$, $\mathbb{E}[z] = 0$, and equation (A.5) into equation (A.6) gives equation (1).

A.2 Anomaly Variables

The data to construct anomaly variables come from CRSP and annual Compustat.

A.2.1 Accounting Anomalies

Anomaly 1: Accruals (AC). Our main sample period starts after 1988, when the data from the statement of cash flows became available. Therefore, following Hou, Xue, and Zhang (2015), we calculate operating accruals in fiscal year t, AC_t, as net income NI_t minus net cash flow from operations $OANCF_t$, scaled by lagged total assets: $AC_t = (NI_t - OANCF_t)/AT_{t-1}$. We match AC with fiscal years ending in calendar year t with stock returns from July of year $t+1$ to June of year $t+2$.

Anomaly 2: Net Operating Assets (NOA). Following Hirshleifer et al. (2004), we calculate net operating assets in fiscal year t, NOA_t, as the difference between operating assets and operating liabilities, scaled by lagged total assets: $NOA_t = (Operating\ Assets_t - Operating\ Liabilities_t)/AT_{t-1}$, where $Operating\ Assets = Total\ Assets\ (AT) - Cash\ and\ Short\text{-}Term\ Investment\ (CHE)$, and $Operating\ Liabilities = Total\ Assets\ (AT) - Short\text{-}Term\ Debt\ (DLC) - Long\text{-}Term\ Debt\ (DLTT) - Minority\ Interests\ (MIB) - Preferred\ Stocks\ (PSTK) - Common\ Equity\ (CEQ)$. We match NOA with fiscal years ending in calendar year t with stock returns from July of year $t+1$ to June of year $t+2$.

Anomaly 3: Investment to Assets (IVA). Following Lyandres, Sun, and Zhang (2008), we calculate investment to assets in fiscal year t, IVA_t, as the annual change in gross property, plant, and equipment plus the annual change in inventories, scaled by lagged total assets: $IVA_t = (PPEGT_t - PPEGT_{t-1} + INVT_t - INVT_{t-1})/AT_{t-1}$. We match IVA with fiscal years ending in calendar year t with stock returns from July of year $t+1$ to June of year $t+2$.

Anomaly 4: Asset Growth (AG). Following Cooper, Gulen, and Schill (2008), we calculate asset growth in fiscal year t, AG_t, as the change in total assets scaled by lagged total assets: $AG_t = (AT_t - AT_{t-1})/AT_{t-1}$. We match AG with fiscal years ending in calendar year t with stock returns from July of year $t+1$ to June of year $t+2$.

Anomaly 5: Gross Profitability (GP). Following Novy-Marx (2013), we calculate gross profitability in fiscal year t, GP_t, as the difference between total revenue and cost of goods sold, scaled by total assets: $GP_t = (REVT_t - COGS_t)/AT_t$. We match GP with fiscal years ending in calendar year t with stock returns from July of year $t+1$ to June of year $t+2$.

A.2.2 Non-Accounting Anomalies

Anomaly 6: Momentum (MOM). Following the standard momentum literature (e.g., Ma (2022)), a stock's past one-year momentum MOM_t at the end of month $t-1$ is calculated as the compounded return over the 11-month ranking period $t-12$ to $t-2$, skipping month $t-1$. We match MOM calculated at the end of month $t-1$ with stock returns in month t.

Anomaly 7: Net Stock Issues (NSI). Following Pontiff and Woodgate (2008), net stock issues on an annual basis are measured as the change in the natural logarithm of a firm's split-adjusted shares over the last year, $NSI_t = Ln(Adjusted\,Shares_t) - Ln(Adjusted\,Shares_{t-1})$, where $Adjusted\,Shares_t$ is the product of common shares outstanding ($CSHO_t$) and the adjustment factor ($AJEX_t$). We match NSI with fiscal years ending in calendar year t with stock returns from July of year $t+1$ to June of year $t+2$.

Anomaly 8: Composite Equity Issues (CEI). Following Daniel and Titman (2006), composite equity issues are measured over the past five years and are defined as the part of the growth in market equity not attributable to stock returns. Since our main analysis keeps observations with fiscal year-ends in December, we calculate CEI at the end of December of each year t, in line with the timing of most of the other anomalies. Specifically, we have $CEI_t = Ln(ME_t/ME_{t-5}) - r(t-5,t)$, ME_t is the market equity at the end of December of year t and ME_{t-5} is the market equity at the end of December of year $t-5$, while $r(t-5,t)$ is the cumulative log return of the stock from the end of December of year $t-5$ to the end of December of year t. We match CEI calculated at the end of December of year t with stock returns from July of year $t+1$ to June of year $t+2$.

