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Yen-Cheng Chang, Pei-Jie Hsiao, Alexander
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*Yen-Cheng Chang, Pei-Jie Hsiao, Alexander Ljungqvist and Kevin Tseng*

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## Abstract

We provide plausibly identified evidence for the role of investor disagreement in asset pricing. Our natural experiment exploits the staggered implementation of EDGAR, which induces a reduction in investor disagreement. Consistent with models of investor disagreement, EDGAR inclusion helps resolve disagreement around information events, leading to stock price corrections. The reduction in disagreement following EDGAR inclusion also reduces stock price crash risk, especially among stocks with binding short-sale constraints and high investor optimism.

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Yen-Cheng Chang - yenchengchang@ntu.edu.tw
*National Taiwan University*

Pei-Jie Hsiao - f05723094@ntu.edu.tw
*National Taiwan University*

Alexander Ljungqvist - alexander.ljungqvist@hhs.se
*Stockholm School of Economics and CEPR*

Kevin Tseng - kevin.tseng@rich.frb.org
*Federal Reserve Bank of Richmond*

# Testing Disagreement Models

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YEN-CHENG CHANG, PEI-JIE HSIAO, ALEXANDER LJUNGQVIST,

AND KEVIN TSENG[*]

## ABSTRACT

We provide plausibly identified evidence for the role of investor disagreement in asset pricing. Our natural experiment exploits the staggered implementation of EDGAR, which induces a reduction in investor disagreement. Consistent with models of investor disagreement, EDGAR inclusion helps resolve disagreement around information events, leading to stock price corrections. The reduction in disagreement following EDGAR inclusion also reduces stock price crash risk, especially among stocks with binding short-sale constraints and high investor optimism.

Disagreement among investors is a key ingredient in boundedly rational and behavioral models of financial markets bubbles. Assuming short-sale constraints, disagreement is used to model overvaluation and speculative bubbles in asset prices (Miller (1977), Harrison and Kreps (1978), Morris (1996), Scheinkman and Xiong (2003)) and to explain higher-order moment features of stock returns such as crash risk (Hong and Stein (2003)). Broadly speaking, disagreement provides a unifying framework that nests other closely related mechanisms such as investor overconfidence, limited attention, and gradual information diffusion (Hong and Stein (2007)).[1]

Our aim is to provide plausibly identified evidence for the role of disagreement in asset prices. Prior empirical studies typically explore cross-sectional correlations between measures of investor disagreement such as analyst forecast dispersion and asset pricing variables such as overvaluation or stock price crash risk. While informative, studies that adopt this methodology typically do not have an identification strategy that adequately controls for omitted variables (such as disclosure quality) that may simultaneously affect investor disagreement and asset prices. A clean identification strategy requires a randomly assigned shock to investor disagreement. Such a shock helps trace out the effects of changes in disagreement on asset prices, using either a difference-in-differences (DD) or instrumental-variables (IV) design.

We exploit the staggered implementation of the Electronic Data Gathering, Analysis, and Retrieval (EDGAR) system by the Securities and Exchange Commission (SEC) as a shock to investor disagreement. Before EDGAR, investors could access firms' mandatory filings (such as 10-Ks, 10-Qs, or 8-Ks) only at high cost, either by subscribing to commercial data providers or by physically visiting the SEC's reference rooms in Chicago, New York, or Washington DC (Rider (2000)). Beginning in April 1993, the SEC required U.S. firms to file their mandatory disclosures electronically through the EDGAR system.

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[1] The literature is divided on whether heterogeneous priors are sufficient to affect asset prices or whether investors also require irrationality of some kind. Hirshleifer (2015) argues that rational investors would adjust their Bayesian updating for the fact that short-sale constraints interfere with the impounding of negative priors in prices. Others are more agnostic. Hong and Stein (2007), for example, argue that failure to update in sophisticated ways could reflect a "simple lack of understanding about the structure of the environment," rather than a behavioral bias, while Kandel and Pearson (1995) note that "each individual is exposed to a different learning experience … [which] makes it impossible for agents to take full account of the information held by others."

Xiong's (2013) taxonomy suggests three main ways in which making a firm's SEC filings available via EDGAR can reduce investor disagreement, without (as we show) being confounded by changes in firms' fundamentals or disclosure policies. First, EDGAR gives investors access to standardized corporate filings, reducing the scope for heterogeneous interpretations of identical signals and hence disagreement (Kandel and Pearson (1995)). Second, EDGAR can reduce overconfidence, by confronting investors with hard information against which to judge the beliefs behind their trading decisions (Einhorn (1980), Griffin and Tversky (1992)). Lower overconfidence, in turn, can reduce disagreement (Scheinkman and Xiong (2003)). Third, disagreement could fall as analyst behavior changes. Xiong argues that strategic behavior by analysts fuels disagreement between naïve and sophisticated investors. Chang, Ljungqvist, and Tseng (2021) find that EDGAR inclusion leads to analysts behaving less strategically, reducing investor disagreement. Beyond these three mechanisms, online access to corporate filings makes stock prices more informative (Gao and Huang (2020)), which in and of itself should reduce disagreement.

Helpfully for identification purposes, the SEC *randomly* assigned firms 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 assignments 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 materially confound our findings. Equally helpfully, the SEC changed key features of the roll-out in ways that imply that a firm's inclusion in EDGAR can be viewed as a surprise, reducing concerns that firms, analysts, or investors altered their behavior in anticipation.

