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Abstract

We show that U.S. analysts alter their forecasting behavior in response to a randomly assigned shock that exogenously varies the timeliness and cost of accessing companies' mandatory disclosures in the cross-section of investors: analysts reduce the number of stocks they cover, issue less optimistic and more accurate forecasts that are less bold, and collectively reduce forecast dispersion. Our investigation of possible channels favors the explanation that analysts reduce the strategic component of their behavior: the changes are more pronounced among analysts with stronger incentives to strategically skew their forecasts, such as affiliated analysts and those catering to retail investors. We conclude that mandatory disclosure is a substitute for information production by analysts, whose behavior is constrained by investors' ability to verify their forecasts using corporate filings.

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Do Corporate Disclosures Constrain Strategic Analyst Behavior? [*] [♠]

Yen-Cheng Chang[†] Alexander Ljungqvist[§] Kevin Tseng[¶]

April 18, 2020

Abstract

We show that U.S. analysts alter their forecasting behavior in response to a randomly assigned shock that exogenously varies the timeliness and cost of accessing companies' mandatory disclosures in the cross-section of investors: analysts reduce the number of stocks they cover, issue less optimistic and more accurate forecasts that are less bold, and collectively reduce forecast dispersion. Our investigation of possible channels favors the explanation that analysts reduce the strategic component of their behavior: the changes are more pronounced among analysts with stronger incentives to strategically skew their forecasts, such as affiliated analysts and those catering to retail investors. We conclude that mandatory disclosure is a substitute for information production by analysts, whose behavior is constrained by investors' ability to verify their forecasts using corporate filings.

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Mandatory disclosure is the cornerstone of U.S. securities market regulation. Major policy changes in disclosure regulation such as the 2000 Regulation Fair Disclosure (Reg FD) or the 2002 Sarbanes-Oxley Act (SOX) aim to improve market quality and protect investors. However, the optimal level of mandatory disclosure remains hotly debated (Goldstein and Yang 2017), with companies often favoring a lower disclosure burden and investor advocates arguing for greater transparency. How mandatory disclosure shapes a firm's external information environment remains a question of great interest to both scholars of information economics and policymakers (Leuz and Wysocki 2016).

In this paper, we explore how mandatory disclosure affects analyst behavior and, by implication, investor utility. Sell-side analysts serve an important role as information intermediaries in the stock market, yet a large literature has documented biases in analysts' earnings forecasts linked to their strategic incentives.[1] We ask how low-cost, timely, and equal access to mandatory disclosures constrain analysts' strategic behavior.

To identify the causal interplay between mandatory disclosure and analyst behavior, we exploit a randomly assigned shock that exogenously varies the timeliness and cost of accessing companies' mandatory disclosures in the cross-section of investors. Specifically, we exploit the staggered implementation of the Electronic Data Gathering, Analysis, and Retrieval (EDGAR) system by the Securities and Exchange Commission (SEC). Before EDGAR, investors could access firms' mandatory filings only at high cost, either by subscribing to commercial data providers or by physically visiting one of the SEC's reference rooms in Chicago, New York, or Washington DC (Rider 2001). Beginning in April 1993, the SEC required U.S. firms to file their mandatory disclosures (such as 10-Ks, 10-Qs, or 8-Ks) electronically through the EDGAR

[1] Prior work shows that analyst forecasts affect stock prices (Womack 2001, Gleason and Lee 2003, Jegadeesh et al. 2004, Ljungqvist, Malloy, and Marston 2007, and Kelly and Ljungqvist 2008, among others). For recent surveys of the literature on analyst behavior, see Beyer et al. (2010), Bradshaw (2011), and Kothari, So, and Verdi (2016).

system. Making a firm's SEC filings available through EDGAR reduced asymmetries of information access among market participants without (as we show) changing the nature of the information firms disclose.

Helpfully for identification purposes, the SEC assigned firms *randomly* to one of ten implementation waves, thereby staggering inclusion in EDGAR over a three-year period between 1993 and 1996.[2] We can thus compare firms that were randomly included in EDGAR in quarter t to observably similar control firms that were not yet included in EDGAR. Conditionally random assignment and staggered implementation significantly reduce endogeneity concerns (Leuz and Wysocki 2016). Critically, an omitted variable would need to coincide in time with the phase-in dates to be able to materially confound our findings.

Using a standard difference-in-differences (DD) approach, we show that analysts alter their forecasting behavior when a firm joins EDGAR and its filings thereby become freely, timely, and equally available to all investors. In short, analysts reduce the number of stocks they cover, issue less optimistic and more accurate forecasts that are less bold, and collectively reduce forecast dispersion.

The reduction in coverage suggests that mandatory disclosure and information production by analysts are substitutes to some extent.[3] A priori, while joining EDGAR reduces information asymmetry among investors (as we show), the effect on coverage is ambiguous. Cheaper and timelier access to corporate disclosures reduces information production costs (Verrecchia 1982, Kim and Verrecchia 1994), and so could encourage an increase in coverage. However, we expect this effect to be small, because many brokerage houses likely already subscribed to commercial

[2] Table 1 lists the ten phase-in dates. In private correspondence, Scott Bauguess, then Acting Chief Economist of the SEC, informed us that the wave assignments were determined solely on the basis of firm size.
[3] This result is reminiscent of the fall in analyst coverage following Reg FD (Irani and Karamanou 2003), which has been interpreted as a crowding-out effect of increased mandatory disclosure.

data feeds before EDGAR. On the other hand, cheaper and timelier access to corporate disclosures by investors could reduce the value of analysts' information production, inducing exit (Lang and Lundholm 1996, Dugast and Foucault 2018).

The two most widely studied measures of analyst forecasting behavior are optimism and inaccuracy (O'Brien 1988). Optimistic and inaccurate forecasts have been attributed to analysts' career concerns, conflicts of interest from the investment banking or trading divisions, or a desire to curry favor with the management of firms analysts follow.[4] We find sizeable reductions in optimistic bias and improvements in accuracy when a firm joins EDGAR. These reductions are evident even at the analyst-firm level: a given analyst becomes less optimistic and more accurate about a given firm's future prospects after that firm joins EDGAR.

At the same time, we find that analyst forecasts have less price impact post-EDGAR, suggesting that broader access to mandatory disclosures improves firms' information environments and thereby reduces the informativeness of analyst forecasts.[5] This pattern reinforces our conclusion that mandatory disclosure and information production by analysts are substitutes. Moreover, dispersion in earnings forecasts and analysts' willingness to deviate from other analysts both decline post-EDGAR. We interpret these results as suggesting that analysts become less willing to strategically reveal their private signals post-EDGAR (Trueman 1994).

Our investigation of possible channels favors the explanation that analysts reduce the strategic component of their behavior: the changes are more pronounced among analysts with stronger incentives to strategically skew their forecasts, such as affiliated analysts and those

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[4] For work on career concerns leading to forecast optimism, see Stickel (1992), Mikhail, Walther, and Willis (1999), Hong, Kubik, and Solomon (2000), Wu and Zang (2009). For work on conflicts of interest arising from investment banking or trading commissions, see McNichols and O'Brien (1997), Michaely and Womack (1999), Ljungqvist, Marston, and Wilhelm (2006), and Groysberg, Healy, and Maber (2011). For work on incentives to curry favor with management, see Francis and Philbrick (1993), Das, Levine, and Sivaramakrishnan (1998), Chen and Matsumoto (2006), Mayew (2008), and Hilary and Hsu (2013).

[5] This finding echoes findings of lower informativeness after the adoption of Reg FD (Gintschel and Markov 2004).

catering to retail investors. We find no evidence that firm fundamentals or transparency change, which reduces concerns that the changes in analyst behavior we document simply reflect changes in companies' prospects or disclosure policies. Nor do we find any evidence consistent with the idea that analysts change their behavior because EDGAR improves their own access to corporate disclosures. Instead, we conclude that mandatory disclosure acts a substitute for information production by analysts, whose ex ante behavior is constrained by investors' improved ability to verify their forecasts ex post with the help of corporate filings.

Our study makes contributions to two literature. First, we contribute to the literature on the economics of mandatory disclosure. Sellside analysts are viewed as important information intermediaries whose self-serving strategic behavior affects the quality of firms' external information environments. Our empirical evidence suggests that free, timely, and equal access to mandatory disclosures can enable investors to police analysts' strategic behavior in ways that improve firms' external information environments, resulting in improved trading liquidity and thereby reductions in firms' costs of capital. Our conclusion that equal *access* to mandatory disclosures matters complements prior work focusing on the *content* of mandatory disclosures, often through the lens of regulatory changes such as Reg FD or SOX (Chhaochharia and Grinstein 2007, Duarte et al. 2008, Koch, Lefanowicz, and Robinson 2013, Coates and Srinivasan 2014).

Our evidence also contributes to the policy debate on the costs and benefits of disclosure. While disclosure regulations are typically motivated by a desire to level the playing field for all investors, they can have unintended consequences such as crowding out information production. Our evidence reinforces this concern, in view of the reductions in analyst coverage and price impact we find. In this sense, our evidence echoes prior work on Reg FD (Gintschel and Markov 2004, Gomes, Gorton, and Madureira 2007, Koch, Lefanowicz, and Robinson 2013).

In their review of the disclosure regulation literature, Leuz and Wysocki (2016) propose that "identification and causal inferences are of first-order importance for policy and regulatory debates." We hope that our policy experiment can expand the range of natural experiments with which the costs and benefits of disclosure have been investigated. As we argue, the staggered implementation of EDGAR, along with conditional random assignment, significantly reduces endogeneity concerns.

The second literature we contribute to is the literature on analyst behavior. Our central conclusion that strategic behavior is a function of verification costs speaks directly to cheap-talk models such as Crawford and Sobel (1982), who show that in equilibrium there is some deception by the sender unless the sender's and receiver's interests are aligned in all states; Morgan and Stocken (2003), Ottaviani and Sørensen (2006), Chen, Kartik, and Sobel (2008), and Guttman (2010), who model senders who differ in the precision of their private signals and whose reports strategically convey both their private signals and their "quality;" Crawford (2003), who models the case of some senders who always tell the truth; and Ottaviani and Sørensen (2006), in whose models some receivers are always trusting. In light of our evidence, a promising avenue for future research in cheap-talk models is to allow for finite verification costs.

1. Empirical Strategy and Data

1.1 Institutional Background

Testing whether free, timely, and equal access to companies' mandatory disclosures constrains strategic analyst behavior requires a shock to disclosure access that is randomly assigned to some firms while other firms are unaffected and so can serve to establish a counterfactual. Our identification strategy relies on the introduction of the SEC's EDGAR system. To understand how EDGAR made disclosure access both timelier and more equal and thereby made the informational playing fields between investors and analysts and among investors more level, consider how investors accessed corporate disclosures pre-EDGAR.

Prior to EDGAR, 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: they could either physically visit one of the three SEC reference rooms (located in Chicago, New York, and Washington DC) or they could subscribe to commercial data vendors.[6] Commercial access was, apparently, quite costly. According to a 1992 petition to the SEC signed by academics, librarians, and journalists, Mead Data Central charged "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."[7] Dialog, a competitor to Mead, charged "$84 per hour plus $1 per page."[8] To illustrate, obtaining Ford's 1994 10-K from Dialog would have cost $145 in page charges alone.

Given these access options, there were three categories of investors: those who chose not to have access to mandatory filings, those who accessed them physically (likely with some delay), and those who paid for timely online access. We suspect that most individuals and quite a few institutional investors fell into the no-access category, with only those located near an SEC reference room accessing filings physically, and only larger institutions paying for online access. As a result, we conjecture that there were widespread and systematic informational asymmetries across different investor groups pre-EDGAR. Retail investors in particular were at an informational disadvantage, not only relative to institutional investors but also relative to information intermediaries such as sell-side analysts.

Facing increasing costs of receiving, managing, and distributing large numbers of corporate filings for public use, and after years of lobbying by civil-society groups and members of

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[6] Investors who were shareholders of record on the record date could wait to receive a copy of the annual report in the mail.

[7] Quoted from http://www.bio.net/bionet/mm/ag-forst/1992-January/000187.html.

[8] Ibid.

Congress, the SEC announced on February 23, 1993 a plan to require all SEC-registered firms to submit their filings electronically.[9] The SEC's announcement included a phase-in schedule, with registered firms joining EDGAR in ten waves over the three years starting April 26, 1993 and ending May 6, 1996. Firms in waves 5 through 10 did not know their EDGAR join dates until a few months before joining.[10]

Electronic *filing* per se would not be expected to affect investors' costs of accessing mandatory disclosures. The actual shock to information *access* that we exploit is due to the National Science Foundation's decision in October 1993 to acquire Mead Data Central's historic EDGAR filings and to fund a project to make all EDGAR filings – past and current – available for free online, hosted by New York University's Stern School of Business. Online access to EDGAR went live on January 17, 1994, when the historic and current filings of firms in the SEC's first four implementation waves (as well as those of previous voluntary filers) became available via the NYU online-access system.[11] In the six remaining waves, firms both joined EDGAR and had their historic and current filings become publicly available online at the same time. Figure 1 illustrates the timeline of events.

1.2 Identification Strategy

We view the information-economic effects of the introduction of online access to corporate filings via first NYU and eventually the SEC's EDGAR website (henceforth simply "EDGAR inclusion") as a reduction in investors' costs of verifying the accuracy and veracity of information provided by information intermediaries such as analysts. In particular, reduced verification costs should constrain analysts' ability to strategically skew their forecasts and

[9] See SEC Release No. 33-6977.
[10] After wave 4 was phased in on December 6, 1993, there was a six-months review. The SEC announced the final rules on EDGAR implementation on December 19, 1994, in which the dates of waves 5 through 10 were revised and made final (SEC Release No. 33-7122). We use the final phase-in dates as per the December 1994 announcement.
[11] The SEC took over the task of hosting online access to EDGAR from NYU in October 1995.

recommendations in ways that benefit themselves or their brokerage-firm employers.[12] Reduced verification costs should thus result in a reduction in strategic behavior by analysts.

Our identification strategy exploits three features of the way the SEC implemented EDGAR. First, the SEC assigned registered firms to the ten implementation waves *randomly*, conditional only on size. Second, while all registered firms joined EDGAR eventually, the staggered roll-out of EDGAR provides us with a set of control firms with which to establish a counterfactual that is plausibly free of the confounding effects of unobserved contemporaneous factors that might have affected analyst behavior. Such confounding factors would not only have to coincide in time with the EDGAR phase-in schedule (and the NSF's online access timetable) but also affect treated (though not control firms) at around the same time as their filings became available online – which, while not impossible, strikes us as unlikely. Third, the fact that firms in waves 1-4 did not know that their filings were going to be put online, coupled with the fact that firms in waves 5-10 were given little notice of their phase-in dates, greatly reduces the risk of confounds that result from firms changing their behavior ahead of treatment.