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Table 1. Distribution of Firms Across EDGAR Phase-In Waves

This table shows for firms in each phase-in wave, whether their accounting information in a particular year is available on EDGAR. "Yes" means filings associated with the corresponding fiscal year end are available on EDGAR and "No" means otherwise.

Phase-in Wave No.	SEC group	Phase-in Date	Shock Date	No. of Firms	Mean Market Cap
1	CF-01	April 26, 1993	January 17, 1994	101	8451.6
2	CF-02	July 19, 1993	January 17, 1994	401	4378.5
3	CF-03	October 4, 1993	January 17, 1994	415	877.0
4	CF-04	December 6, 1993	January 17, 1994	599	308.3
5	CF-05	January 30, 1995	January 30, 1995	669	185.0
6	CF-06	March 6, 1995	March 6, 1995	571	83.4
7	CF-07	May 1, 1995	May 1, 1995	467	85.4
8	CF-08	August 7, 1995	August 7, 1995	254	65.1
9	CF-09	November 6, 1995	November 6, 1995	137	174.9
10	CF-10	May 6, 1996	May 6, 1996	935	336.9
All				4,549	820.7

Table 2. Availability of Accounting Information for the EDGAR Sample

This table shows for firms in each phase-in wave, whether their accounting information in a particular year is available on EDGAR. "Yes" means filings associated with the corresponding fiscal year end are available on EDGAR and "No" means otherwise.

Fiscal year-end	CF-01 to CF-04	CF-05 to CF-09	CF-10
Dec-1991	No	No	No
Dec-1992	No	No	No
Dec-1993	Yes	No	No
Dec-1994	Yes	No	No
Dec-1995	Yes	Yes	No
Dec-1996	Yes	Yes	Yes

Table 3. Firm Distribution in Matched Samples

This table reports the distribution of treated firms in the EDGAR sample (Panel A) and XBRL sample (Panel B) after the propensity-score matching process of selecting control firms. For both samples, we keep only firms with fiscal year ends in December in the propensity matching procedure and exclude financial and utility firms. For the EDGAR sample, there are two effective treatment dates: January 1994 for firms in phase-in waves 1 through 4 (Cohort 1) and December 1995 for firms in phase-in waves 5 through 9 (Cohort 2). For the XBRL sample, there are also two effective treatment dates: December 2009 (Group 1) and December 2010 (Group 2). At each of the two treatment dates, we estimate a nearest-neighbor propensity score model to find control firms that match treated firms on equity market capitalization in both levels and logs. We consider only matches in the common support to be valid using a caliper of 0.05. There are 691 treated firms and 691 control firms in the final EDGAR sample, and there are 252 treated firms and 252 control firms in the final XBRL sample.

Panel A: EDGAR Sample	
Group	No. of Firms
CF-01	11
CF-02	30
CF-03	90
CF-04	197
CF-05	167
CF-06	109
CF-07	54
CF-08	23
CF-09	10
All	691
Panel B: XBRL Sample	
Group	No. of Firms
Group 1	66
Group 2	186
All	252

Table 4. List of Anomalies

This table lists the eight anomalies we study. “Yes” indicates that the anomaly is an accounting anomaly, while “No” indicates that the anomaly is a non-accounting anomaly. A sign of 1 (-1) indicates that the anomaly variable is positively (negatively) associated with subsequent stock returns on average.

	Accounting Anomaly?	Sign	Original Publication
Accruals (AC)	Yes	-1	Sloan (1996)
Net Operating Assets (NOA)	Yes	-1	Hirshleifer et al. (2004)
Investment to Assets (IVA)	Yes	-1	Lyandres, Sun, and Zhang (2008)
Asset Growth (AG)	Yes	-1	Cooper, Gulen, and Schill (2008)
Gross Profitability (GP)	Yes	1	Novy-Marx (2013)
Momentum (MOM)	No	1	Jegadeesh and Titman (1993)
Net Stock Issues (NSI)	No	-1	Pontiff and Woodgate (2008)
Composite Equity Issues (CEI)	No	-1	Daniel and Titman (2006)

Table 5. Mean Anomaly Returns in the EDGAR and XBRL Samples

The table examines the mean anomaly returns in the EDGAR and XBRL samples for accounting and non-accounting anomalies, respectively. From both samples, we only keep one copy of observations for those firm-month observations that appear repeatedly (in different cohorts). The sample period is July 1992 to June 1997 for the EDGAR sample and July 2009 to June 2012 for the XBRL sample, respectively. At the beginning of each month t, we sort all (treated and control) firms into quintiles according to a particular anomaly variable and define an aggregate anomaly variable Net_{it}, which is the sum of the numbers of long-side and short-side anomaly quintile portfolios firm i belongs to at the beginning of month t. We calculate Net_{it} for accounting and non-accounting anomalies separately. We then run the pooled panel regression $R_{it} = \beta_1 Net_{it} + \gamma_t + \epsilon_{it}$ and report the coefficient β_1 for each sample and anomaly group. We cluster standard errors by firm.