Using a stacked DD approach with to-be-treated firms as clean controls, we begin by comparing changes in investor disagreement among treated and control firms around EDGAR

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[2] Table I lists the 10 phase-in dates. As Chang, Ljungqvist, and Tseng (2021) note, the SEC assigned firms to waves randomly conditional on firm size.

inclusion. We use three alternative proxies for disagreement: dispersion in analysts' earnings forecasts, short interest, and trading volume around earnings announcements. For each of these proxies, we find that investor disagreement is significantly reduced after a firm is included in EDGAR, compared to similar firms not yet included in EDGAR. The magnitude of the effect is both statistically significant and economically meaningful: disagreement falls by between 3.7% and 25.3% from its pre-EDGAR mean, depending on which proxy we use. Quantile DD regressions reveal that the reduction in disagreement is significant regardless of the initial level of disagreement and that it is larger the larger the initial level of disagreement. Consistent with random assignment to EDGAR waves, we find no evidence of diverging pre-trends, which indirectly supports the parallel-trends assumption necessary for identification in a DD setting.

Having established that EDGAR inclusion affects standard disagreement measures, we investigate the effects of disagreement on stock returns. In disagreement models such as Miller's (1977), stocks are overpriced because pessimistic investors cannot express their views fully owing to short-sale constraints. Cash flow news causes investors to reevaluate their views, leading to a reduction in disagreement and a fall in share prices. We predict and find larger share price falls in response to cash flow news for firms that have joined EDGAR than for firms that have yet to join EDGAR, consistent with Miller's model.

Finally, we investigate the effects of EDGAR inclusion and of investor disagreement on a key asset pricing quantity: stock price crash risk. Hong and Stein (2003) propose a model in which investors agree to disagree over a firm's fundamental value, which, assuming short-sale constraints, in turn leads to higher crash risk. When initial disagreement is high, pessimistic investors, prevented from expressing their views through short sales, can at best sell their shares. Market prices then primarily reflect optimistic views. Small price drops tend to reveal negative information as the market learns about the extent of the negative information in the hands of pessimistic investors. As a result, stock prices move asymmetrically: they experience big drops (or crashes) in market downturns but not vice versa.

We investigate stock price crash risk by first estimating DD regressions. The literature proposes a variety of proxies for crash risk, and we find consistent results for all of them. Chen, Hong, and Stein's (2001) two measures of crash risk – return skewness and down-to-up volatility – both fall significantly over the four quarters after EDGAR inclusion, by 36.7% and 38.2% from their pre-EDGAR means, respectively. Hutton, Marcus, and Tehranian's (2009) crash measure – identifying firms experiencing extreme negative stock returns – similarly falls significantly, by between 6.1% (for negative returns at the first percentile) and 31.6% (at the 0.01 percentile).

The result that EDGAR inclusion leads to both a reduction in investor disagreement and a reduction in stock price crash risk suggests (but does not prove) that disagreement affects crash risk causally. To test whether it does, we estimate two-stage-least-squares (2SLS) regressions in which investor disagreement is instrumented using the EDGAR shock.[3] Consistent with Hong and Stein's (2003) model, we find that investor disagreement positively affects stock price crash risk, regardless of which measures of investor disagreement and stock price crash risk we use.[4]

A causal interpretation of these findings requires that EDGAR inclusion affects crash risk only through its effect on disagreement and not directly or through another channel. We investigate the plausibility of this identifying assumption through the lens of the leading alternative explanation for crash risk that does not involve disagreement: bad-news hoarding (Jin and Myers (2006), Hutton, Marcus, and Tehranian (2009)). We find no evidence that EDGAR inclusion triggers the kinds of changes in voluntary disclosure policies or earnings management practices that the literature associates with bad-news hoarding.

To add further nuance to our findings, we explore two cross-sectional predictions of crash-risk models. The first concerns short-sale constraints. Using triple-difference models, we find that crash risk decreases more following EDGAR inclusion the more binding a firm's short-sale constraints. The second comes from Miller's (1977) model, which implies that investor optimism

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[3] As Atanasov and Black (2016) note, shock-based instruments tend to provide more convincing causal inference strategies than other types of instruments.

[4] As we show in the Internet Appendix, our results continue to hold for less widely used measures of crash risk.

plays a key role in linking disagreement and crash risk. Intuitively, the marginal investor's optimism magnifies the effect of disagreement on asset prices. When optimism is high, asset prices become more prone to crashes. In our context, we expect the effect of EDGAR inclusion on crash risk to be stronger for firms whose marginal investors are more optimistic. Measuring investor optimism by the extent to which a firm's share price values the firm based on future growth opportunities rather than assets in place (Benveniste et al. (2003)), we find results that are in line with Miller's model.

Our paper is part of a recent body of work exploiting the staggered way in which EDGAR was implemented. We differ from this body of work in that we focus on EDGAR's asset pricing consequences in the context of disagreement models. Chang, Ljungqvist, and Tseng (2021), the paper closest to ours, shows that EDGAR inclusion constrains strategic analyst behavior. Emery and Gulen (2019) and Gao and Huang (2020) view EDGAR as an IT improvement and show that it helps the retail customers of an online discount broker to overcome their home bias and that it improves the informativeness of their trades. Guo et al. (2019), a paper that partly overlaps with ours in its focus on crash risk, finds that accounting conservatism increases post-EDGAR, consistent with a bad-news hoarding channel for crash risk and in contrast to our findings.[5]

We make two principal contributions to the literature. First, we systematically test the implications of disagreement models as regards overvaluation and stock price crash risk in a unified setting using a single identification strategy, exploiting a randomly assigned shock to investor disagreement as firms join EDGAR. We view prior empirical work on disagreement as incomplete for two reasons: it focuses on either overvaluation or crash risk in isolation, making their interplay difficult to evaluate; and it is cross-sectional in nature, which, as Chen, Hong, and Stein (2001) note, raises endogeneity concerns that make it difficult to draw causal inferences. We revisit this literature using plausibly identified evidence and provide a unified setting in

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[5] As outlined in subsequent footnotes, we have reservations about their research design.

which both overvaluation and crash risk can be analyzed simultaneously. Our findings provide empirical support to a broad class of models of investor disagreement.