We implement our identification strategy using a standard difference-in-differences (DD) design, comparing a set of treated firms whose mandatory disclosures become freely available to all investors at the same time to a set of matched control firms with similar characteristics whose disclosures randomly remain expensive for investors to access. Specifically, we estimate DD regressions of the following general form:

$$outcome_{it} = \alpha + \beta_1 SHOCK_{it} + \beta_2 POSTSHOCK_{it} + \gamma \mathbf{X}_{it-1} + c_i + c_q + c_f + \varepsilon_{it}, \quad (1)$$

[12] The analyst literature has explored how reputational concerns counteract strategic analyst behavior (see Hong, Kubik, and Solomon 2000, Krigman, Shaw, and Womack 2001, Cowen, Groysberg, and Healy 2006, Ljungqvist, Marston, and Wilhelm 2006, Ljungqvist et al. 2007, Clarke, Khorana, Patel, and Rau 2007, Kolasinski and Kothari 2008, among others). Reduced verification costs would make reputational concerns more salient and thereby reduce strategic behavior.

where $outcome_{it}$ is measured for firm i in fiscal quarter t; $SHOCK_{it}$ and $POSTSHOCK_{it}$ are indicator variables that equal one if firm i is included in EDGAR in fiscal quarter t and $t-1$ to $t-4$, respectively; $\mathbf{X}_{it-1}$ is a vector of control variables; and c_i, c_q, and c_f are firm, time, and fiscal-quarter fixed effects, respectively. Standard errors are clustered at the firm level, given that we exploit a firm-level shock.

Random assignment and staggering go a long way towards ensuring the internal validity of the EDGAR experiment. We discuss the plausibility of the identifying assumptions behind our DD design in detail towards the end of Section 2.

1.3 Sample and Data

1.3.1 Treated and Control Firms

We construct our samples of treated and control firms as follows. With one important exception, firms are treated from the fiscal quarter in which they are included in EDGAR. The exception concerns firms in phase-in waves 1 through 4, whose electronic EDGAR filings did not become publicly available *online* until January 17, 1994, and so are considered treated for our purposes only from that date onwards.[13]

Following standard practice, we restrict the sample to firms traded on the NYSE, NASDAQ, or AMEX and exclude firms with CRSP share codes greater than 11 (foreign issuers, real estate trusts, master limited partnerships, and the like). We follow each treated firm for nine fiscal quarters centered on the quarter its filings went online (its EDGAR inclusion quarter for short).

Eventually, all SEC-registered firms are treated, as every issuer is obliged to file through EDGAR starting on May 6, 1996. Control firms are thus selected from the set of to-be-treated

[13] In this respect, we depart from other work using EDGAR as a shock, such as Gao and Huang (2019) and Emery and Gulen (2019). As will show, analysts respond in ways that support our claim that it is online access, not electronic filing, that matters.

firms. Naturally, the last EDGAR wave lacks controls and – due to bunching towards the end of the SEC's phase-in schedule – so do waves 8 and 9. We thus have four staggered treatment dates: January 17, 1994, January 30, 1995, March 6, 1995, and May 1, 1995.

We select control firms using a nearest-neighbor propensity-score method. We match treated and controls on three dimensions: equity market capitalization (in levels and logs), to hold constant the SEC's size criterion when assigning firms to implementation waves; fiscal quarter, to hold constant well-known seasonalities in analyst forecasts over the course of the fiscal year;[14] and the log number of analysts covering a stock, to hold constant competitive effects among analysts constraining their behavior (Hong and Kacperczyk 2010).

Only matches in the common support are considered valid, using a 0.05 caliper. This limits our estimation sample to a total of 2,158 treated and 2,158 control firms. As Table 1 shows, the average treated firm has an equity market capitalization of $199.2 million in the fiscal quarter before treatment. This average is considerably smaller than the $860.5 million market cap of the average listed U.S. firm in the quarter before its phase-in wave. Table 1 shows why. The SEC skewed assignment in the first two waves heavily towards large firms. Because the first two waves occurred only three months apart, there are few large untreated firms left in the common support: only 87 of the 510 firms in the first two waves have valid controls. To the extent that smaller firms provide analysts greater scope to engage in strategic behavior, our empirical estimates may accordingly overstate the effects of free, timely, and equal access to corporate disclosures on analyst behavior for the average U.S. listed firm.

1.3.2 Measures of Analyst Behavior

Following the extensive literature on analyst behavior, we focus on analyst optimism (or

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[14] Earnings forecasts tend to become more accurate the later in a firm's fiscal year they are made (Richardson, Teoh, and Wysocki 2004). See Chang et al. (2020) for further discussion.

forecast bias), inaccuracy (or forecast errors), informativeness (or the price impact of forecast revisions), dispersion in forecasts, and forecast boldness (or deviations from consensus). Analysts make forecasts for both short-term (say, next fiscal-quarter) and long-term (say, fiscal-year) earnings. Accordingly, we measure optimism, inaccuracy, dispersion, and boldness for both next-fiscal-quarter and fiscal-year forecasts. Variable definitions and details of their construction can be found in Appendix A.

Table 2 reports summary statistics, separately for treated and control firms and measured in either levels or changes in the fiscal quarter before treatment. Treated and control firms have near-identical optimism, inaccuracy, informativeness, dispersion, and boldness in the quarter before treatment, both in levels and in changes. The *t*-tests shown in the last column confirm that with one exception, the difference in pre-treatment changes between treated and controls is not statistically significant. The exception is optimism in long-term forecasts, which increases by significantly more for control firms than for treated firms in the quarter before treatment.[15]

1.3.3 Control Variables

Given conditional random assignment to treatment, treated and control firms differ only randomly from each other in their characteristics. While this obviates the need for the kinds of control variables sometimes included in empirical work in this area, we still have to deal with two issues. The first issue is that the SEC's assignment to treatment is *conditionally* random, i.e., conditional on market capitalization. Our research design takes this issue into account by matching on market cap when selecting control firms. As Table 2 shows, our treated and control firms are matched quite precisely on market cap. We additionally include log market cap as a control variable in our DD regressions.

[15] In subsequent tests, we cannot reject the null hypothesis of no diverging pre-trends even in long-term optimism, supporting the parallel-trends assumption required for identification.

The second issue is the aforementioned seasonality in analyst forecasting behavior. To hold seasonality constant, our research design matches on fiscal year-end when selecting control firms. We additionally include fixed effects for fiscal quarter in our DD regressions.

Finally, we include the usual time and firm fixed effects in our specifications, to ensure consistent estimation of treatment effects in a DD context. Since time is measured in quarters in our setting, we include calendar-quarter fixed effects. These time effects remove the effects of any common shocks that affect all firms in a given quarter, such as market-wide changes in regulations or macroeconomic news.

2. Disclosure Access and Analyst Behavior

2.1 Validating the Shock

To establish that EDGAR inclusion is a sufficiently large shock to a firm's information environment such that it has the potential to materially affect analyst behavior, we begin by estimating changes in a standard measure of investor attention, abnormal trading volume (Barber and Odean 2008). Table 3 shows that trading volume increases significantly in the fiscal quarter a firm is included in EDGAR, relative to matched controls not yet included in EDGAR (p=0.017). The point estimate shown in column 1 suggests that trading volume increases by an economically meaningful 5.3% from the sample mean in the pre-treatment quarter. Retail investors should be particularly responsive to easier access to corporate disclosure, as they faced the highest access costs to begin with. Column 2 shows that retail trading volume (measured using Barber and Odean's 2008 discount-brokerage data) increases significantly in the fiscal quarter a firm is included in EDGAR (p=0.080).[16] We interpret these increases in trading activity as consistent with investors (and perhaps especially retail investors) paying more attention to a firm when its mandatory disclosures are more easily accessible.

[16] We thank Terrance Odean for sharing the brokerage data.

Next, we consider three standard measures of liquidity: Amihud's (2002) illiquidity measure (better known as AIM), Goyenko, Holden, and Trzcinka's (2009) effective tick measure, and Lesmond, Ogen, and Trzcinka's (1999) fraction of trading days with zero or missing returns. If, as we have argued, EDGAR inclusion reduces information asymmetries, we expect liquidity to improve. The estimates shown in column 3 through 5 support this prediction. All three measures decline in the quarter of EDGAR inclusion and over the next four quarters, suggesting that liquidity improves. Amihud's illiquidity measure, for example, drops by 4.2% from the pre-treatment mean over the four quarters following EDGAR inclusion (p=0.017).

Finally, we consider volatility. If less costly, timelier, and more equal access to corporate disclosures reduces uncertainty about a firm's prospects, we expect treated firms to see a reduction in volatility. The DD results, shown in column 6, support this prediction. Over the four quarters post-EDGAR inclusion, volatility decreases by 2.4% compared to the pre-treatment mean (p=0.048).

The results in Table 3 suggest that EDGAR inclusion is a meaningful shock to firms' information environments: investors respond to it by trading more, liquidity improves, and volatility declines. We next investigate how analysts respond to EDGAR inclusion.

2.2 Changes in Analyst Behavior Around EDGAR Inclusion

We investigate analysts' responses to a firm's mandatory disclosures becoming freely and timely accessible to all investors on both the extensive margin (does the analyst continue to cover the stock?) and the intensive margin (how does the analyst change her forecasting behavior?). We model responses both at the stock-level (asking for example how the number of analysts or the dispersion of analyst forecasts changes) and at the analyst/stock-level (asking how a given analyst changes her coverage or forecasts of a given stock around EDGAR inclusion). In the remainder of this section, we present arguably causal evidence that analysts change their behavior. In Section 3, we explore possible reasons for why they do so.

2.2.1 Analyst Coverage

Table 4 reports results for the extensive margin. At the stock-level, we see a significant decline in analyst coverage starting in the quarter of EDGAR inclusion and continuing for the next four quarters ($p<0.001$). For example, the number of analysts declines by 0.16 in the quarter a company joins EDGAR, relative to control firms. This represents a 7% decline in coverage compared to the previous quarter. At the analyst/stock-level, we see a similar decline in coverage. Here, the DD coefficients represent the effect of EDGAR inclusion on the likelihood that a *given* analyst continues to cover a *given* stock post-EDGAR. This likelihood decreases by 3% in the treatment quarter ($p=0.001$) and remains 4.2% lower over the subsequent four quarters.

The internal validity of our DD analysis requires that treated and control firms would have experienced similar trends in coverage but for the EDGAR treatment. A common way to gauge the plausibility of the parallel-trends assumption is to check for the absence of diverging trends before treatment. Figure 2 plots dynamic DD estimates of the effects of EDGAR inclusion on coverage over our nine-quarter window, along with 95% confidence intervals. The figure confirms the absence of diverging pre-trends: coverage is similar among treated and control firms in the quarters before EDGAR inclusion and then falls significantly among treated firms in the quarter they join EDGAR inclusion, without recovering over the next four quarters.

The absence of diverging pre-trends supports a causal interpretation of the patterns in Table 4 and Figure 2. EDGAR inclusion leads to companies being covered by fewer analysts, suggesting that mandatory disclosures and information production by analysts are substitutes to some extent.

2.2.2 Forecast Optimism

Prior literature documents that analysts' earnings forecasts are, on average, overly optimistic and that strategic considerations (reflecting career concerns, compensation incentives, or a desire to stay on good terms with management) may be at play. How does optimism (the scaled difference between forecast and realized earnings) change when investors have free, timely, and equal access to a firm's mandatory disclosures?

Table 5 reports the results. At the stock-level, average optimism decreases for both short-term (next-quarter) forecasts and long-term (fiscal-year) forecasts in the quarter a firm joins EDGAR; optimism remains at a significantly lower level over the next four quarters. Each DD estimate is statistically significant and economically sizeable. To illustrate, joining EDGAR reduces average short-term optimism by 51%, from 0.007 in the pre-treatment quarter to around 0.003 in the treatment quarter and the next four quarters. At the analyst/stock-level, we see that a given analyst issues forecasts that are less optimistic post-EDGAR than that same analyst's forecasts were for that stock in the previous four quarters. The reduction in analyst/stock-level optimism is statistically significant for both short-term and long-term forecasts starting in the treatment quarter and does not revert back over the next four quarters ($p<0.001$).

Figure 3 plots the corresponding dynamic DD estimates, confirming again the absence of significantly diverging pre-trends as well as the persistence in the decline in forecast optimism.

2.2.3 Inaccuracy

Less optimistic forecasts are not necessarily the same as more accurate forecasts. To assess accuracy requires measuring forecast errors, taking the absolute value of the difference between a forecast and realized earnings (appropriately scaled). We refer to these forecast errors as "inaccuracy," given that larger errors correspond to less accurate forecasts.

Table 5 shows that inaccuracy decreases significantly following EDGAR inclusion and that forecasts remain more accurate in the following four quarters. This is true for both short- and long-term forecasts. It is also true both for the average forecast made for a given firm and for a given analyst-firm pair. The improvements in accuracy are economically sizeable. To illustrate, joining EDGAR reduces average short-term inaccuracy by 24%, from 0.012 in the pre-treatment quarter to 0.009 in the treatment quarter, without reverting back over the next four quarters.

Figure 4 plots the corresponding dynamic DD estimates. There is no evidence of significantly diverging pre-trends, consistent with the parallel-trends assumption required for identification.

2.2.4 Informativeness of Analyst Forecasts

Announcements of analyst forecasts move prices when they are seen as revealing new information in the eyes of the marginal investor. We predict that there is less scope for analysts to move prices when the marginal investor is given free, timely, and equal access to corporate disclosures. The reduction in bias and noise found in the previous two subsections may, on the other hand, mitigate the predicted reduction in price impact.

Measuring informativeness as the price impact that can be attributed to analyst forecasts (Lehavy, Li, and Merley 2011, Merkley, Michaely, and Pacelli 2017), Table 6 shows that investors view analyst forecasts as less informative once a stock joins EDGAR. For the average treated stock, informativeness declines by 11%, from 0.071 in the quarter before treatment to 0.063 in the treatment quarter, without reverting back over the next four quarters (p<0.001). Informativeness declines even though the average analyst forecast has become both less biased and less noisy.

Like our earlier results showing a reduction in coverage, we interpret these findings to suggest that mandatory disclosure and information production by analysts are substitutes.

2.2.5 Forecast Dispersion

There are at least three reasons to expect the dispersion of analyst forecasts to decline after a firm joins EDGAR. First, reduced optimism and increased accuracy should mechanically reduce dispersion. Second, to the extent that EDGAR inclusion improves some analysts' access to mandatory disclosures (for example, those at smaller brokerage houses that could not justify the expense of a subscription to Mead Data Central or Dialog), we expect information asymmetries, and hence disagreement, among analysts to decline. Third, EDGAR inclusion may increase analysts' incentives to herd rather than stand out from the crowd. Timelier and less costly access to corporate disclosures makes it easier for investors to evaluate an analyst's forecast performance, which could discourage the kinds of long-shot (or bold) forecasts that could hurt an analyst's career if later proven wrong.[17]

Table 6, columns 2 and 3 confirm our prediction: dispersion in both short-term and long-term forecasts declines significantly, beginning in the quarter after EDGAR inclusion (p<0.001). The reductions are again economically meaningful, averaging 12% for short-term dispersion and 16% for long-term dispersion. Columns 4 and 5 use a standard analyst/stock-level measure of boldness borrowed from Hong, Kubik, and Solomon (2000), showing that a given analyst makes significantly less bold short-term and long-term forecasts for a given firm after the firm joins EDGAR, all else equal. Economically, boldness decreases by 20% for short-term forecasts (p=0.031) and 22% for long-term forecasts (p=0.004).