	(1) EDGAR Accounting	(2) EDGAR Non-Accounting	(3) XBRL Accounting	(4) XBRL Non-Accounting
Net	0.36***	0.34***	0.25***	0.22**
	(6.31)	(3.71)	(3.10)	(2.02)
Time Fixed Effects	Yes	Yes	Yes	Yes
No. of Observations	35,443	35,443	8,555	8,555

Table 6. DiD Results for Accounting Anomalies: EDGAR

This table presents the stacked DiD results for accounting anomalies. For each cohort g, at the beginning of each month t, we sort all (treated and control) firms into quintiles according to a particular accounting anomaly variable and denote the quintile firm i from cohort g belongs to by Qtl_{igt}. We also define an aggregate anomaly variable Net_{igt}, which is the sum of the numbers of long-side and short-side anomaly quintile portfolios firm i from cohort g belongs to at the beginning of month t. We estimate the following set of stacked DiD regressions, $R_{igt} = \beta_1 Qtl_{igt} \times POST\_EDGAR_{igt} + \beta_2 POST\_EDGAR_{igt} + \beta_3 Qtl_{igt} \times Treated_{ig} + \beta_4 Treated_{ig} + Qtl_{igt} \times TimeFE + TimeFE + \epsilon_{igt}$, $R_{igt} = \beta_1 Net_{igt} \times POST\_EDGAR_{igt} + \beta_2 POST\_EDGAR_{igt} + \beta_3 Net_{igt} \times Treated_{ig} + \beta_4 Treated_{ig} + Net_{igt} \times TimeFE + TimeFE + \epsilon_{igt}$. We cluster standard errors by firm, given that the implementation of EDGAR is a firm-level shock.

	(1) AC	(2) NOA	(3) IVA	(4) AG	(5) GP	(6) Aggregate
$Qtl \times POST\_EDGAR$	-0.41**	-0.31*	-0.31**	-0.51***	-0.17	
	(-2.36)	(-1.86)	(-1.99)	(-2.98)	(-1.01)	
$Net \times POST\_EDGAR$						-0.40***
						(-2.80)
$POST\_EDGAR$	0.74	0.24	0.52	1.12**	0.10	-0.41*
	(1.43)	(0.47)	(1.01)	(2.08)	(0.18)	(-1.96)
$Treat$	-0.45	0.02	-0.20	-0.75*	-0.21	0.10
	(-1.14)	(0.06)	(-0.52)	(-1.84)	(-0.51)	(0.58)
$Qtl \times Treat$	0.24*	0.09	0.07	0.28**	0.13	
	(1.82)	(0.67)	(0.51)	(1.98)	(1.05)	
$Net \times Treat$						0.20*
						(1.80)
$Qtl\times$ Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
No. of Observations	52,839	47,776	51,983	52,899	53,025	53,061

Table 7. DiD Results for Non-Accounting Anomalies: EDGAR

This table presents the stacked DiD results for non-accounting anomalies. For each cohort g, at the beginning of each month t, we sort all (treated and control) firms into quintiles according to a particular non-accounting anomaly variable and denote the quintile firm i from cohort g belongs to by Qtl_{igt}. We also define an aggregate anomaly variable Net_{igt}, which is the sum of the numbers of long-side and short-side anomaly quintile portfolios firm i from cohort g belongs to at the beginning of month t. We estimate the following set of stacked DiD regressions, $R_{igt} = \beta_1 Qtl_{igt} \times POST\_EDGAR_{igt} + \beta_2 POST\_EDGAR_{igt} + \beta_3 Qtl_{igt} \times Treated_{ig} + \beta_4 Treated_{ig} + Qtl_{igt} \times TimeFE + TimeFE + \epsilon_{igt}$, $R_{igt} = \beta_1 Net_{igt} \times POST\_EDGAR_{igt} + \beta_2 POST\_EDGAR_{igt} + \beta_3 Net_{igt} \times Treated_{ig} + \beta_4 Treated_{ig} + Net_{igt} \times TimeFE + TimeFE + \epsilon_{igt}$. We cluster standard errors by firm, given that the implementation of EDGAR is a firm-level shock.