Second, our study contributes to the literature on mandatory disclosure. There has been much debate about the costs and benefits of increased mandatory disclosure, such as reductions in information production costs (Verrecchia (1982), Kim and Verrecchia (1994)), stock-price uncertainty (Goldstein and Yang (2017)), benefits to becoming informed (Dugast and Foucault (2018)), and information overload (Barber and Odean (2008)). We contribute to this debate by showing that improved mandatory disclosure leads to less disagreement and reduced crash risk and thereby helps stabilize markets. This finding should be of interest to both securities regulators and scholars of corporate disclosure.

## I. Empirical Strategy and Data

*A. Institutional Background*

Identifying the causal effects of investor disagreement on asset prices requires a shock to disagreement 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 EDGAR system. 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 could either physically visit one of the three SEC reference rooms (located in Chicago, New York, and Washington DC) or subscribe to commercial data vendors such as Mead Data Central (at, apparently, high cost).[6] Facing increasing costs of receiving, storing, and distributing large numbers of corporate filings for public use, and after lobbying from Ralph Nader's "Taxpayer Assets Project" and high-ranking members of Congress, the SEC on February 23, 1993 announced a plan to require all registered firms to submit their filings electronically. SEC Release No. 33-6977 included a preliminary phase-in schedule, with registered firms joining

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[6] According to a 1992 petition to the SEC signed by academics, librarians, and journalists, Mead 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" (see http://www.bio.net/bionet/mm/ag-forst/1992-January/000187.html). Dialog, a competitor to Mead, charged "$84 per hour plus $1 per page" (quoted from the same source). We calculate that obtaining Ford's 1994 10-K from Dialog would have cost $145 in page charges alone.

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.[7]

As Chang, Ljungqvist, and Tseng (2021) note, electronic *filing* per se would not be expected to affect investors' costs of accessing mandatory disclosures. The actual shock to information *access* 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 EDGAR filings available for free online, hosted by NYU.[8] 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.[9] In waves 5 through 10, 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.


INSERT FIGURE 1 ABOUT HERE


*B. Identification Strategy*

The introduction of online access to corporate filings via first NYU and eventually EDGAR (henceforth, with a slight abuse of terminology, simply "EDGAR inclusion") provides an appealing empirical setting to study the causal effects of investor disagreement on asset prices. As noted in the introduction, EDGAR inclusion can reduce investor disagreement through its effect on three channels: heterogeneous priors, overconfidence, and information transmission.

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[7] The phase-in schedule included a six-month review, to begin after wave 4 on Dec. 6, 1993. The review took longer than planned, leading to the suspension of waves 5 (originally scheduled for Aug. 1994) and 6 (originally scheduled for Nov. 1994). On Dec. 19, 1994, the SEC announced the final rules on EDGAR implementation, revising the dates for waves 5 and 6 to Jan. 1995 and Mar. 1995, respectively, confirming the date for wave 7, and modifying the dates for waves 8 through 10 (SEC Release No. 33-7122). We use the final phase-in dates as per the Dec. 1994 announcement. In doing so, we follow Chang, Ljungqvist, and Tseng (2021) but depart from Emery and Gulen (2019), Guo et al. (2019), and Gao and Huang (2020), all of which use the preliminary dates.
[8] The SEC's original plan was to allow public access to EDGAR only via dedicated terminals located in the SEC's three reference rooms.
[9] The SEC took over the task of hosting online access to EDGAR from NYU in October 1995.

First, a rich seam of literature explores the effects of investors with heterogeneous priors disagreeing about their interpretations of identical signals (Harrison and Kreps (1978), Varian (1989), Romer (1993), Harris and Raviv (1993), Kim and Verrecchia (1994), Kandel and Pearson (1995), Odean (1998), Hong and Stein (2003), Scheinkman and Xiong (2003), and Banerjee and Kremer (2010)). As Xiong (2013) notes, the scope for investors to hold heterogeneous priors is more limited when learning costs are low. In our context, the standardized corporate filings EDGAR gives investors access to can reduce learning costs and hence investor disagreement.

Second, overconfidence can engender disagreement in two ways: because overconfident investors exaggerate the precision of their signals (Odean (1998), Scheinkman and Xiong (2003)) and because overconfidence prevents investors with different private signals from learning from each other through trading, which contributes to slow news diffusion in the stock market (Hong and Stein (1999, 2007)). Prior literature shows that investor overconfidence is more severe when decision feedback is ambiguous (Einhorn (1980), Griffin and Tversky (1992)). Once a stock joins EDGAR, investors gain access to hard data against which to evaluate their trading decisions. The possibility of feedback of this kind can reduce overconfidence and hence investor disagreement (Daniel, Hirshleifer, and Subrahmanyam (1998)).

Third, Xiong (2013) proposes that biased analyst earnings forecasts drive a wedge between naïve investors, who do not debias analyst forecasts, and sophisticated investors, who are better able to debias analyst forecasts. Chang, Ljungqvist, and Tseng (2021) show that EDGAR constrains analysts' strategic behavior, leading to less biased and more accurate earnings forecasts, especially among those analysts with greater reason to behave strategically in the first place (such as affiliated analysts and those serving predominantly retail clients).[10] This, in turn, helps narrow disagreement between naïve and sophisticated investors.