2.3 Identification Concerns

Difference-in-differences models like ours make certain identifying assumptions which need to be satisfied for DD estimates to be interpreted as causal. First, treatment must be randomly assigned, or else systematic unobserved differences between treated and controls could cause post-treatment differences between treated and controls that are nothing to do with the treatment. This assumption is arguably satisfied in our case, given the specifics of the way the SEC implemented the transition to EDGAR.

Second, and closely related to random assignment, the difference between treated and

[17] For example, issuing bold forecasts runs the risk of being labelled low-ability if investors believe high-ability analysts receive correlated information (Scharfstein and Stein 1990, Prendergast and Stole 1996).

controls must be constant over time in the absence of treatment. Conditional random assignment goes a long way to ensuring that this parallel-trends assumption is likely to hold in our setting, by eliminating concerns that treated and controls differ systematically on unobservables that could cause differences in post-treatment trends to emerge. The fact that we fail to find diverging pre-trends further supports the parallel-trends assumption.

Third, treatment must not coincide in time with other events that affect the treated and controls differently. Conditional random assignment coupled with the staggered way in which the SEC rolled out EDGAR greatly reduces the scope for violations of this assumption: random assignment ensures that treated and controls are not plausibly differentially sensitive to unobserved contemporaneous shocks, and staggering ensures that firms are treated at different times on a schedule that is unlikely to coincide with unobserved shocks.

Fourth, the effects of treatment must be confined to the treated and not spill over to controls, or else interactions between treated and controls could bias the estimated treatment effect. In our setting, this Stable Unit Treatment Value Assumption (SUTVA for short) would be violated if analyst k changed her forecasting behavior in quarter t not just for those stocks i_k that join EDGAR at t but also for the other stocks $\neg i_k$ she covers that will join EDGAR at a future time and so serve as our controls.[18] Of course, if analysts did change their behavior for both treated and controls, our DD estimates would be attenuated downwards. In other words, violations of SUTVA work against us finding any effect of EDGAR inclusion on analyst behavior.

Still, a small change to our empirical design allows us to test for violations of SUTVA directly. So far, we have coded as the treated unit either stock i or an analyst-stock pair i_k. Now,

[18] A technological reason why the analyst might adjust her behavior simultaneously for current treated and future treated stocks is that EDGAR changes her production function in ways that are not confined to current treated stocks (for example, because the analyst combines information from multiple companies when making forecasts). An information-theoretic reason is that she anticipates that even stocks that have not yet joined EDGAR will eventually join EDGAR, which will change the nature of the game she plays with investors and firms.

we code analyst k as being treated from the time one or more of her stocks first joins EDGAR and ask how her forecasting behavior differs between those stocks joining EDGAR and those that have not yet joined EDGAR. In contrast to our previous specifications, we thus hold the analyst constant in this design. If the analyst changes her behavior for all her stocks when only some are included in EDGAR, the coefficient estimates will be zero.

The results, reported in Table IA.1 in the Internet Appendix, show that a given analyst reduces her optimism and inaccuracy by substantially and significantly more in stocks joining EDGAR than in those that have not yet joined EDGAR. Moreover, the coefficient estimates in Table IA.1 are economically quite close to those reported in our baseline models in Table 5, suggesting that potential violations of SUTVA have little material effect in our setting. To illustrate, in Table 5, analysts reduce long-term optimism for stocks joining EDGAR by an average of 1.461 relative to control stocks *covered by themselves or other analysts*, while in Table IA.1, they reduce long-term optimism by an average of 1.479 relative to control stocks *covered by themselves*.

The final identifying assumption DD models make is that treatment must be unexpected, or else treated (and potentially controls) could adjust to treatment prior to treatment in ways that confound the estimated treatment effect. In our setting, analysts might change their behavior well before a (or indeed any) stock joins EDGAR, knowing that free, timely, and equal access to corporate filings will eventually allow more investors to verify analyst reports in embarrassing ways. While EDGAR itself was not a surprise, two features of its implementation arguably were. The first feature is that EDGAR was not, when it was announced, intended to provide free, timely, and equal access to corporate filings. Instead, the SEC announced EDGAR as an electronic filing system. Only once the NSF funded NYU's attempts to put EDGAR online from January 17, 1994 did EDGAR become an electronic access system. This means that firms in the first four waves arguably were not expected to have their filings accessible online. Importantly, all our results are robust to using only the first four waves.[19] The second feature is that the SEC announced assignments to waves 5 through 10 only in December 1994. Thus, firms in these waves did not know their EDGAR join dates until a few months before joining.

2.4 Impact on Investors

So far, we have reported arguably causal evidence that analysts change their behavior around EDGAR inclusion, an event that investors appear to regard as sufficiently important so that trading volume, liquidity, and volatility all change in response. Before we consider possible reasons for why analysts change their behavior in response to EDGAR inclusion, we briefly consider how EDGAR impacts investors.

The finding that analyst coverage falls may on net be detrimental to investors, to the extent that it reduces information production about a stock. The finding that forecast bias and forecast errors both fall may on net be beneficial to investors, to the extent that the task of debiasing signals received from analysts becomes easier as a result. The finding that forecast dispersion falls could have the beneficial effect of reducing disagreement among investors, which in turn could make a stock less prone to crash risk (Chang et al. 2020). The finding that analysts make fewer bold forecasts could either harm investors (if it means that fewer outlier signals are incorporated in prices) or benefit them (if bold forecasts simply add noise to the consensus).

A summary measure of investor welfare eludes both us and the literature. Instead, we consider what happens to standard measures of the net precision of the signals available to investors. The finding that analyst forecasts carry less information post-EDGAR inclusion

[19] Table IA.2 in the Internet Appendix reports the results of estimating our baseline model using as treated firms those in waves 1 through 4 and with January 17, 1994 as the date on which firms unexpectedly had their corporate filings made accessible online. They confirm that analysts significantly reduce optimism and inaccuracy when a firm's filing become available online. Compared to the baseline estimates in Table 5, the treatment effects are somewhat larger in absolute terms, but not significantly so.

suggests the possibility that investors have become better informed. After all, investors combine signals from analyst reports and companies' (now more easily accessible) mandatory disclosures to guide their investment decisions and their response to new information.

Table 7 focuses on four measures of how investors respond to the new information contained in earnings announcements. If EDGAR inclusion improves the net precision of the conditioning information investors have access to, we expect investors to respond in a more muted way to earnings announcements than before. The four measures we use are the volatility of returns and volume of trading in a three-day window around a firm's quarterly earnings announcement, earnings surprises (i.e., standardized unexpected earnings, or SUE), and the speed with which stock prices adjust to the earnings announcement.

For each of our four measures, we find the expected attenuation in investor response. Earnings announcements are associated with significantly lower volatility (p=0.044) and reduced trading (p=0.016) after a firm joins EDGAR than before, all else equal. Earnings surprises become significantly smaller (p=0.006). Finally, stock prices converge significantly faster to the earnings news (p=0.015), in the sense that prices change by less in absolute terms when earnings are announced (Heflin, Subramanyam, and Zhang 2003).

Overall, we interpret the results in Table 7 as suggesting that EDGAR inclusion improves the net precision of the signals investors base their trading decisions on.

3. Why Do Analysts Change Their Behavior?

Having established that analyst behavior changes around a firm's inclusion in EDGAR, we next ask why analyst behavior changes. We investigate what we view as the three likeliest explanations: that EDGAR inclusion causes changes in firms' fundamentals or disclosure policies to which analysts respond by changing their forecasts; that EDGAR inclusion improves some analysts' access to corporate filings and thereby leads to changed forecasts; and that

EDGAR inclusion improves investors' access to corporate filings and thereby leads to a change in analysts' *strategic* behavior.[20] But first we confirm that it is online *access* to corporate disclosures – rather than electronic filing per se – that affects analyst behavior.

To do so, we exploit the delay between the dates when firms in waves 1 through 4 started submitting electronic filings and the dates when their filings went online on January 17, 1994 thanks to the NSF-NYU initiative. This allows us to test whether optimism and inaccuracy change when a firms begins to file electronically or when those filings become freely, timely, and equally available online. The results, reported in Table IA.3 in the Internet Appendix, confirm that analyst behavior changes around online access. This, in turn, validates our choice of coding January 17, 1994 as the treatment date for firms in waves 1 through 4.

3.1 Changes in Firm Fundamentals or Voluntary Disclosure Policies

Inclusion in EDGAR could potentially constrain firms' ability to "spin" or "manage" their earnings, as free, timely, and equal access to their mandatory disclosures enhances the ability of external parties (such as the media or activist investors) to scrutinize firms' external reporting.[21] If so, analysts might change their behavior around EDGAR inclusion not because they feel constrained in their ability to behave strategically, but because the nature of the corporate information on which they base their earnings forecasts has changed.[22]

We find no evidence suggesting that firm fundamentals change or that firms vary how transparent their financial reporting is. Table 8 reports the results. Column 1 shows that reported return on assets is no different, economically or statistically, after EDGAR inclusion than before.

[20] It is instructive to remember that even the best identified difference-in-differences model does not identify the reason for the observed change(s). Instead, identifying plausible explanations is a process of elimination.
[21] There is a substantial body of work in accounting documenting how mandatory disclosure can help constrain firms' reporting behavior. See Healy and Palepu (2001) for a survey and Jo and Kim (2007), Chuk (2013), Bonaimé (2015), and Chen, Hung, and Wang (2018) for recent contributions.
[22] Plenty of prior work documents that firm disclosure affects analysts' forecasting behavior. See Lang and Lundholm (1996) for a pioneering contribution or the literature survey of Healy and Palepu (2001).

In other words, we see no change in firm fundamentals that could plausibly cause analysts to change their forecasting behavior.

The remainder of Table 8 shows that firms do not appear to alter their voluntary disclosure policies around EDGAR inclusion. Column 2 uses Chen, Miao, and Shevlin's (2015) DQ measure. Chen, Miao, and Shevlin argue that finer reporting in the form of a greater number of line items in financial reports corresponds to higher disclosure quality.[23] The change in disclosure quality as measured by DQ is essentially zero following EDGAR inclusion, both economically and statistically.

Columns 3 and 4 use two accruals-based measures of earnings management. The first is discretionary accruals obtained from a modified Jones model (Dechow, Sloan, and Sweeney 1995); the second is performance-matched discretionary accruals (Kothari, Leone, and Wasley 2005). Neither measure changes significantly around EDGAR inclusion, either economically or statistically, suggesting that firms do not change how they manage their earnings.

Column 5 considers an alternative measure of the transparency of financial reporting: the tendency for a firm's reported earnings to narrowly meet-or-beat analysts' consensus forecast. Malmendier and Tate (2009) show that pressure to avoid missing consensus can induce CEOs to manage earnings to at least meet consensus. This shows up in the empirical distribution of earnings surprises as bunching in the interval from zero to one cent difference between reported earnings and consensus. To capture earnings management of this kind, we follow the literature and code whether a firm's reported earnings beat consensus by up to 1 cent. Consistent with the two accruals-based measures, we find no evidence that firms become any more (or less) likely to meet-or-beat consensus when they become EDGAR filers.

[23] Chen, Miao, and Shevlin (2015) construct their DQ measure at an annual frequency. We adapt their measure to the quarterly frequency used in our empirical design. Note that our inclusion of fiscal-quarter fixed effects controls for any potential seasonality in disclosure quality over the course of the fiscal year.

We tentatively conclude that the changes in analyst behavior we document do not appear in any obvious way to have been triggered by changes in firm fundamentals or voluntary disclosure policies. Instead, it seems reasonable to surmise, it is the cost and timeliness of access to mandatory disclosures that change as a firm joins the EDGAR system.

3.2 Broker Channel

Inclusion in EDGAR implies free, timely, and equal access to mandatory disclosures not just for investors but also for analysts. Analyst behavior might then change for non-strategic reasons, as analysts working for brokerage firms without (expensive) subscriptions to electronic data feeds from commercial vendors gain free, timely, and equal access to corporate filings alongside investors. As a result, analyst forecasts might become less optimistic and more accurate for the simple reason that they gain timely access to corporate filings.

Clearly, this argument applies only to some analysts (those without access to data feeds) and not to others (those already enjoying access to timely data from commercial vendors). In other words, the argument implies a heterogeneous treatment effect whereby inclusion in EDGAR changes analyst behavior more for some analysts than for others.

Data on which brokerage firms subscribed to data feeds pre-EDGAR are not publicly available. However, it seems reasonable to assume that there would have been substantially economies of scale in data-feed costs. If so, larger brokers and those covering a larger fraction of the universe of firms could have spread their data-feed costs over a larger quantity of output and so would have been more likely to subscribe to data feeds than smaller ones, all else equal.

In Table 9, we report the results of triple-difference specifications which show that the reductions in optimism and inaccuracy observed around EDGAR inclusion are *not* concentrated among analysts working for smaller brokers or for brokers covering only a small part of the stock universe. We find significant reductions in optimism and inaccuracy regardless of broker size and coverage. Indeed, if anything, we find significantly *larger* reductions among analysts at larger brokers and at brokers covering more of the stock universe.

We use two measures of a broker's size: its annual fee revenue from equity underwriting and the number of analysts it employs. We use two measures of a broker's breadth of coverage: the fraction of all stocks listed in the U.S., by either number or market capitalization, that the broker's analysts cover. All four variables are measured as of the quarter before a firm joins EDGAR. Panel A of Table 9 reports triple-diff estimates for optimism in short-term and long-term forecasts, while Panel B reports triple-diff estimates for inaccuracy in short-term and long-term forecasts. We thus estimate 16 triple-diff regressions.

The double-diff estimate ($treated \times post$) is negative in each of the 16 regressions, and it is statistically significant throughout, except in column 1. In other words, we find significant reductions in optimism and inaccuracy regardless of broker size and coverage in 14 of the 16 regressions. A closer look at column 1 in either panel is instructive. The insignificant double-diff estimate implies that analysts working for brokers without equity underwriting fee revenue do not significantly alter their short-term forecasts. The triple-diff estimate ($treated \times post \times equity\ fees$), in turn, is negative, implying that short-term optimism and inaccuracy around EDGAR inclusion are reduced by more the *larger* the brokerage firm (as measured by equity underwriting fee revenue). The negative sign of the triple-diff coefficient directly runs counter to the hypothesis that analyst behavior changed around EDGAR inclusion for the simple reason that analysts themselves gained free, timely, and equal access to corporate filings alongside investors. In fact, nowhere do we find any evidence that analysts working for smaller brokers changed their forecast behavior by more than did analysts working for larger brokers: every one of the 16 triple-diff coefficients in Table 9 is negative, and six of them are significantly negative.

3.3 Strategic Analyst Channel

We view the results reported in Table 9 as difficult to reconcile with the hypothesis that it is access to corporate disclosures *by analysts* that explains the observed changes in analyst forecasts around EDGAR inclusion. A leading alternative explanation, motivated by the large body of literature studying analysts, is that by providing free, timely, and equal access to corporate disclosures *to investors*, EDGAR inclusion constrains analysts' strategic behavior. In this section, we report evidence consistent with a strategic analyst channel.