	(1) MOM	(2) NSI	(3) CEI	(4) Aggregate
$Qtl \times POST\_EDGAR$	-0.05	0.03	0.16	
	(-0.31)	(0.20)	(0.91)	
$Net \times POST\_EDGAR$				0.09
				(0.48)
$POST\_EDGAR$	-0.30	-0.50	-1.24**	-0.43**
	(-0.53)	(-0.95)	(-2.04)	(-2.02)
$Treat$	0.21	-0.19	0.54	0.11
	(0.52)	(-0.45)	(1.27)	(0.66)
$Qtl \times Treat$	-0.01	0.11	-0.19	
	(-0.10)	(0.86)	(-1.36)	
$Net \times Treat$				-0.11
				(-0.65)
$Qlt \times$Time Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes
No. of Observations	52,479	52,647	30,656	53,061

Table 8. DiD Results for Accounting Anomalies: XBRL

This table presents the stacked DiD results for accounting anomalies. For each cohort g, at the beginning of each month t, we sort all (treated and control) firms into quintiles according to a particular accounting anomaly variable and denote the quintile firm i from cohort g belongs to by Qtl_{igt}. We also define an aggregate anomaly variable Net_{igt}, which is the sum of the numbers of long-side and short-side anomaly quintile portfolios firm i from cohort g belongs to at the beginning of month t. We estimate the following set of stacked DiD regressions, $R_{igt} = \beta_1 Qtl_{igt} \times POST\_XBRL_{igt} + \beta_2 POST\_XBRL_{igt} + \beta_3 Qtl_{igt} \times Treated_{ig} + \beta_4 Treated_{ig} + Qtl_{igt} \times TimeFE + TimeFE + \epsilon_{igt}$, $R_{igt} = \beta_1 Net_{igt} \times POST\_XBRL_{igt} + \beta_2 POST\_XBRL_{igt} + \beta_3 Net_{igt} \times Treated_{ig} + \beta_4 Treated_{ig} + Net_{igt} \times TimeFE + TimeFE + \epsilon_{igt}$. We cluster standard errors by firm, given that the implementation of XBRL is a firm-level shock.

	(1) AC	(2) NOA	(3) IVA	(4) AG	(5) GP	(6) Aggregate
$Qtl \times POST\_XBRL$	-0.36	0.36	-0.48**	-0.24	-0.44*	
	(-1.33)	(1.49)	(-2.12)	(-0.86)	(-1.73)	
$Net \times POST\_XBRL$						-0.45**
						(-2.28)
$POST\_XBRL$	1.93**	-0.16	2.24***	1.62*	2.21**	0.90***
	(2.17)	(-0.22)	(3.12)	(1.87)	(2.56)	(2.79)
$Treat$	-2.17***	-0.26	-1.10*	0.11	-2.90***	-1.27***
	(-2.71)	(-0.40)	(-1.66)	(0.16)	(-3.66)	(-4.41)
$QTL \times Treat$	0.31	-0.35	-0.05	-0.47**	0.55**	
	(1.32)	(-1.58)	(-0.22)	(-1.99)	(2.42)	
$Net \times Treat$						0.05
						(0.27)
$Qtl\times$ Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
No. of Observations	11,559	11,211	11,367	11,559	11,559	11,559

Table 9. DiD Results for Non-Accounting Anomalies: XBRL

This table presents the stacked DiD results for non-accounting anomalies. For each cohort g, at the beginning of each month t, we sort all (treated and control) firms into quintiles according to a particular non-accounting anomaly variable and denote the quintile firm i from cohort g belongs to by Qtl_{igt}. We also define an aggregate anomaly variable Net_{igt}, which is the sum of the numbers of long-side and short-side anomaly quintile portfolios firm i from cohort g belongs to at the beginning of month t. We estimate the following set of stacked DiD regressions, $R_{igt} = \beta_1 Qtl_{igt} \times POST\_XBRL_{igt} + \beta_2 POST\_XBRL_{igt} + \beta_3 Qtl_{igt} \times Treated_{ig} + \beta_4 Treated_{ig} + Qtl_{igt} \times TimeFE + TimeFE + \epsilon_{igt}$, $R_{igt} = \beta_1 Net_{igt} \times POST\_XBRL_{igt} + \beta_2 POST\_XBRL_{igt} + \beta_3 Net_{igt} \times Treated_{ig} + \beta_4 Treated_{ig} + Net_{igt} \times TimeFE + TimeFE + \epsilon_{igt}$. We cluster standard errors by firm, given that the implementation of XBRL is a firm-level shock.

	(1) MOM	(2) NSI	(3) CEI	(4) Aggregate
$Qtl \times POST\_XBRL$	0.11	0.04	0.05	
	(0.44)	(0.18)	(0.17)	
$Net \times POST\_XBRL$				0.07
				(0.25)
$POST\_XBRL$	0.58	0.69	0.38	0.79**
	(0.67)	(0.82)	(0.39)	(2.43)
$Treat$	-1.58**	-0.91	-1.12	-1.32***
	(-2.01)	(-1.37)	(-1.49)	(-4.76)
$Qtl \times Treat$	0.08	-0.14	0.02	
	(0.36)	(-0.74)	(0.08)	
$Net \times Treat$				-0.07
				(-0.33)
$Qlt \times$Time Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes
No. of Observations	11,396	11,451	8,734	11,559