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[10] Chang, Ljungqvist, and Tseng (2021) characterize the information-economic effects of EDGAR inclusion as a reduction in investors' costs of verifying the accuracy and veracity of information provided by information

Three features of the way the SEC implemented EDGAR greatly reduce endogeneity concerns. First, the SEC assigned registered firms to the ten implementation waves *randomly*, conditional only on size (Chang, Ljungqvist, and Tseng (2021)). 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 investor disagreement, such as market-wide changes in regulations and sentiment or macroeconomic news. 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 (but 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 ever going to be put online, coupled with the fact that firms in waves 5-10 were given short notice of their phase-in dates, greatly reduces the risk of confounds that result from firms, analysts, or investors changing their behavior ahead of treatment.

Random assignment, staggering, and lack of anticipation effects go a long way towards ensuring the internal validity of the EDGAR experiment. The identifying assumption in the context of a DD design is, as always, parallel trends, which we can evaluate directly in the usual ways. The identifying assumption in the context of an IV design is that the EDGAR experiment satisfies the exclusion restriction, that is, that EDGAR inclusion affects asset pricing variables of interest only through the channel of investor disagreement. In that sense, an IV design is more restrictive than a DD design, committing the researcher to a particular channel to the exclusion of others. We investigate the plausibility of the exclusion restriction in greater detail in Section IV.

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intermediaries such as sellside stock analysts. In particular, reduced verification costs constrain analysts' ability to strategically skew their forecasts and recommendations in ways that benefit themselves or their brokerage-firm employers. The analyst literature has explored how reputational concerns counteract strategic analyst behavior (Hong, Kubik, and Solomon (2000), Cowen, Groysberg, and Healy (2006), Ljungqvist, Marston, and Wilhelm (2006), Ljungqvist et al. (2007), and Kolasinski and Kothari (2008)). Reduced verification costs make reputational concerns more salient and thereby reduce strategic behavior.

## C. Sample and Data

### C.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.[11] Following standard practice, we exclude utilities (SIC code 49) and financial-services firms (SIC code 6), as accounting rules and disclosure requirements are different for regulated firms. We also 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 investment trusts, master limited partnerships, and the like).

Eventually, all SEC-registered firms are treated, as every issuer is obliged to file through EDGAR starting on May 6, 1996. To avoid biases that can arise in staggered DD approaches with time-varying treatments and treatment effect heterogeneity (Baker, Larcker, and Wang (2021)), we select "clean" control firms from the set of future-treated firms.[12] Naturally, the last EDGAR wave lacks clean controls and (due to bunching towards the end of the SEC's phase-in schedule) so do waves 8 and 9. This leaves us with four staggered treatment dates: January 17, 1994, January 30, 1995, March 6, 1995, and May 1, 1995.

Given that the SEC assigned firms to EDGAR phase-in waves randomly conditional on size, it is essential to select control firms that are similar in size, or else one would end up comparing large treated to small control firms, a classic apples-to-oranges problem. Indeed, without matching, we find severe diverging pre-trends in our DD tests, fundamentally undermining the internal validity of results from unmatched research designs.[13] We select control firms using a

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[11] Our focus on the dates when filings go online is another point of departure from Emery and Gulen's (2019), Guo et al.'s (2019), and Gao and Huang's (2020) studies using EDGAR as a shock.

[12] Restricting control firms to come from the set of future-treated firms is a third point of departure from Emery and Gulen (2019), Guo et al. (2019), and Gao and Huang (2020).

[13] Matching on size is a fourth point of departure from Emery and Gulen (2019), Guo et al. (2019), and (except in one robustness test) Gao and Huang (2020).

nearest-neighbor propensity-score method, matching on equity market capitalization (in levels and logs) and fiscal quarter. Only matches in the common support are considered valid, using a 0.05 caliper. This limits our estimation sample to a total of 1,694 treated and 1,694 control firms.

We follow each treated firm and its matched control for nine fiscal quarters centered on the treated firm's EDGAR inclusion quarter. Coupled with the fact that we use only clean controls, our research design is equivalent to Cengiz et al.'s (2019) stacked-regression estimator, which Baker, Larcker, and Wang (2021) show with the help of simulations to be unbiased.

As Table I shows, the average treated firm has an equity market cap of $179.4 million in the fiscal quarter before treatment. This average is considerably smaller than the $791.9 million market cap of the average listed U.S. firm in Q1 1993, the quarter before the first wave. Figure 2 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 untreated large firms left in the common support: only 73 of the 351 firms in the first two waves that otherwise satisfy our sample filters have valid controls. To the extent that smaller firms are subject to above-average investor disagreement, our empirical estimates may accordingly overstate the effects of disagreement on asset pricing quantities of interest for the average U.S. listed firm. Put differently, our estimates should be thought of as local average treatment effects (LATE).

INSERT TABLE I ABOUT HERE

INSERT FIGURE 2 ABOUT HERE

*C.2. Investor Disagreement Measures*

Investor disagreement is not observed directly. To proxy for investor disagreement, we follow three strands of the literature. The first starts with Diether, Malloy, and Scherbina's (2002) influential work on differences of opinion among investors. Diether, Malloy, and Scherbina show that analyst forecast dispersion is a good proxy for differences of opinion among

investors, and this proxy has since become a widely used measure of investor disagreement. The implicit identifying assumption behind this proxy is that investors use analyst earnings forecasts to inform their expectations of a company's future cash flows and hence its market value. Investors who are clients of brokerage firms with a more bullish analyst covering a given stock are more likely to form optimistic expectations, while investors who are clients of more bearish analysts are more likely to form pessimistic expectations, all else equal. If so, investor disagreement is higher the greater the dispersion in analysts' earnings forecasts. A direct consequence of the reduced strategic behavior Chang, Ljungqvist, and Tseng (2021) document post-EDGAR is a reduction in dispersion in analyst forecasts and, by this argument, in investor disagreement.