We begin by exploring how cross-analyst variation in optimism and accuracy before EDGAR inclusion affects the size of the changes in optimism and accuracy when a stock joins EDGAR. Figures 5 and 6 graph estimates from quantile DD regressions for optimism and inaccuracy, respectively, at (a) the stock level and (b) the analyst-stock level, separately for short- and long-term forecasts. In all eight graphs, there is a pronounced negative slope, such that the reduction in optimism and inaccuracy around EDGAR inclusion is larger, the larger the initial level of optimism and inaccuracy, respectively. This is true both within stock and within an analyst-stock pair. The variation in the economic magnitude of the effects across deciles is large. To illustrate, while average short-term forecast optimism in Figure 5(a) decreases by an average of 28% from the pre-EDGAR mean in the decile of stocks with the lowest initial optimism (p=0.001), it decreases by 86% in the decile of stocks with the highest initial optimism (p=0.015).

These quantile DD results are consistent with analysts changing a strategic dimension of their forecast behavior when a stock joins EDGAR. While alternative interpretations are no doubt possible, any alternative interpretation would need to involve an omitted variable that correlates with strategic behavior – i.e., with pre-EDGAR optimism and inaccuracy – and yet is not itself strategic. We struggle to think what such an omitted variable might be.

Next, we exploit heterogeneity across analysts in their incentives to strategically bias their earnings forecasts. This reveals that it is the analysts who had the greatest incentives to behave strategically pre-EDGAR who change their behavior the most post-EDGAR.

Using triple-diff regressions, we investigate cross-analyst variation in four variables which the literature associates with strategic analyst behavior. The first interaction variable proxies for an analyst's reputation capital. If free, timely, and equal access to corporate filings allows investors to more easily verify analyst reports, and assuming investors can sanction analysts for issuing biased and inaccurate reports, we expect EDGAR inclusion to raise the cost to analysts of behaving strategically. Some analysts have more to lose than others as investors' verification costs decline. In particular, we expect "star" analysts to moderate their behavior by more when a stock joins EDGAR than non-rated analysts.[24] The results, reported in Table 10, support this prediction. The triple interaction $treated \times post \times star\ analyst$ has a negative and significant coefficient, whether we look at optimism or inaccuracy and for both short-term and long-term forecasts. This implies that star analysts reduce optimism and inaccuracy by significantly more when a stock joins EDGAR than do non-rated analysts (who, it is worth noting, also reduce optimism and inaccuracy significantly).

The second interaction variable seeks to capture variation in the reduction in verification costs. It seems reasonable to assume that retail investors experienced a larger reduction in the cost of accessing corporate filings (and so in verification costs) than did institutional investors. If so, we expect analysts catering to retail clients to moderate their behavior by more when a stock joins EDGAR than analysts serving institutional clients. We measure a brokerage firm's retail focus as the share of its registered representatives who are licensed to provide advice to retail clients, using data gathered from the Securities Industry Association's yearbooks. The triple-diff

[24] Stickel (1992) and Fang and Yasuda (2009) find that star analysts have more reputational capital at stake. By one estimate, the compensation of star analysts is 61% higher on average than that of their peers (Groysberg, Healy, and Maber 2011).

results reported in Table 10 support our prediction. The triple interaction $treated \times post \times retail\ focus$ has an economically large negative coefficient in all four specifications, which is statistically significant in three of them. This confirms that the post-EDGAR reduction in optimism and inaccuracy is larger the greater a broker's focus on retail clients.

The third interaction variable seeks to capture a much debated source of distorted incentives: conflicts of interest stemming from a broker's desire to keep an investment banking client happy (Michaely and Womack 1999, Ljungqvist, Marston, and Wilhelm 2006, Ljungqvist et al. 2007).[25] To capture such conflicts, we code as "affiliated" those analysts whose brokerage firm provided debt or equity underwriting services to the focal company in the three years before joining EDGAR. If EDGAR inclusion moderates analysts' strategic behavior, we expect a larger reduction in optimism and inaccuracy post-EDGAR among affiliated analysts than among unaffiliated analysts. Consistent with this prediction, the triple interaction $treated \times post \times affiliated\ analyst$ has an economically large negative coefficient in all four specifications, though it is statistically significant only for short-term optimism (p=0.016).

Our final interaction variable exploits cross-analyst variation in the propensity to make bold forecasts. Both herding and its antithesis – making bold forecasts to stand out from the crowd – are viewed as strategic behavior (see Trueman 1994 and Bernhardt, Campello, and Kutsoati 2006, respectively). We follow Hong, Kubik, and Solomon (2000) and measure each analyst's propensity to make bold forecasts across the stocks she covers and relative to other analysts covering those stocks. Consistent with Bernhardt, Campello, and Kutsoati's argument that analysts strategically "anti-herd," we find that bolder analysts reduce their optimism and

[25] Alleged conflicts of interest between research and investment banking were the stated reason for the 2003 "Global Settlement" between the New York State Attorney General and twelve large investment banks, requiring structural separation of research and investment banking. It is also a key motivation for the parts of the European Union's MiFID II Directive that "unbundle" the provision of research by investment banks.

inaccuracy by more than do less bold analysts. The coefficients for the triple interaction $treated \times post \times boldness$ are negative in all four specifications, with marginal significance (ranging from p=0.053 to p=0.099) in three of them.

In sum, the triple-diff results in Table 10 suggest that EDGAR inclusion has a larger effect on the behavior of those analysts the literature regards as most susceptible to strategic considerations. We view these results as consistent with the interpretation that free, timely, and equal access to corporate filings curtails a strategic component of analyst behavior.

4. Conclusions

A rich literature in finance and accounting documents that sellside analysts engage in strategic behavior rather than providing objective information to buyside clients: analysts are prone to biasing earnings forecasts, to inflating recommendations, and to suspending coverage rather than issuing unflattering reports when a company is doing poorly. We provide evidence that permits the interpretation that analysts' strategic behavior is constrained by investors' ability to verify analyst reports. Using a plausibly exogenous, randomly assigned natural experiment, we find that free, timely, and equal access to firms' mandatory disclosures results in analysts making earnings forecasts that are less optimistic and more accurate. The shock thins the ranks of analysts covering a given firm as analysts whose reports add little value when corporate filings become freely available exit, consistent with the model of Dugast and Foucault (2018). It also results in analyst forecasts moving share prices by less as investors gain access to better conditioning information. Overall, free, timely, and equal access to corporate filings improves market quality, as measured by liquidity, volatility, and earnings surprises.

The natural experiment we use is the SEC's rollout of the EDGAR system in the first half of the 1990s. We take seriously the possibility that EDGAR could have changed analyst behavior because it improved *analysts*', rather than *investors*', access to corporate filings but find no support for it. We also investigate the possibility that analysts may have change their behavior because firms responded to being included in EDGAR by changing their reporting practices but again find no support for it. Instead, based on the finding that behavior changed the most when analysts had the greatest strategic incentive to change their behavior, we favor the interpretation that analysts changed a strategic component of their forecasts.

Our findings highlight the importance of verification costs in the game analysts and investors play. Cheap-talk models, which are often used to study this game, typically assume that verification costs are infinite. The nature of our natural experiment is such that it can plausibly be interpreted to vary verification costs for a subset of investors (i.e., retail and small institutional investors), from arguably something approaching infinity to something much closer to zero. Seen through this lens, we interpret the observed changes in analyst behavior as indicating that free, timely, and equal access to corporate information improves investors' ability to verify analyst reports ex post, which in turn constrains analysts' strategic behavior ex ante. Our interpretation fits well with theory models that view reputational concerns as helping to discipline analysts and encouraging truthful communication (Benabou and Laroque 1992, Meng 2015).

Our findings also speak to the interplay between mandatory disclosure and information intermediaries such as analysts in shaping firms' external information environments. We find that greater access to mandatory disclosure and analyst coverage are substitutes to some extent, consistent with theoretical models showing that greater disclosure could discourage private information production (Verrecchia 1982, Gao and Liang 2013, Banerjee, Davis, and Gondhi 2018). We leave to future research whether the partial crowding out of information dissemination by analysts we document improves investor welfare on net.

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Appendix A: Variable Definitions

Stock-level measures

 # analysts is the number of analysts who issue earnings-per-share forecasts for a firm in a fiscal quarter, counting unique I/B/E/S analyst identifiers (I/B/E/S unadjusted detail file variable $analys$).

Abnormal volume is the quarterly volume ratio constructed following Barber and Odean (2008). It is defined as $\left(V_{i,q}/V_i\right)/\left(V_{m,q}/V_m\right)$, where $V_{i,q}$ is average daily trading volume for firm i in fiscal quarter q, V_i is average daily trading volume for firm i in fiscal quarter $q-1$, $V_{m,q}$ is average daily market trading volume in quarter q, and V_m is average daily market trading volume in quarter $q-1$. Trading volume is defined as the number of shares traded (CRSP variable vol) multiplied by the daily closing price (CRSP variable prc). Trading volume on Nasdaq is adjusted using the Gao and Ritter (2010) procedure. Market trading volume is calculated using all CRSP common stocks (share code 10 or 11).

Abnormal volume (retail) is the quarterly turnover by the retail customers of a large discount brokerage firm using the data of Barber and Odean (2000). It is defined as the total number of buy and sell trades divided by the number of shares outstanding at the previous quarter-end (CRSP variable $shrout$).

AIM is the natural log of one plus Amihud's (2002) illiquidity measure. We use daily CRSP data to calculate the ratio of absolute return to dollar volume, $[1{,}000{,}000 \times |ret|/(|prc| \times vol)]$, for each trading day in a fiscal quarter. We then average over the quarter and take logs. Trading volume on Nasdaq is adjusted using the Gao and Ritter (2010) procedure.

DA (Jones) is firm i's discretionary accruals in fiscal quarter t obtained from a modified Jones model following Dechow, Sloan, and Sweeney (1995). The modified Jones model is specified as $TA_{iq}/ASSET_{iq-1} = \beta_0 + \beta_1\, 1/ASSET_{iq-1} + \beta_2\, \Delta REV_{iq}/ASSET_{iq-1} + \beta_3\, PPE_{iq}/ASSET_{iq-1} + \varepsilon_{iq}$, where TA_{iq} is total accruals, defined as earnings before extraordinary items and discontinued operations (Compustat variable ibq) minus operating cash flows (Compustat variable $oancfy$), $ASSET_{iq-1}$ is lagged total assets (Compustat variable atq), ΔREV_{iq} is the change in quarterly revenue (Compustat variable $saleq$), and PPE_{iq} is gross property, plant, and equipment (Compustat variable $ppegtq$). Jones discretionary accruals is defined as $DA_{iq} = \left(TA_{iq}/ASSET_{iq-1}\right) - NA_{iq}$, where $NA_{iq} = \widehat{\beta_0} + \widehat{\beta_1} + 1/ASSET_{iq-1} + \widehat{\beta_2}\left(\Delta REV_{iq} - \Delta AR_{iq}\right)/ASSET_{iq-1} + \widehat{\beta_3}\, PPE_{iq}/ASSET_{iq-1}$ and AR_{iq} is accounts receivable (Compustat variable $rectq$).

DA (Kothari) is the performance-matched discretionary accruals in a fiscal quarter following Kothari, Leone, and Wasley (2005), defined as a firm's discretionary accruals from a modified Jones model minus that of a matched firm in the same Fama-French 48 industry with the closest return on assets.

Dispersion is the standard deviation of analysts' earnings forecasts made in fiscal quarter t (I/B/E/S variable $stdev$), scaled by the end-of-quarter stock price (CRSP variable prc). I/B/E/S data are obtained from the unadjusted summary history files. Short-term dispersion is based on forecasts made for fiscal quarter $t+1$ ($fpi = 7$); long-term dispersion is based on forecasts made for the current fiscal year ($fpi = 1$). See Lehavy, Li, and Merkley (2011) for further details.

DQ is a firm's quarterly "disclosure quality" score which captures the level of disaggregation in its financial reporting by counting the number of non-missing Compustat line items, computed separately for the income statement and the balance sheet and averaged to the firm level. See Chen, Miao, and Shevlin (2015) for further details.

Effective tick is the quarterly average of Goyenko, Holden, and Trzcinka's (2009) effective tick measure. Using daily CRSP data (CRSP variables prc and vol) and based on end-of-day price clustering, we calculate an average effective spread over the quarter as the probability-weighted average of each effective spread size deflated by the stock price.

ROA is the firm's diluted quarterly earnings per share (Compustat variable $epsfxq$), scaled by the previous quarter-end stock price (CRSP variable prc).

Fraction zero-return is the fraction of trading days with zero or missing returns in a fiscal quarter. See Lesmond, Ogen, and Trzcinka (1999) and Goyenko, Holden, and Trzcinka (2009) for further details.

Inaccuracy is the average absolute difference between analysts' earnings forecasts and realized earnings for a firm in a fiscal quarter. Following Hong and Kubik (2003), we compute, for each analyst making an earnings forecast in a fiscal quarter, the absolute difference between realized earnings and the forecast, scaled by the previous fiscal quarter-end share price (CRSP monthly file variable prc). We are careful to compare diluted forecasts to diluted earnings (Compustat variables $epsfxq$ and $epsfx$ for quarterly and annual earnings, respectively) and primary forecasts to primary earnings (Compustat variables $epspxq$ and $epspx$ for quarterly and annual earnings, respectively). We then average the absolute differences across analysts following a firm. Short-term forecast inaccuracy is based on forecasts made for the next fiscal quarter ($fpi = 7$); long-term inaccuracy is based on forecasts made for the current fiscal year ($fpi = 1$).

Informativeness is the fraction of cumulative daily absolute abnormal returns that can be attributed to analyst forecasts in a fiscal quarter. Following Lehavy, Li, and Merley (2011) and Merkley, Michaely, and Pacelli (2017), the measure is defined as $\sum_{d=1}^{NREVS}\left|R_{i,d} - Dec\ ret_{i,d}\right| / \sum_{d=1}^{D}\left|R_{i,d} - Dec\ ret_{i,d}\right|$. $NREVS$ is the number of trading days for which there is at least one analyst forecast in the I/B/E/S detail history file. D is the number of trading days in a quarter. $R_{i,d}$ is the daily return of firm i on day d (CRSP variable ret). $Dec\ ret_{i,d}$ is the CRSP size-decile portfolio return (variable $decret$).

Idiosyncratic volatility is the standard deviation of regression residuals from a Fama-French three-factor model using daily stock returns (CRSP variable ret) in a firm-fiscal quarter, measured following Ang et al. (2006).

Meet-or-beat is an indicator variable set equal to one if a firm's EPS is both greater than and within 1 cent of the median analyst's earnings forecast.

Optimism is the average difference between analysts' earnings forecasts and realized earnings for a firm in a fiscal quarter. Following Abarbanell and Lehavy (2003), we compute, for each analyst making an earnings forecast in a fiscal quarter, the difference between realized earnings and the forecast, scaled by the previous fiscal quarter-end share price (CRSP monthly file variable prc). We are careful to compare diluted forecasts to diluted earnings (Compustat variables $epsfxq$ and $epsfx$ for quarterly and annual earnings, respectively) and primary forecasts to primary earnings (Compustat variables $epspxq$ and $epspx$ for quarterly and annual earnings, respectively). We then average the differences across analysts following a firm. Short-term forecast optimism is based on forecasts made for the next fiscal quarter ($fpi = 7$); long-term optimism is based on forecasts made for the current fiscal year ($fpi = 1$).