The literature operationalizes forecast dispersion in two ways, using either the standard deviation of analyst earnings forecasts (Diether, Malloy, and Scherbina (2002)) or the difference between the highest and lowest forecast (De Bondt and Forbes (1999)), in each case scaled by the end-of-quarter stock price. We refer to these as dispersion and range, respectively. We measure dispersion and range over two horizons, based either on forecasts made for the next fiscal quarter or the current fiscal year. This gives us four analyst-based measures of investor disagreement. (All variable definitions and details of their construction can be found in the Appendix.)

The second strand of the literature we follow measures investor disagreement using short interest. Following Karpoff and Lou (2010), we measure abnormal short interest as the residual from a cross-sectional regression of a firm's short interest ratio on size, book-to-market, momentum, and industry.

The final strand of the literature we follow measures disagreement using trading volume around earnings announcements (Kandel and Pearson (1995), Daniel, Hirshleifer, and Subrahmanyam (1998), Barber and Odean (2008)). It is well known that earnings announcements are (among) the most important drivers of share prices and trading volume as investors process the information such announcements contain. Kandel and Pearson (1995) find

increases in trading volume even among earnings announcements that do not lead to changes in share prices. Noting that existing heterogeneous-investor models that assume investors interpret information identically cannot explain this pattern, and after ruling out alternative explanations, Kandel and Pearson propose a model in which investors agree to disagree in their interpretations of identical public signals. A key prediction of their model is that trading intensity around earnings announcements increases in disagreement. This makes trading volume around earnings announcements a potential proxy for disagreement.

Table II reports summary statistics of our disagreement measures, separately for treated and control firms, as of the fiscal quarter before treatment. Treated and control firms have near-identical dispersion, range, abnormal short interest, and trading volume around earnings announcements in the quarter before treatment, both in levels and – more importantly for identification purposes – in changes. The *t*-test shown in the last column confirms that there are no diverging pre-trends, in the sense that the difference in pre-treatment changes between treated and controls is not statistically significant for any of our disagreement measures.


INSERT TABLE II ABOUT HERE


*C.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, that is, conditional on market cap. We take this into account by matching on size when selecting control firms. As Table II shows, treated and control firms are matched quite precisely on size. We additionally include log market cap as a control variable in our empirical specifications.

The second issue is that analyst forecasts are known to exhibit seasonalities. Earnings forecasts tend to become more accurate over the course of a firm's fiscal year (Richardson, Teoh,

and Wysocki (2004)), especially (but not only) as regards forecasts of full-year (as opposed to quarterly) earnings. Differences in fiscal year-ends could potentially confound our DD estimates, or at minimum make them noisier.[14] To avoid bias and to reduce noise, our research design matches on fiscal year-end when selecting control firms. We additionally include fixed effects for fiscal quarter as control variables in our empirical specifications.

Finally, we include the usual firm and time 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 year-quarter fixed effects. These time effects remove the effects of any common shocks that affect all firms in a given quarter, such as macroeconomic news or market-wide changes in regulations or investor sentiment.

## II. Investor Disagreement and Information Access

We begin our empirical analysis by examining the impact of EDGAR inclusion (or more precisely, online access to corporate filings) on investor disagreement. To investigate how investor disagreement changes when mandatory filings become available online, we estimate the following stacked DD regression (Cengiz et al. (2019)):

$$DISAGREEMENT_{iwt} =$$

$$\alpha + \beta_1 SHOCK_{iwt} + \beta_2 POSTSHOCK_{iwt} + \gamma \mathbf{X}_{iwt} + c_{iw} + c_q + c_{if} + \varepsilon_{iwt}, \qquad (1)$$

where $DISAGREEMENT_{iwt}$ for firm $i$ joining EDGAR in wave $w$ in quarter $t$ is measured using forecast dispersion, short interest, or trading volume around earnings announcements; $SHOCK_{iwt}$ and $POSTSHOCK_{iwt}$ are treatment indicators that equal one in the quarter a firm joins EDGAR and the next four quarters, respectively; $\mathbf{X}_{iwt-}$ includes the control variables described in Section I.C.3; and $c_{iw}$, $c_q$, and $c_{if}$ are wave-specific firm, calendar-time, and fiscal-quarter fixed effects. Standard errors are clustered at the firm level, given that we exploit a firm-level shock

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[14] To see how, suppose we were to systematically compare treated firms in their last fiscal quarter (when the quarterly change in forecast dispersion and range would be relatively minor) to control firms in their first fiscal quarter (when forecast dispersion and range would typically be considerably greater than a quarter ago). Such a comparison could yield a negative DD estimate simply as a result of the misalignment of fiscal year-ends rather than because EDGAR inclusion reduces investor disagreement. The opposite pattern is also possible. Depending on the empirical distribution of fiscal year-ends among treated and control firms, there could thus be positive or negative bias, and at minimum there would be an increase in statistical noise.

and the time dimension of our panel is substantially smaller than the firm dimension (Petersen (2009), Section 3).[15]

Table III reports the results. The effect of EDGAR inclusion is uniformly negative across our six disagreement measures. It is statistically significantly negative in the treatment quarter for four of our six measures and consistently statistically significantly negative for all six measures in the four quarters following treatment.[16] Economically, the estimated treatment effects are non-trivial. To illustrate, the point estimates shown in column 1 suggest that all else equal and relative to the pre-treatment mean, EDGAR inclusion reduces average quarter-ahead forecast dispersion by 7.5% in the quarter of treatment ($p=0.109$) and by 18% over the next four quarters ($p<0.001$). The economic magnitudes are similar for the other three analyst-based disagreement measures. Relative to its pre-treatment mean, abnormal short interest falls by 13.2% in the quarter of EDGAR inclusion ($p=0.01$) and remains 21.7% lower over the next four quarters ($p=0.002$). Assuming, as the literature does, that analyst forecast dispersion and short interest are reasonable proxies for investor disagreement, we interpret these findings as consistent with the prediction that easier access to mandatory disclosures reduces differences of opinion in the market.


INSERT TABLE III ABOUT HERE


Trading volume in the three days around earnings announcements declines on average by 0.5% in the treatment quarter ($p=0.691$) and by 3.7% over the next four quarters ($p=0.001$),

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[15] Prominent examples of studies using DD models that cluster by firm in short panels include Fracassi and Tate (2012), Fang, Tian, and Tice (2014), and Balakrishnan et al. (2014). Our results are robust to double clustering by firm and fiscal quarter instead, consistent with Petersen's (2009, p. 460) conclusion that "[w]hen there are only a few clusters in one dimension, clustering by the more frequent cluster yields results that are almost identical to clustering by both firm and time." In our setting, robustness is unsurprising: given conditionally random assignment, firms in each phase-in wave have only one thing in common – size. Since our regressions control for size, there are unlikely to be unobserved characteristics that could induce correlated responses within a quarter.
[16] Chang, Ljungqvist, and Tseng (2021) find no evidence that analysts change the timing of their forecasts around EDGAR inclusion. It is thus not the case that forecast dispersion falls simply because there are fewer stale outstanding forecasts. Our results are robust to including only the last forecast made by each analyst in each quarter.

relative to matched controls. As Kandel and Pearson (1995) note, changes in trading intensity are difficult to reconcile with models that require investors to agree when presented with the same information (such as an earnings announcement).[17] The reduction in trading intensity we find following EDGAR inclusion is thus consistent with a reduction in investor disagreement. Kandel and Pearson further show that investor disagreement can vary even in the absence of accompanying news. Using absolute abnormal returns around earnings announcements as a proxy for news, column 7 shows that trading volume decreases following EDGAR inclusion even in the absence of news (that is, when absolute announcement returns are zero) and that the effect of trading volume is not significantly related to the size of the returns.

Table III reports formal tests of diverging pre-trends between treated and controls obtained from event-study dynamic DD specifications (Baker, Larcker, and Wang (2021)). These confirm the absence of pre-treatment effects for all five of our disagreement measures at the 95% level, as required for the internal validity of our DD approach. Figure 3 visualizes the event-study dynamic DD estimates for each of the six disagreement measures over the nine-quarter window around EDGAR inclusion, along with 95% confidence intervals. The figure confirms the absence of diverging pre-trends in all cases, except for the range of fiscal-year forecasts, for which we see a statistically significant reduction in quarter $t = -1$.


INSERT FIGURE 3 ABOUT HERE


Figure 4 investigates how the size of the effect of EDGAR inclusion on disagreement varies with the level of pre-treatment disagreement. Specifically, the figure graphs point estimates and 95% bootstrapped confidence intervals obtained from quantile DD regressions of each of our six

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[17] In models of investor heterogeneity in which agents agree on a common distribution and observe independent signals from this distribution, there is typically no trading in the absence of news (Kim and Verrecchia (1991a, 1991b), Harris and Raviv (1993), Romer (1993)). Kandel and Pearson (1995), in contrast, allow agents to have different interpretations even when they receive identical signals. This important feature leads to trading even when there is no news, providing a justification for the high trading volumes seen in financial markets (Hong and Stein (2007)).

measures of investor disagreement on EDGAR inclusion. This generates two important insights. First, disagreement falls post-EDGAR inclusion regardless of the initial level of disagreement: the estimated treatment effects are significantly negative across all deciles for each of our six measures. Second, the slope is negative across deciles, meaning that the fall in disagreement is larger the larger the initial level of disagreement. This pattern is particularly noticeable for the two measures based on fiscal-year forecasts, followed by the two measures based on quarter-ahead forecasts, with much flatter slopes for the short-interest and trading-volume measures.

INSERT FIGURE 4 ABOUT HERE

Overall, the results in Table III and Figures 3 and 4 are consistent with investor disagreement falling significantly, both economically and statistically, when it becomes less costly for investors to access mandatory corporate disclosure filings through EDGAR.

### III. Investor Disagreement and Stock Prices

We next turn our focus to the effect of EDGAR inclusion on returns. The workhorse model in the literature is Miller (1977). In Miller's model, stocks are overpriced because investors hold divergent opinions about firm value and pessimistic investors are prohibited from short-selling. Stock prices thus disproportionately reflect the valuations of optimists. Disagreement is reduced through earnings news (or as Miller writes, it "is reduced as the company acquires a history of earnings or lack of them"). Accordingly, overpricing due to investor disagreement is corrected as new information forces optimistic investors to revise their valuations downwards, leading to negative stock price effects, a prediction Berkman et al. (2009) find empirical support for.

To test Miller (1977) in our setting, we focus on changes in stock prices over a short window around two key information events: earnings announcements and analyst forecasts. Earnings announcements are widely used in the empirical literature to study corrections to mispricing.[18]

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[18] For example, La Porta et al. (1997) use stock price effects around earnings announcements to investigate price corrections for overpriced growth stocks.

Prior empirical work shows that a significant amount of investor disagreement is resolved around earnings announcements as the earnings news leads investors to update their priors. Barth et al. (2020), for example, find that 30% of disagreement is resolved around earnings announcements. In our setting, EDGAR inclusion facilitates access to historical corporate filings and so may reduce the scope for heterogeneous interpretations of current earnings news among investors. As a result, we expect that earnings news resolves more investor disagreement for treated stocks (those included in EDGAR) than for control stocks (those not yet included in EDGAR). Earnings announcements should therefore trigger larger share price falls for treated than for control stocks.