Price convergence is the absolute cumulative abnormal return around a firm's quarterly earnings announcement. Following Heflin, Subramanyam, and Zhang (2003), we compute $\left|\prod_{t=-30}^{2}\left(1 + AR_{i,q,t}\right) - 1\right|$ for each firm i and fiscal quarter q, from 30 days before the earnings announcement date to two days after. Daily abnormal returns are CAPM-adjusted. Earnings announcement dates are from Compustat (variable rdq).

$\boldsymbol{SUE}$ is standardized unexpected earnings. We code earnings surprises following Barron, Byard, and Yu (2008). SUE for firm i in fiscal quarter q announced in fiscal quarter $q + 1$ is defined as the absolute difference between I/B/E/S reported earnings per share (I/B/E/S detail history variable $value$) and the median outstanding analyst earnings forecast made for quarter q earnings, scaled by the firm's quarter $q - 1$ quarter-end share price. Analyst earnings forecasts are taken from the I/B/E/S unadjusted detail history file. Share prices are taken from the CRSP monthly file (variable prc).[26]

[26] Note that the difference between SUE and *inaccuracy* is in the timing. SUE is a measure of investors' surprise at

Volatility at earnings announcement is the annualized daily return volatility in a three-day window centered on a firm's earnings announcement date, following Kelly and Ljungqvist (2012). Earnings announcement dates are from Compustat (variable rdq). Daily returns are from CRSP (variable ret).

Volume at earnings announcement is the sum of CRSP daily log trading volume (variable vol) in a three-day window centered on a firm's earnings announcement date. Earnings announcement dates are from Compustat (variable rdq). CRSP trading volume is adjusted using the Gao and Ritter (2010) algorithm.

Analyst/stock-level measures

Affiliated analyst is an indicator variable set equal to one if the analyst works for a brokerage house that underwrote any of the firm's equity or debt issues in the previous three years. In our triple-diff specifications, affiliation is measured in the fiscal quarter before the focal firm joins EDGAR.

Boldness is the absolute difference between an analyst's most recent earnings forecast for a firm in the first two months of a quarter and the average consensus earnings forecast made by all other analysts covering the firm. Short-term forecast boldness is based on forecasts made for the next fiscal quarter ($fpi = 7$); long-term boldness is based on forecasts made for the current fiscal year ($fpi = 1$).

Coverage is an indicator variable set equal to one if an analyst issues an earnings forecast for firm i in fiscal quarter t, according to the I/B/E/S unadjusted detail history file.

Inaccuracy is the average absolute difference between an analyst's earnings forecasts and realized earnings for a firm in a fiscal quarter. Following Hong and Kubik (2003), we compute the absolute difference between realized earnings and each forecast the analyst makes for that firm that quarter, scaled by the firm's previous fiscal quarter-end share price (CRSP monthly file variable prc). We are careful to compare diluted forecasts to diluted earnings (Compustat variables $epsfxq$ and $epsfx$ for quarterly and annual earnings, respectively) and primary forecasts to primary earnings (Compustat variables $epspxq$ and $epspx$ for quarterly and annual earnings, respectively). We then average the absolute scaled differences for that analyst for that firm in that quarter. Short-term forecast inaccuracy is based on forecasts made for the next fiscal quarter ($fpi = 7$); long-term inaccuracy is based on forecasts made for the current fiscal year ($fpi = 1$).

Optimism is the average difference between an analyst's earnings forecasts and realized earnings for a firm in a fiscal quarter. Following Abarbanell and Lehavy (2003), we compute the difference between realized earnings and each forecast the analyst makes for that firm that quarter, scaled by the firm's previous fiscal quarter-end share price (CRSP monthly file variable prc). We are careful to compare diluted forecasts to diluted earnings (Compustat variables $epsfxq$ and $epsfx$ for quarterly and annual earnings, respectively) and primary forecasts to primary earnings (Compustat variables $epspxq$ and $epspx$ for quarterly and annual earnings, respectively). We then average the scaled differences for that analyst for that firm in that quarter. Short-term forecast optimism is based on forecasts made for the next fiscal quarter ($fpi = 7$); long-term optimism is based on forecasts made for the current fiscal year ($fpi = 1$).

Analyst-level measures

Relative boldness is a measure of the analyst's boldness relative to the other analysts coverage her stocks, constructed following Hong, Kubik, and Solomon (2000). For each stock an analyst covers, we first measure the deviation from consensus, defined as the absolute difference between the analyst's forecast and the average consensus of all other analysts covering the firm. Next, we calculate a boldness score, $100 - [(rank - 1)/(number\ of\ analysts - 1)] \times 100$, where $rank$ is determined by sorting deviations from consensus of

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the time earnings are announced relative to the analyst forecasts available to investors at that time. *Inaccuracy* is a measure of analysts' forecast errors, measured in the quarter a forecast is made relative to the *future* earnings realization.

analysts covering a particular stock with the best analyst assigned a $rank$ of 1. Finally, we average the analyst's scores across her stocks. In our triple-diff specifications, relative boldness is measured in the fiscal quarter before the focal firm joins EDGAR.

Star analyst is an indicator variable set equal to 1 if the analyst is ranked an all-star analyst by the *Wall Street Journal* in the *Journal*'s June rankings immediately preceding the fiscal quarter before the focal firm joins EDGAR.

Broker-level measures

$\boldsymbol{Coverage}(\$)$ is the total market capitalization of all firms covered by a broker's analysts divided by the total market capitalization of all firms in I/B/E/S, both measured in the fiscal quarter before the focal firm joins EDGAR.

$\boldsymbol{Coverage}(\#)$ is the number of all firms covered by a broker's analysts divided by the total number of firms in I/B/E/S, measured in the fiscal quarter before the focal firm joins EDGAR.

$\boldsymbol{Equity\ fees}$ is the natural log of a broker's annual revenue from equity underwriting (across all its clients), measured in the fiscal quarter before the focal firm joins EDGAR.

$\#\ \boldsymbol{analysts}$ is the number of analysts working at each broker according to I/B/E/S, measured in the fiscal quarter before the focal firm joins EDGAR.

$\boldsymbol{Retail\ focus}$ is the ratio of the number of retail representatives to the total number of registered representatives at each broker, measured in the fiscal quarter before the focal firm joins EDGAR. The data are taken from the Securities Industry Association's yearbooks.

Figure 1. Timeline of EDGAR Implementation.

The figure shows the major milestones in the SEC's implementation of EDGAR. SEC Release 33-6977 is the SEC's announcement of its plan to require all registered firms to submit their filings electronically, ultimately in ten waves. The release contains the phase-in dates for four "significant test groups," to be followed by a six-month evaluation period in the first half of 1994 leading to a final rule concerning the phase-in dates for the remaining firms. SEC Release 33-7122 contains final rules on EDGAR implementation, including the dates of the remaining six waves. The National Science Foundation announced on October 22, 1993 funding for a project to make all EDGAR filings available for free online, hosted by New York University's Stern School of Business. The SEC took over online access in October 1995.

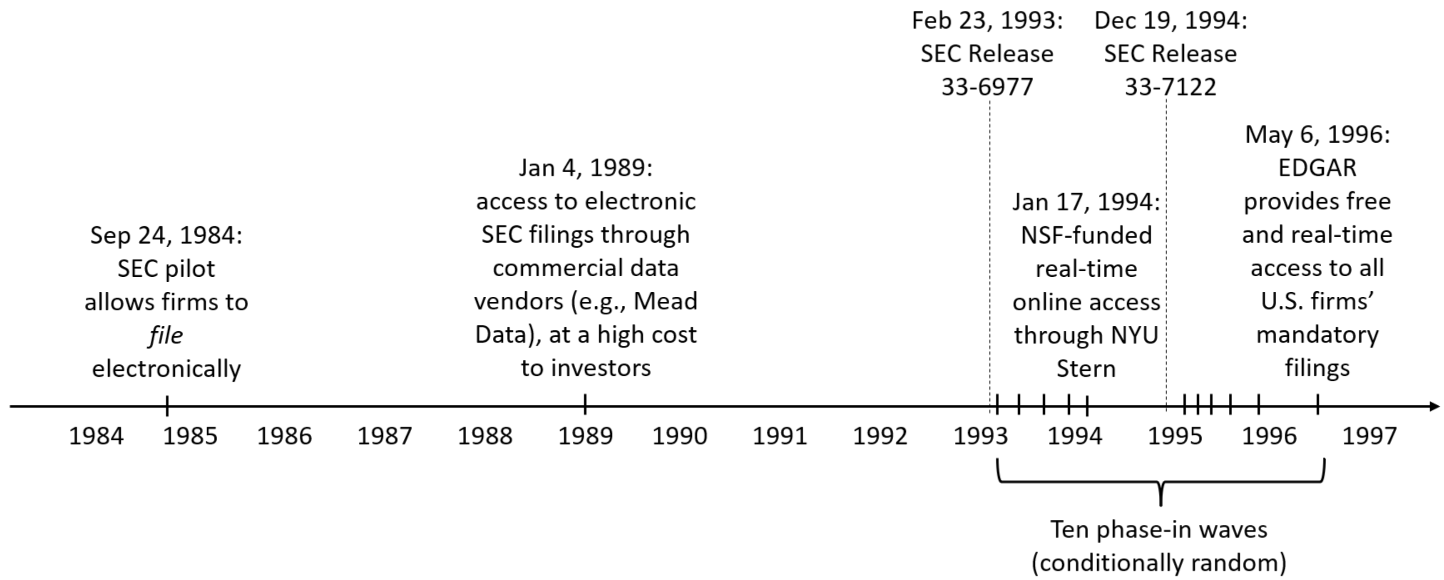
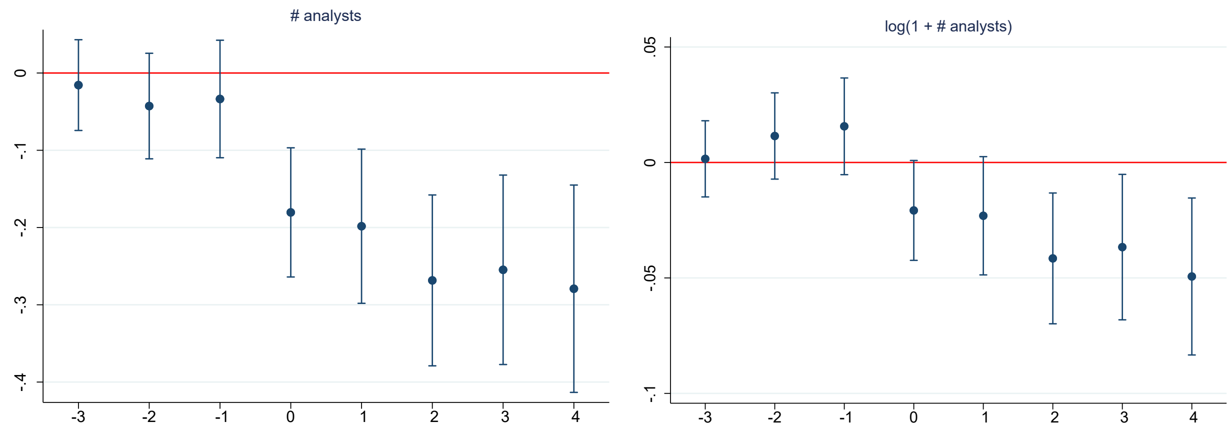
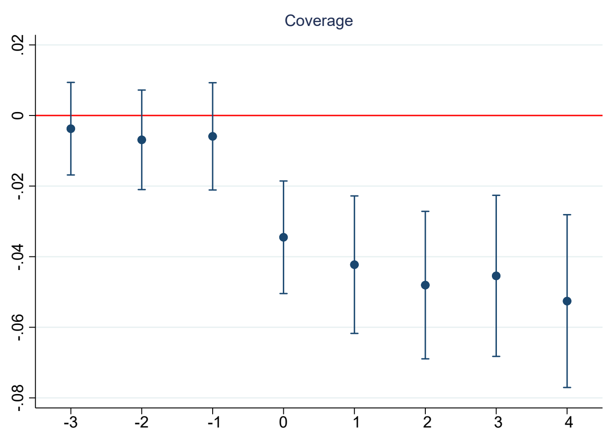


Figure 2. Testing for Diverging Pre-trends: Analyst coverage.
The figure graphs difference-in-differences estimates of the effects of inclusion in EDGAR on analyst coverage. Treated firms are those included in EDGAR at time 0; control firms are nearest-neighbor propensity-scored matched on equity market capitalization (in levels and logs), number of analysts (in logs and lags), and fiscal quarter using a 0.05 caliper. We include data from a nine-fiscal quarter window centered on the fiscal quarter in which a treated firm's EDGAR inclusion takes place. All specifications are estimated using OLS and include controls (the one-quarter lag of log market cap) and fixed effects for calendar-quarter, fiscal-quarter, and firm (or analyst-firm). For variable definitions and details of their construction see Appendix A.