A similar argument applies to the second type of information event we investigate, analysts' earnings forecasts. Like earnings announcements, analyst forecasts convey cash flow news, but unlike earnings announcements, which reveal a firm's actual historical performance, analyst forecasts tend to be biased signals: driven by career concerns and a desire to curry favor with management, analysts have been shown to issue forecasts that help firms positively surprise the market (Francis and Philbrick (1993), McNichols and O'Brien (1997), Hong, Kubik, and Solomon (2000)).[19] The less biased the signal, the more disagreement it resolves (Andrade, Bian, and Burch (2013)). Chang, Ljungqvist, and Tseng (2021) show that EDGAR inclusion constrains analysts' strategic behavior. As a result, we expect analyst forecasts to resolve more investor disagreement after EDGAR inclusion, leading to larger stock price corrections for treated stocks.

Our tests compare raw and market-adjusted cumulative returns of treated and control firms in the [-1,1] trading-day window around the first earnings announcement or analyst forecast in either the treatment quarter or the quarter before. Table IV reports the results. The evidence supports disagreement models such as Miller (1977). Consistent with the predicted greater resolution of investor disagreement following EDGAR inclusion, we find negative return effects for treated firms, compared to control firms, around earnings announcements (in columns 1 and

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[19] Overly optimistic analyst forecasts drive disagreement between naïve and sophisticated investors (Xiong (2013)). Supporting this view, Malmendier and Shanthikumar (2007) and So (2013) find that naïve investors fixate on analysts' biased forecasts, while Hilary and Hsu (2013) find that sophisticated investors can unravel these biases.

2) and analyst forecasts (in columns 3 and 4) after EDGAR inclusion (in Panel A) but not before (in Panel B).[20] The economic magnitudes are meaningful: share prices fall by 1.9 percentage points more for treated firms than for control firms around the first earnings announcement and by 70 to 90 basis points more around the first analyst forecast in the treatment quarter. These return differentials are statistically significant at the 5% level or better for three of the point estimates and at the 5.6% level for the fourth point estimate. In the quarter before EDGAR inclusion, the return differentials are economically small and statistically zero, as expected.

INSERT TABLE IV ABOUT HERE

Starting with Miller (1977), one important assumption made in every disagreement model is that at least some investors face short-sale constraints. From a theoretical perspective, short-sale constraints are necessary (but not sufficient) to generate asset pricing consequences such as overpricing and stock price crash risk (Diether, Malloy, and Scherbina (2002), Chen, Hong, and Stein (2001)): divergent views on firms' cash flows among investors only matter when short-sale constraints are binding such that (some) pessimistic investors can at best sell the shares they already own. Cross-sectionally, we therefore expect price corrections around information events to be larger the more short-sale constrained the firm.

Short-sale constraints are notoriously difficult to measure, especially in the early 1990s, a period that precedes the availability of the type of proxies for short-sale constraints favored in today's literature.[21] One proxy that is available in the early 1990s is institutional ownership.

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[20] We view the coefficients in Table IV, Panel A as the structural counterparts to Berkman et al.'s (2009) reduced-form estimates, in the sense that we are able to exploit an exogenous shock to investor disagreement while they rely on cross-sectional variation in their proxies for investor disagreement.

[21] For example, Markit's database of stock lending fees starts in July 2006 (well after our sample period), while OptionMetrics' database, which Muravyev, Pearson, and Pollet (2020) use to estimate option-implied lending fees, starts in 1996 (also after the EDGAR roll-out). Other proxies for short-sale constraints sometimes used in the literature are ambiguous or not suited to our setting. Idiosyncratic volatility, a proxy for limits to arbitrage, can deter arbitrage for both overpriced and underpriced stocks. Breadth of mutual fund ownership is best suited as a proxy for short-sale constraints among the largest stocks (Chen, Hong, and Stein (2002)), whereas our sample skews towards smaller stocks.

Firms whose institutional ownership has decreased going into the EDGAR inclusion quarter are likely to be more short-sale constrained, given that institutions are the main suppliers of stock loans (D'Avolio (2002)). If so, we expect the effect of EDGAR inclusion on stock price returns around our two information events to be stronger for stocks whose institutional ownership has decreased and weaker for stocks whose institutional ownership has increased. Consistent with this prediction, Panel C shows that firms experience a less pronounced fall in stock prices following EDGAR inclusion the less binding their short-sale constraints, as proxied by changes in institutional ownership, significantly so around earnings announcements.

Finally, Table IA.I in the Internet Appendix reports standard calendar-time portfolio alphas to test for return predictability over longer windows of three, six, and twelve months. Our results support the negative long-run effect of disagreement on returns documented in the cross-section by Diether, Malloy, and Scherbina (2002), Chen, Hong, and Stein (2002), and Yu (2011).

## IV. Investor Disagreement and Stock Price Crash Risk

Investor disagreement is viewed as a possible explanation for stock price crash risk. Hong and Stein (2003) model a market in which disagreement among investors can result in stock prices being more prone to large downward movements than to large upward movements – that is, to crash risk. Hong and Stein assume that investors disagree about a firm's future prospects and that some (but not all) investors face short-sale constraints. When the initial disagreement is large, pessimistic investors subject to short-sale constraints can do no more than sell their shares. Their opinions are therefore not fully incorporated into the firm's share price: all that is known is that their valuations are below the current share price, but not by how much. However, if the share price begins to fall (either because of a market downturn or because the more optimistic investors change their minds), the pessimists' pent-up information begins to be incorporated in the share price through their decisions at which price to begin buying the stock. There is no corresponding delayed incorporation of optimistic opinions when the share price goes up, since optimistic investors can freely buy the stock. This asymmetry implies that returns are positively skewed conditional on prices rising and negatively skewed conditional on prices falling.