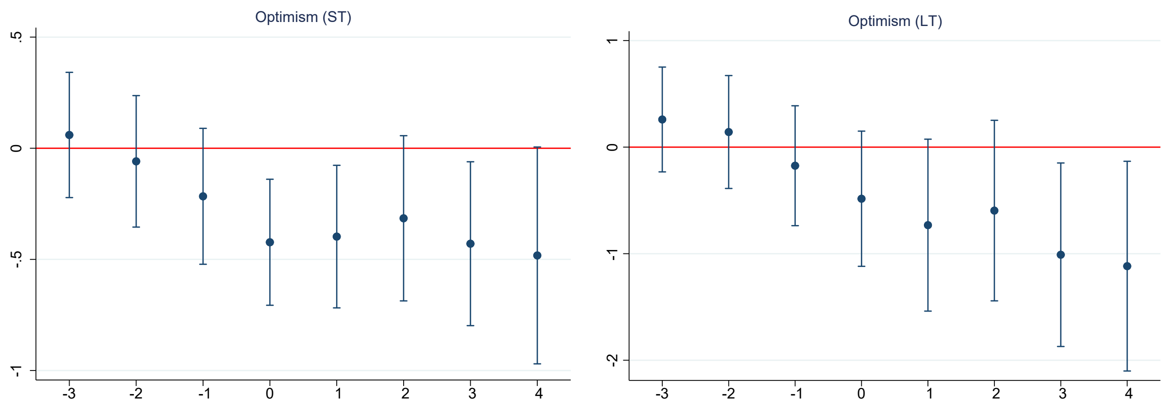


(a) Stock level

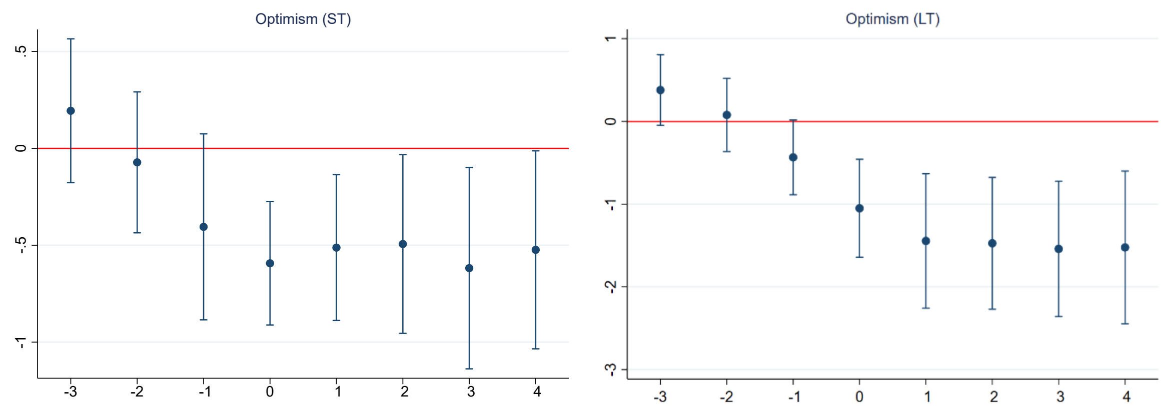


(b) Analyst-stock level

Figure 3. Testing for Diverging Pre-trends: Forecast Optimism.
The figure graphs difference-in-differences estimates of the effects of inclusion in EDGAR on forecast optimism. Treated firms are those included in EDGAR at time 0; control firms are nearest-neighbor propensity-scored matched on equity market capitalization (in levels and logs), number of analysts (in logs and lags), and fiscal quarter using a 0.05 caliper. We include data from a nine-fiscal quarter window centered on the fiscal quarter in which a treated firm's EDGAR inclusion takes place. All specifications are estimated using OLS and include controls (the one-quarter lag of log market cap) and fixed effects for calendar-quarter, fiscal-quarter, and firm (or analyst-firm). For variable definitions and details of their construction see Appendix A.



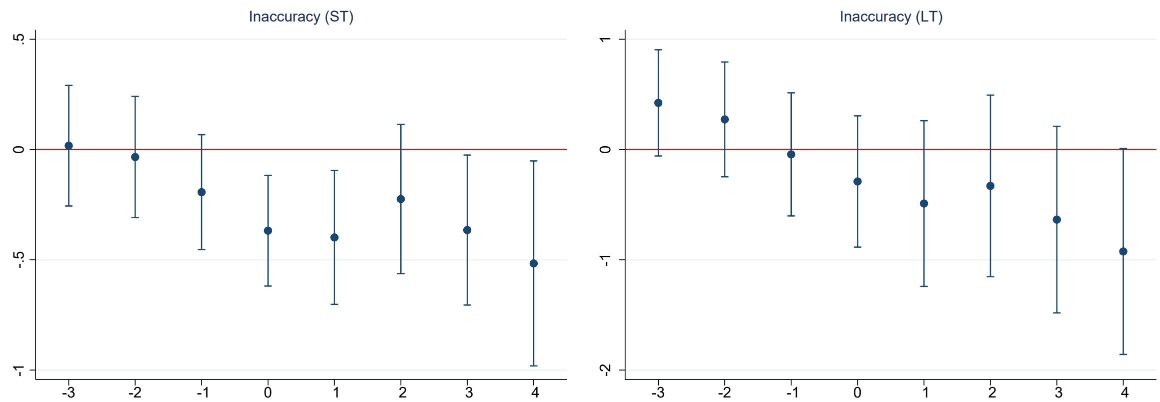
(a) Stock level



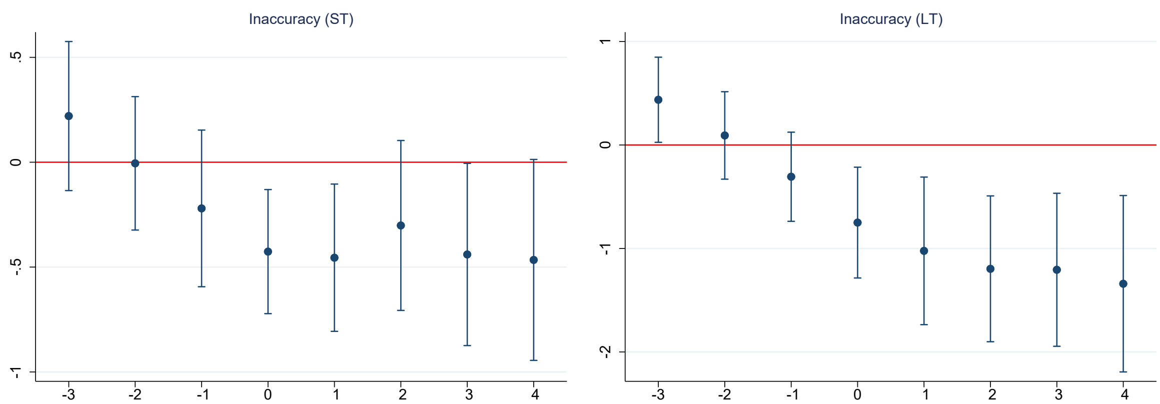
(b) Analyst-stock level

Figure 4. Testing for Diverging Pre-trends: Forecast Inaccuracy.

The figure graphs difference-in-differences estimates of the effects of inclusion in EDGAR on forecast inaccuracy. Treated firms are those included in EDGAR at time 0; control firms are nearest-neighbor propensity-scored matched on equity market capitalization (in levels and logs), number of analysts (in logs and lags), and fiscal quarter using a 0.05 caliper. We include data from a nine-fiscal quarter window centered on the fiscal quarter in which a treated firm's EDGAR inclusion takes place. All specifications are estimated using OLS and include controls (the one-quarter lag of log market cap) and fixed effects for calendar-quarter, fiscal-quarter, and firm (or analyst-firm). For variable definitions and details of their construction see Appendix A.



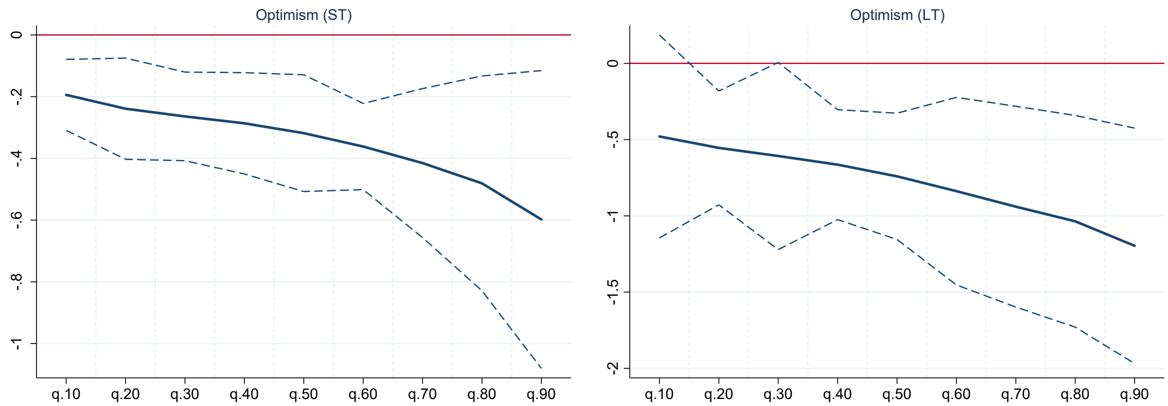
(a) Stock level



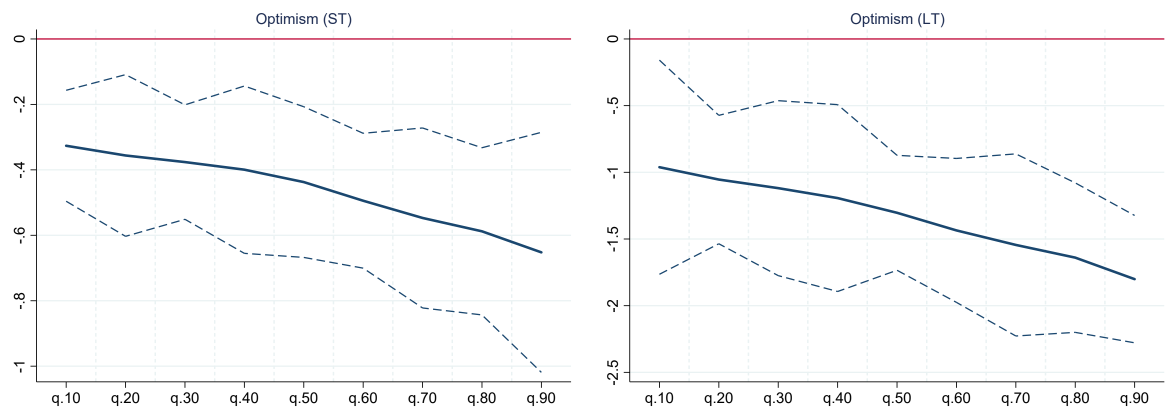
(b) Analyst-stock level

Figure 5. Quantile regressions: Forecast Optimism.

The figure graphs quantile-regression DD estimates of the effects of inclusion in EDGAR on forecast optimism. Treated firms are those included in EDGAR; control firms are nearest-neighbor propensity-scored matched on equity market capitalization (in levels and logs), number of analysts (in logs and lags), and fiscal quarter using a 0.05 caliper. All specifications are estimated using OLS and include controls (the one-quarter lag of log market cap) and fixed effects for calendar-quarter, fiscal-quarter, and firm (or analyst-firm). The dashed lines indicate 95% confidence intervals. For variable definitions and details of their construction see Appendix A.



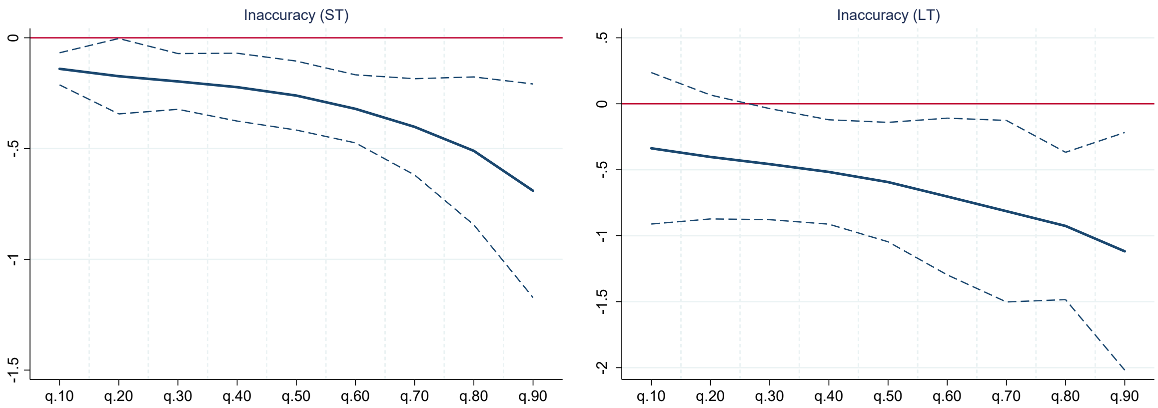
(a) Stock level



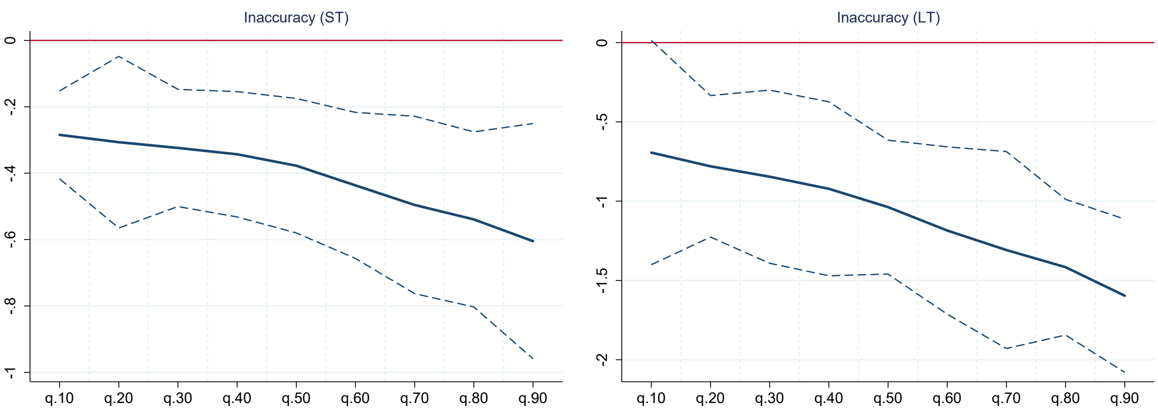
(b) Analyst-stock level

Figure 6. Quantile regressions: Forecast Inaccuracy.

The figure graphs quantile-regression DD estimates of the effects of inclusion in EDGAR on forecast inaccuracy. Treated firms are those included in EDGAR; control firms are nearest-neighbor propensity-scored matched on equity market capitalization (in levels and logs), number of analysts (in logs and lags), and fiscal quarter using a 0.05 caliper. All specifications are estimated using OLS and include controls (the one-quarter lag of log market cap) and fixed effects for calendar-quarter, fiscal-quarter, and firm (or analyst-firm). The dashed lines indicate 95% confidence intervals. For variable definitions and details of their construction see Appendix A.



(a) Stock level



(b) Analyst-stock level

Table 1. EDGAR Phase-in Waves.

The table provides a breakdown of the universe of listed U.S. firms and of the sample of treated firms by EDGAR phase-in wave. Listed U.S. firms are those listed on the NYSE, NASDAQ, or AMEX with CRSP share codes of 10 or 11. Treated firms require the existence of a valid control firm using a nearest-neighbor propensity-score method matching on equity market capitalization (in levels and logs), analyst coverages (in logs and lags), and fiscal quarter. Only matches in the common support are considered valid, using a 0.05 caliper. Market cap is measured in the fiscal quarter prior to inclusion in EDGAR.

			All listed U.S. firms		Treated firms	
Phase-in wave no.	SEC designation	Phase-in date	No. of firms	Mean market cap ($m)	No. of firms	Mean market cap ($m)
1	CF-01	April 26, 1993	105	8,418.5	23	303.9
2	CF-02	July 19, 1993	405	4,450.6	64	844.4
3	CF-03	October 4, 1993	416	952.0	232	375.5
4	CF-04	December 6, 1993	599	326.7	507	195.6
5	CF-05	January 30, 1995	664	198.6	492	216.3
6	CF-06	March 6, 1995	566	91.4	467	86.6
7	CF-07	May 1, 1995	458	97.1	373	95.8
8	CF-08	August 7, 1995	246	79.1		
9	CF-09	November 6, 1995	132	191.1		
10	CF-10	May 6, 1996	905	356.9		
All			4,496	860.5	2,158	199.2

Table 2. Summary Statistics.

The table reports summary statistics for the variables used in our analysis, separately for treated and control firms measured in levels and changes in the quarter before treatment. Treated firms are those included in EDGAR; control firms are nearest-neighbor propensity-score matched on equity market capitalization (in levels and logs), analyst coverage (in logs and lags), and fiscal quarter using a 0.05 caliper. Boldness is measured at the analyst/firm/fiscal-quarter level. All other variables are measured at the firm/fiscal-quarter level. For variable definitions and details of their construction see Appendix A. The final two columns test whether the difference in pre-treatment changes between treated and controls is statistically significant.

	Pre-treatment levels						**Pre-treatment changes (from *t*-2 to *t*-1)**							
	Treated firms			Control firms			Treated firms			Control firms			Treated - Controls	
	# obs.	Mean	Std. dev.	# obs.	Mean	Std. dev.	# obs.	Mean	Std. dev.	# obs.	Mean	Std. dev.	Diff-erence	*t*-stat
Matching variables:														
Market capitalization ($m)	2,158	199.2	422.9	2,158	223.3	481.0	2,150	6.587	88.633	2,143	6.959	82.839	-0.372	-0.142
# of analysts	2,158	2.3	3.5	2,158	2.3	3.3	2,158	-0.027	1.167	2,158	-0.041	1.209	0.014	0.384
Analyst behavior:														
Optimism (short-term)	614	0.007	0.025	700	0.006	0.021	454	-0.002	0.035	515	-0.002	0.031	0.000	0.062
Optimism (long-term)	1,190	0.031	0.088	1,174	0.021	0.072	1,071	-0.006	0.051	1,043	-0.001	0.051	0.005	2.041
Inaccuracy (short-term)	614	0.012	0.024	700	0.010	0.020	454	-0.004	0.032	515	-0.003	0.029	0.000	-0.120
Inaccuracy (long-term)	1,190	0.042	0.091	1,174	0.031	0.072	1,071	-0.006	0.049	1,043	-0.003	0.050	-0.003	-1.193
Informativeness	1,204	0.071	0.071	1,190	0.072	0.068	1,082	-0.005	0.066	1,059	-0.001	0.063	-0.003	-1.176
Dispersion (short-term)	582	0.003	0.003	637	0.002	0.003	515	0.000	0.003	571	0.000	0.002	0.000	-0.618
Dispersion (long-term)	1,050	0.009	0.014	1,130	0.007	0.013	1,014	0.000	0.009	1,093	0.000	0.008	0.000	-0.498
Boldness (short-term)	610	0.004	0.008	661	0.003	0.007	207	0.000	0.000	197	0.000	0.000	0.000	0.344
Boldness (long-term)	2,619	0.006	0.012	2,492	0.005	0.009	1,315	0.000	0.000	1,142	0.000	0.000	0.000	-3.457
Market reaction:														
Abnormal volume	2,156	1.270	1.034	2,158	1.256	1.014	2,154	-0.003	1.571	2,158	-0.060	1.644	0.057	1.164
Abnormal volume (retail)	666	0.078	0.106	600	0.081	0.114	507	0.004	0.085	441	0.009	0.101	-0.005	-0.842
AIM	1,984	1.012	1.275	2,019	1.015	1.233	1,932	-0.054	0.455	1,978	-0.042	0.505	-0.011	-0.723
Effective tick	1,984	0.026	0.028	2,016	0.026	0.028	1,932	0.000	0.017	1,973	0.000	0.018	0.000	-0.716
Fraction zero-return	2,158	0.270	0.123	2,158	0.246	0.119	2,158	-0.008	0.094	2,158	-0.006	0.090	-0.003	-1.024
Idiosyncratic volatility	2,157	0.037	0.026	2,158	0.041	0.028	2,157	0.001	0.013	2,158	0.000	0.015	0.000	1.075

Table 3. Validating the Shock.

The table reports difference-in-differences estimates of the effects of inclusion in EDGAR on abnormal trading volume, trading liquidity, and volatility. Treated firms are those included in EDGAR; control firms are nearest-neighbor propensity-scored matched on equity market capitalization (in levels and logs), number of analysts (in logs and lags), and fiscal quarter using a 0.05 caliper. We include data from a nine-fiscal quarter window centered on the fiscal quarter in which a treated firm's EDGAR inclusion takes place. All specifications are estimated using OLS and include controls (the one-quarter lag of log market cap) and fixed effects for calendar-quarter, fiscal-quarter, and firm. For variable definitions and details of their construction see Appendix A. The coefficients in columns 4, 5, and 6 are multiplied by 100 for ease of exposition. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

	Abnormal volume		Liquidity			Volatility
	All trading	Retail trading	AIM	Effective tick	Fraction zero-return days	Idio-syncratic volatility
	(1)	(2)	(3)	(4)	(5)	(6)
Quarter of EDGAR inclusion	0.067**	0.007*	-0.003	-0.059	-0.677***	0.023
	0.028	*0.004*	*0.013*	*0.043*	*0.206*	*0.036*
Next four quarters	-0.010	0.002	-0.043**	-0.084*	-0.172	-0.089**
	0.027	*0.005*	*0.018*	*0.046*	*0.213*	*0.045*
Controls?	yes	yes	yes	yes	yes	yes
Calendar quarter FE?	yes	yes	yes	yes	yes	yes
Fiscal quarter FE?	yes	yes	yes	yes	yes	yes
Firm FE?	yes	yes	yes	yes	yes	yes
R-squared	13.3%	58.0%	88.8%	77.4%	70.1%	81.6%
No. of firms	4,315	2,367	4,204	4,202	4,316	4,315
No. of firm-quarters	37,274	11,197	34,708	34,670	37,299	37,281

Table 4. Extensive Margin: Analyst Coverage.

The table reports difference-in-differences estimates of the effects of inclusion in EDGAR on analyst coverage. Treated firms are those included in EDGAR; control firms are nearest-neighbor propensity-scored matched on equity market capitalization (in levels and logs), number of analysts (in logs and lags), and fiscal quarter using a 0.05 caliper. We include data from a nine-fiscal quarter window centered on the fiscal quarter in which a treated firm's EDGAR inclusion takes place. All specifications are estimated using OLS and include controls (the one-quarter lag of log market cap) and fixed effects for calendar-quarter, fiscal-quarter, and firm (or analyst-firm). For variable definitions and details of their construction see Appendix A. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

	Stock level		Analyst-stock level
	# analysts	log(1+# analysts)	Coverage
	(1)	(2)	(3)
Quarter of EDGAR inclusion	-0.160***	-0.028***	-0.030***
	0.031	*0.008*	*0.006*
Next four quarters	-0.221***	-0.044***	-0.042***
	0.041	*0.010*	*0.008*
Controls?	yes	yes	yes
Calendar quarter FE?	yes	yes	yes
Fiscal quarter FE?	yes	yes	yes
Firm FE?	yes	yes	
Analyst-Firm FE?			yes
R-squared	90.7%	89.3%	35.1%
No. of firms	4,316	4,316	2,481
No. of observations	37,299	37,299	157,776

Table 5. Analyst Behavior: Forecast Optimism and Inaccuracy.

The table reports difference-in-differences estimates of the effects of inclusion in EDGAR on forecast optimism and inaccuracy. Treated firms are those included in EDGAR; control firms are nearest-neighbor propensity-scored matched on equity market capitalization (in levels and logs), number of analysts (in logs and lags), and fiscal quarter using a 0.05 caliper. We include data from a nine-fiscal quarter window centered on the fiscal quarter in which a treated firm's EDGAR inclusion takes place. All specifications are estimated using OLS and include controls (the one-quarter lag of log market cap) and fixed effects for calendar-quarter, fiscal-quarter, and firm (or analyst-firm). For variable definitions and details of their construction see Appendix A. The coefficients are multiplied by 100 for ease of exposition. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

	Optimism				Inaccuracy			
	Stock level		Analyst-stock level		Stock level		Analyst-stock level	
	short-term	long-term	short-term	long-term	short-term	long-term	short-term	long-term
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Quarter of EDGAR inclusion	-0.355***	-0.490*	-0.504***	-1.012***	-0.291***	-0.378	-0.395***	-0.745***
	0.111	*0.259*	*0.133*	*0.249*	*0.099*	*0.236*	*0.123*	*0.220*
Next four quarters	-0.339***	-0.895**	-0.439***	-1.461***	-0.309***	-0.737***	-0.399***	-1.196***
	0.122	*0.352*	*0.151*	*0.369*	*0.112*	*0.331*	*0.139*	*0.331*
Controls?	yes	yes	yes	yes	yes	yes	yes	yes
Calendar quarter FE?	yes	yes	yes	yes	yes	yes	yes	yes
Fiscal quarter FE?	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE?	yes	yes			yes	yes		
Analyst-Firm FE?			yes	yes			yes	yes
R-squared	37.5%	57.8%	54.6%	66.3%	43.4%	64.0%	59.1%	70.5%
No. of firms	2,520	3,117	2,520	3,117	2,520	3,117	2,520	3,117
No. of observations	12,681	20,474	26,748	73,833	12,681	20,474	26,748	73,833

Table 6. Analyst Behavior: Informativeness, Dispersion, and Boldness.

The table reports difference-in-differences estimates of the effects of inclusion in EDGAR on forecast informativeness, dispersion, and boldness. Treated firms are those included in EDGAR; control firms are nearest-neighbor propensity-scored matched on equity market capitalization (in levels and logs), number of analysts (in logs and lags), and fiscal quarter using a 0.05 caliper. We include data from a nine-fiscal quarter window centered on the fiscal quarter in which a treated firm's EDGAR inclusion takes place. All specifications are estimated using OLS and include controls (the one-quarter lag of log market cap) and fixed effects for calendar-quarter, fiscal-quarter, and firm (analyst-firm). For variable definitions and details of their construction see Appendix A. The coefficients are multiplied by 100 for ease of exposition. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

	Informative-ness	Dispersion		Boldness	
		short-term	long-term	short-term	long-term
	(1)	(2)	(3)	(4)	(5)
Quarter of EDGAR inclusion	-0.809***	-0.007	-0.032	0.014	0.024
	0.178	*0.010*	*0.034*	*0.038*	*0.046*
Next four quarters	-0.884***	-0.037***	-0.140***	-0.081**	-0.130***
	0.183	*0.012*	*0.042*	*0.037*	*0.045*
Controls?	yes	yes	yes	yes	yes
Calendar quarter FE?	yes	yes	yes	yes	yes
Fiscal quarter FE?	yes	yes	yes	yes	yes
Firm FE?	yes	yes	yes		
Analyst-Firm FE?				yes	yes
R-squared	60.1%	68.7%	67.8%	71.1%	65.4%
No. of firms	3,116	1,898	2,550	1,418	2,193
No. of observations	20,878	11,082	18,986	13,917	43,738

Table 7. Effects on Investor: Net Precision.
The table reports difference-in-differences estimates of the effects of inclusion in EDGAR on measures of the net precision of investors' information sets: volatility at earnings announcements, trading volume at earnings announcements, standardized unexpected earnings (SUE), and price convergence. Treated firms are those included in EDGAR; control firms are nearest-neighbor propensity-scored matched on equity market capitalization (in levels and logs), number of analysts (in logs and lags), and fiscal quarter using a 0.05 caliper. We include data from a nine-fiscal quarter window centered on the fiscal quarter in which a treated firm's EDGAR inclusion takes place. All specifications are estimated using OLS and include controls (the one-quarter lag of log market cap) and fixed effects for calendar-quarter, fiscal-quarter, and firm. For variable definitions and details of their construction see Appendix A. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use *** and ** to denote significance at the 1% and 5% level, respectively.

	Volatility at earnings announcement (1)	Volume at earnings announcement (2)	SUE (3)	Price convergence (4)
Quarter of EDGAR inclusion	0.004	0.046	0.001	0.001
	0.015	*0.084*	*0.001*	*0.007*
Next four quarters	-0.027**	-0.202**	-0.002***	-0.019**
	0.014	*0.084*	*0.002*	*0.008*
Controls?	yes	yes	yes	yes
Calendar quarter FE?	yes	yes	yes	yes
Fiscal quarter FE?	yes	yes	yes	yes
Firm FE?	yes	yes	yes	yes
R-squared	40.6%	79.1%	57.2%	51.0%
No. of firms	4,118	4,002	1,981	3,927
No. of firm-quarters	31,495	27,607	10,463	13,137

Table 8. Firm Responses: Fundamentals and Reporting Quality.
The table reports difference-in-differences estimates of the effects of inclusion in EDGAR on firm fundamentals and reporting practices: return on assets (ROA), two measures of disclosure quality (DQ), discretionary accruals (DA), and the likelihood of meeting or beating analyst consensus ($meet\text{-}or\text{-}beat$). Treated firms are those included in EDGAR; control firms are nearest-neighbor propensity-scored matched on equity market capitalization (in levels and logs), number of analysts (in logs and lags), and fiscal quarter using a 0.05 caliper. We include data from a nine-fiscal quarter window centered on the fiscal quarter in which a treated firm's EDGAR inclusion takes place. All specifications are estimated using OLS and include controls (the one-quarter lag of log market cap) and fixed effects for calendar-quarter, fiscal-quarter, and firm. For variable definitions and details of their construction see Appendix A. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use *** and ** to denote significance at the 1% and 5% level, respectively.

	ROA (1)	DQ (2)	DA ($Jones$) (3)	DA ($Kothari$) (4)	$Meet\text{-}or\text{-}beat$ (5)
Quarter of EDGAR inclusion	-0.001	-0.003	0.000	-0.001	0.017
	0.001	*0.002*	*0.002*	*0.003*	*0.014*
Next four quarters	0.002	0.000	-0.003	-0.002	0.009
	0.001	*0.002*	*0.001*	*0.002*	*0.012*
Controls?	yes	yes	yes	yes	yes
Calendar quarter FE?	yes	yes	yes	yes	yes
Fiscal quarter FE?	yes	yes	yes	yes	yes
Firm FE?	yes	yes	yes	yes	yes
R-squared	61.5%	85.4%	16.9%	10.5%	24.5%
No. of firms	4,314	4,314	4,263	4,254	2,721
No. of firm-quarters	36,667	37,280	35,842	33,758	16,946

Table 9, Panel A. Broker Channel: Forecast Optimism.

This table reports triple-difference estimates of the effects of inclusion in EDGAR on forecast optimism. For the corresponding difference-in-differences models, see Table 5. We interact treatment with four measures of brokerage house size, each measured in the quarter before EDGAR inclusion and so not time-varying: the log of the broker's annual fee income from underwriting equity issues ($equity\ fees$), the number of analysts the broker employs ($\#\ analysts$), and the fraction of the universe of U.S. listed stocks the broker's analysts cover, by number ($coverage(\#)$) and by market cap ($coverage(\$)$). All specifications are estimated using OLS and include controls (the one-quarter lag of log market cap) and fixed effects for calendar-quarter, fiscal-quarter, and analyst-firm. For variable definitions and details of their construction see Appendix A. The coefficients are multiplied by 100 for ease of exposition. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Optimism							
	short-term				long-term			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$treated \times post$	-0.068	-0.361**	-0.350**	-0.276*	-1.267***	-0.975***	-0.948***	-0.914***
	0.265	*0.165*	*0.156*	*0.144*	*0.415*	*0.320*	*0.320*	*0.304*
$treated \times post \times equity\ fees$	-0.035*				-0.016			
	0.018				*0.025*			
$treated \times post \times \#\ analysts$		-0.005				-0.016**		
		0.004				*0.008*		
$treated \times post \times coverage(\#)$			-2.269				-6.422**	
			1.552				*3.072*	
$treated \times post \times coverage(\$)$				-1.043*				-2.272**
				0.539				*1.021*
Controls?	yes	yes	yes	yes	yes	yes	yes	yes
Calendar quarter FE?	yes	yes	yes	yes	yes	yes	yes	yes
Fiscal quarter FE?	yes	yes	yes	yes	yes	yes	yes	yes
Analyst-Firm FE?	yes	yes	yes	yes	yes	yes	yes	yes
R-squared	51.6%	54.4%	54.4%	54.4%	63.7%	66.3%	66.3%	66.3%
No. of firms	2,306	2,511	2,511	2,511	2,866	3,109	3,109	3,109
No. of observations	22,231	26,415	26,415	26,415	61,777	72,927	72,927	72,927

Table 9, Panel B. Broker Channel: Forecast Inaccuracy.
This table reports triple-difference estimates of the effects of inclusion in EDGAR on forecast inaccuracy. For the corresponding difference-in-differences models, see Table 5. We interact treatment with four measures of brokerage house size, each measured in the quarter before EDGAR inclusion and so not time-varying: the log of the broker's annual fee income from underwriting equity issues ($equity\ fees$), the number of analysts the broker employs ($\#\ analysts$), and the fraction of the universe of U.S. listed stocks the broker's analysts cover, by number ($coverage(\#)$) and by market cap ($coverage(\$)$). All specifications are estimated using OLS and include controls (the one-quarter lag of log market cap) and fixed effects for calendar-quarter, fiscal-quarter, and analyst-firm. For variable definitions and details of their construction see Appendix A. The coefficients are multiplied by 100 for ease of exposition. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Inaccuracy							
	short-term				long-term			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$treated \times post$	-0.028	-0.410***	-0.408***	-0.338**	-0.849**	-0.890***	-0.896***	-0.854***
	0.231	*0.157*	*0.149*	*0.137*	*0.390*	*0.301*	*0.304*	*0.286*
$treated \times post \times equity\ fees$	-0.033**				-0.027			
	0.016				*0.023*			
$treated \times post \times \#\ analysts$		-0.002				-0.010		
		0.004				*0.007*		
$treated \times post \times coverage(\#)$			-0.969				-3.802	
			1.408				*2.745*	
$treated \times post \times coverage(\$)$				-0.559				-1.401
				0.488				*0.903*
Controls?	yes	yes	yes	yes	yes	yes	yes	yes
Calendar quarter FE?	yes	yes	yes	yes	yes	yes	yes	yes
Fiscal quarter FE?	yes	yes	yes	yes	yes	yes	yes	yes
Analyst-Firm FE?	yes	yes	yes	yes	yes	yes	yes	yes
R-squared	57.1%	59.1%	59.1%	59.2%	68.3%	70.5%	70.5%	70.5%
No. of firms	2,306	2,511	2,511	2,511	2,866	3,109	3,109	3,109
No. of observations	22,231	26,415	26,415	26,415	61,777	72,927	72,927	72,927

Table 10, Panel A. Strategic Analyst Behavior: Heterogeneous Treatment Effects, Forecast Optimism.

This table reports triple-difference estimates of the effects of inclusion in EDGAR on forecast optimism. For the corresponding difference-in-differences models, see Table 5. We interact treatment with four measures of an analyst's incentives to engage in strategic behavior, each measured in the quarter before EDGAR inclusion and so not time-varying: whether the analyst is ranked as a "star," how focused on retail investors the analyst's brokerage house is, whether the analyst is "affiliated" in the sense of working for a brokerage house that has an investment banking relationship with the firm that is joining EDGAR, and the analyst's forecast boldness relative to other analysts covering her stocks. All specifications are estimated using OLS and include controls (the one-quarter lag of log market cap) and fixed effects for calendar-quarter, fiscal-quarter, and analyst-firm. For variable definitions and details of their construction see Appendix A. The coefficients are multiplied by 100 for ease of exposition. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Optimism							
	short-term				long-term			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$treated \times post$	-0.389***	-0.304**	-0.473***	0.450	-1.315***	-1.048***	-1.415***	-0.399
	0.127	*0.139*	*0.152*	*0.544*	*0.324*	*0.279*	*0.364*	*0.781*
$treated \times post \times star\ analyst$	-1.137**				-1.651**			
	0.458				*0.768*			
$treated \times post \times retail\ focus$		-0.455**				-0.964**		
		0.210				*0.439*		
$treated \times post\ \times affiliated\ analyst$			-0.748**				-0.613	
			0.311				*0.575*	
$treated \times post \times boldness$				-0.019*				-0.022*
				0.010				*0.013*
Controls?	yes	yes	yes	yes	yes	yes	yes	yes
Calendar quarter FE?	yes	yes	yes	yes	yes	yes	yes	yes
Fiscal quarter FE?	yes	yes	yes	yes	yes	yes	yes	yes
Analyst-Firm FE?	yes	yes	yes	yes	yes	yes	yes	yes
R-squared	50.8%	52.0%	54.6%	51.4%	62.2%	63.3%	66.3%	63.1%
No. of firms	2,263	2,328	2,520	2,259	2,780	2,849	3,117	2,796
No. of observations	21,440	23,351	26,748	21,107	58,219	64,238	73,833	58,200

Table 10, Panel B. Strategic Analyst Behavior: Heterogeneous Treatment Effects, Forecast Inaccuracy.

This table reports triple-difference estimates of the effects of inclusion in EDGAR on forecast inaccuracy. For the corresponding difference-in-differences models, see Table 5. We interact treatment with four measures of an analyst's incentives to engage in strategic behavior, each measured in the quarter before EDGAR inclusion and so not time-varying: whether the analyst is ranked as a "star," how focused on retail investors the analyst's brokerage house is, whether the analyst is "affiliated" in the sense of working for a brokerage house that has an investment banking relationship with the firm that is joining EDGAR, and the analyst's forecast boldness relative to other analysts covering her stocks. All specifications are estimated using OLS and include controls (the one-quarter lag of log market cap) and fixed effects for calendar-quarter, fiscal-quarter, and analyst-firm. For variable definitions and details of their construction see Appendix A. The coefficients are multiplied by 100 for ease of exposition. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Inaccuracy							
	short-term				long-term			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$treated \times post$	-0.338***	-0.328**	-0.446***	0.402	-1.053***	-0.842***	-1.156***	-0.596
	0.120	*0.128*	*0.146*	*0.499*	*0.291*	*0.261*	*0.330*	*0.666*
$treated \times post \times star\ analyst$	-1.137***				-1.654**			
	0.428				*0.750*			
$treated \times post \times retail\ focus$		-0.293				-0.798**		
		0.201				*0.398*		
$treated \times post\ \times affiliated\ analyst$			-0.384				-0.496	
			0.268				*0.536*	
$treated \times post \times boldness$				-0.017*				-0.012
				0.009				*0.011*
Controls?	yes	yes	yes	yes	yes	yes	yes	yes
Calendar quarter FE?	yes	yes	yes	yes	yes	yes	yes	yes
Fiscal quarter FE?	yes	yes	yes	yes	yes	yes	yes	yes
Analyst-Firm FE?	yes	yes	yes	yes	yes	yes	yes	yes
R-squared	56.4%	57.2%	59.2%	56.9%	66.9%	67.9%	70.5%	67.8%
No. of firms	2,263	2,328	2,520	2,259	2,780	2,849	3,117	2,796
No. of observations	21,440	23,351	26,748	21,107	58,219	64,238	73,833	58,200

INTERNET APPENDIX

for

Do Corporate Disclosures Constrain Strategic Analyst Behavior? * ♠

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(NOT INTENDED FOR PUBLICATION)

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♠ Disclaimer: The views expressed in this research do not necessarily reflect the position of the Federal Reserve Bank of Richmond or the Federal Reserve System.

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Table IA.1. SUTVA.

The table reports difference-in-differences estimates of the effects of inclusion in EDGAR on forecast optimism and inaccuracy. In contrast to the corresponding specifications in Table 5, we focus on analysts rather than firms as the treated unit. We ask whether analyst k changes her behavior for all the firms she covers, or just for firms joining EDGAR, when the first of the firms she covers joins EDGAR or goes online (a time which we call "quarter of first EDGAR inclusion"). In the set of stocks covered by analyst k, stock i is treated if it joins EDGAR in the quarter of first EDGAR inclusion; the remaining stocks (which join EDGAR later) are analyst k's controls. This treated-stock indicator is then interacted with the indicator for quarter of first EDGAR inclusion (and ditto for the indicator for the next four quarters). We include both analyst and firm fixed effects. The reported treatment effects reported in the table then capture the average difference in the change of an analyst's behavior between those of her stocks that joined EDGAR first and those that joined EDGAR later, measured in the quarter of first EDGAR inclusion and over the next four quarters. Cases where all the stocks covered by an analyst join EDGAR in the same quarter are excluded (given that such analysts have no control stocks). We include data from a nine-fiscal quarter window centered on the fiscal quarter in which a treated firm's EDGAR inclusion takes place. All specifications are estimated using OLS and include controls (the one-quarter lag of log market cap) and fixed effects for calendar-quarter and fiscal-quarter. For variable definitions and details of their construction see Appendix A. The coefficients are multiplied by 100 for ease of exposition. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Optimism		Inaccuracy	
	short-term	long-term	short-term	long-term
	(1)	(2)	(3)	(4)
Treated stock ×				
Quarter of first EDGAR inclusion	-0.595***	-1.524***	-0.353**	-1.150***
	0.157	*0.287*	*0.150*	*0.255*
Next four quarters	-0.665***	-1.479***	-0.411***	-1.375***
	0.122	*0.342*	*0.114*	*0.287*
Controls?	yes	yes	yes	yes
Calendar quarter FE?	yes	yes	yes	yes
Fiscal quarter FE?	yes	yes	yes	yes
Analyst FE?	yes	yes	yes	yes
Firm FE?	yes	yes	yes	yes
R-squared	12.1%	13.3%	17.1%	16.4%
No. of firms	2,107	2,579	2,107	2,579
No. of observations	25,718	80,167	25,718	80,167

Table IA.2. Analyst Behavior: Forecast Optimism and Inaccuracy (Waves 1 Through 4).

The table reports difference-in-differences estimates of the effects of inclusion in EDGAR on forecast optimism and inaccuracy. Treated firms are those whose filings became available online in January 17, 1994; control firms are nearest-neighbor propensity-score matched on equity market capitalization (in levels and logs), analyst coverage (in logs and lags), and fiscal quarter using a 0.05 caliper. We include data from a nine-fiscal quarter window centered on the fiscal quarter in which a treated firm's EDGAR inclusion takes place. All specifications are estimated using OLS and include controls (the one-quarter lag of log market cap) and fixed effects for calendar-quarter, fiscal-quarter, and firm (or analyst-firm). For variable definitions and details of their construction see Appendix A. The coefficients are multiplied by 100 for ease of exposition. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

	Optimism				Inaccuracy			
	Stock level		Analyst-stock level		Stock level		Analyst-stock level	
	short-term	long-term	short-term	long-term	short-term	long-term	short-term	long-term
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Quarter of EDGAR inclusion	-0.514***	-1.286***	-0.789***	-1.799***	-0.406**	-1.199***	-0.729***	-1.263***
	0.169	*0.418*	*0.259*	*0.428*	*0.161*	*0.380*	*0.253*	*0.387*
Next four quarters	-0.399***	-1.306**	-0.738***	-1.940***	-0.447***	-1.155**	-0.779***	-1.592***
	0.154	*0.516*	*0.250*	*0.542*	*0.143*	*0.466*	*0.235*	*0.471*
Controls?	yes	Yes	yes	yes	yes	yes	yes	yes
Calendar quarter FE?	yes	Yes	yes	yes	yes	yes	yes	yes
Fiscal quarter FE?	yes	Yes	yes	yes	yes	yes	yes	yes
Firm FE?	yes	Yes			yes	yes		
Analyst-Firm FE?			yes	yes			yes	yes
R-squared	39.2%	55.8%	53.9%	65.2%	47.4%	65.0%	60.4%	70.7%
No. of firms	1,184	1,418	1,184	1,418	1,184	1,418	1,184	1,418
No. of observations	6,377	10,155	13,968	42,066	6,377	10,155	13,968	42,066

Table IA.3. Placebo Test: Filing vs. Access.

The table reports difference-in-differences estimates of the effects of inclusion in EDGAR on forecast optimism and inaccuracy. To test whether analyst behavior changes when a firm begins to submit electronic filings to EDGAR or when those filings become accessible online, we include an indicator set equal to one for fiscal quarters before January 17, 1994 (the date filings of firms in waves 1 through 4 become accessible online). Under the null hypothesis that analyst behavior changes in response to online access, the coefficient on the indicator variable should be zero. In all other respects, the specifications reported in the table are identical to those in Table 5. Treated firms are those included in EDGAR; control firms are nearest-neighbor propensity-scored matched on equity market capitalization (in levels and logs), number of analysts (in logs and lags), and fiscal quarter using a 0.05 caliper. We include data from a nine-fiscal quarter window centered on the fiscal quarter in which a treated firm's EDGAR inclusion takes place. All specifications are estimated using OLS and include controls (the one-quarter lag of log market cap) and fixed effects for calendar-quarter, fiscal-quarter, and firm (or analyst-firm). For variable definitions and details of their construction see Appendix A. The coefficients are multiplied by 100 for ease of exposition. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

	Optimism				Inaccuracy			
	Stock level		Analyst-stock level		Stock level		Analyst-stock level	
	short-term	long-term	short-term	long-term	short-term	long-term	short-term	long-term
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Quarter of EDGAR inclusion	-0.364***	-0.473*	-0.513***	-0.984***	-0.312***	-0.369	-0.421***	-0.761***
	0.114	*0.261*	*0.138*	*0.227*	*0.101*	*0.239*	*0.129*	*0.208*
Next four quarters	-0.349***	-0.873**	-0.450***	-1.428***	-0.335***	-0.726**	-0.432***	-1.216***
	0.126	*0.354*	*0.156*	*0.343*	*0.116*	*0.333*	*0.147*	*0.314*
Pre-January 17, 1994	-0.117	0.243	-0.103	0.243	-0.296	0.118	-0.296	-0.142
	0.243	*0.534*	*0.399*	*0.505*	*0.203*	*0.506*	*0.350*	*0.451*
Controls?	yes	yes	yes	yes	yes	yes	yes	yes
Calendar quarter FE?	yes	yes	yes	yes	yes	yes	yes	yes
Fiscal quarter FE?	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE?	yes	yes			yes	yes		
Analyst-Firm FE?			yes	yes			yes	yes
R-squared	37.5%	57.8%	54.6%	66.3%	43.4%	64.0%	59.1%	70.5%
No. of firms	2,520	3,117	2,520	3,117	2,520	3,117	2,520	3,117
No. of observations	12,681	20,474	26,748	73,833	12,681	20,474	26,748	73,833