Chen, Hong, and Stein (2001) provide empirical evidence consistent with Hong and Stein's (2003) model, showing that trading volume (one of our proxies for investor disagreement) is positively correlated with stock price crash risk. Whether this is causal remains an open question. Our analysis in this section provides what we consider plausibly identified evidence of a causal link between disagreement and crash risk.

*A. Empirical Measures*

*A.1. Stock Price Crash Risk Measures*

To proxy for stock price crash risk, we use five widely used measures. The first two, $NCSKEW$ and $DUVOL$, come from Chen, Hong, and Stein (2001). $NCSKEW$ is the negative coefficient of return skewness. $DUVOL$ is "down-to-up volatility" (the ratio of the return volatility during "down" days to the return volatility during "up" days). The final three measures follow Hutton, Marcus, and Tehranian (2009) and capture the incidence of extreme negative share price returns, with "extreme" denoting left-tail returns in the bottom 0.01%, 0.1%, or 1% of a normal distribution. We refer to these measures as $CRASH001$, $CRASH01$, and $CRASH1$, respectively. (See the Appendix for formal definitions.) Higher values of $NCSKEW$, $DUVOL$, and $CRASHx$ correspond to greater stock price crash risk. As the summary statistics in Table II show, treated and control firms have very similar levels of crash risk in the fiscal quarter before treatment.[22] For example, 7.9% of treated firms and 8.5% of control firms experience one or more days in a quarter with returns in the left 0.01% tail of the return distribution. More importantly for identification purposes, we find no significant differences in pre-treatment changes between treated and controls, suggesting there is no significant divergence in pre-trends.

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[22] The observant reader may notice that both $NCSKEW$ and $DUVOL$ have negative averages, meaning that daily returns are on average *positively* skewed. This echoes the summary statistics of Chen, Hong, and Stein's (2001) sample. Chen, Hong, and Stein offer an intuitive explanation for positive average skewness: *conditional* on share prices rising, returns are positively skewed (as pessimistic opinions are prevented from being fully incorporated in prices due to short-sale constraints); and *conditional* on share prices falling, returns are negatively skewed (as pessimistic investors rejoin the market). *Unconditionally*, then, returns can be either positively or negatively skewed, depending on which effect dominates. The change in skewness that our research design identifies is within-firm, meaning that we isolate the net change in unconditional skewness as a firm joins EDGAR. If EDGAR inclusion reduces investor disagreement, we expect skewness to increase (become more positive), as the conditional negative skewness is reduced. In other words, we expect disagreement as measured by $NCSKEW$ and $DUVOL$ to fall.

*A.2. Stock Price Jump Measures*

To test for asymmetry in the effect of disagreement on share prices, we use Hutton, Marcus, and Tehranian's (2009) measures of the incidence of stock price jumps, evaluated at the 0.01%, 0.1%, and 1% levels ($JUMP001$, $JUMP01$, and $JUMP1$). They are constructed analogously to $CRASH001$, $CRASH01$, and $CRASH1$, except that they capture the incidence of extreme positive returns. Table II confirms that our sample is well behaved in the sense that treated and control firms do not differ significantly from each other in the fiscal quarter before treatment.

*B. Difference-in Differences Results*

The starting point of our investigation of stock price crash risk is the stacked DD regression,

$$CRASH\ RISK_{iwt} =$$

$$\alpha + \delta_1 SHOCK_{iwt} + \delta_2 POSTSHOCK_{iwt} + \alpha\mathbf{X}_{iwt-} + c_{iw} + c_q + c_{if} + \xi_{iwt}, \qquad (2)$$

where $CRASH\ RISK_{iwt}$ is measured using one of our five proxies introduced in Section IV.A.1 and we control for momentum (Harvey and Siddique (2000)) and the firm's lagged share price as of the previous fiscal-quarter-end (Cheong and Thomas (2011)) alongside the other right-hand-side variables used in equation (1).

Table V reports the results. Across all five measures, we find that EDGAR inclusion leads to a statistically significant reduction in stock price crash risk, all else equal, beginning in the quarter after treatment. To illustrate, column 1 shows that $NCSKEW$ falls by an average of 0.049 ($p$=0.014) when firms' mandatory disclosures become freely available online, relative to size-matched firms whose disclosures remain expensive to access. Economically, this treatment effect is sizeable, amounting to a 45.0% reduction relative to the sample mean of $NCSKEW$ reported in Table II.[23] Column 2 shows that $DUVOL$ falls by an average of 0.031 ($p$=0.012) following EDGAR inclusion, equivalent to a 45.6% reduction from $DUVOL$'s sample mean. The incidence

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[23] In contemporaneous work, Guo et al. (2019) report a similar result for $NCSKEW$, but their non-standard research design makes it difficult to compare. Specifically, Guo et al. include lagged $NCSKEW$ as a regressor, which will cause their estimate of EDGAR inclusion to be biased unless the time dimension of their panel is large relative to the number of firms (a condition that is not met in this setting). See Wooldridge (2010), chapter 11 for details.

of extreme negative returns ($CRASHx$) falls by 34.2%, 25.0%, and 7.0% from the corresponding mean, for returns in the 0.01%, 0.1%, and 1% left tail (each highly statistically significant).

INSERT TABLE V ABOUT HERE

Formal tests of diverging pre-trends, reported in Table V, confirm the absence of pre-treatment effects for all five crash risk measures, as required for the internal validity of our DD approach. Figure 5 investigates how the effect of EDGAR inclusion on crash risk varies with the level of pre-treatment crash risk. As in Figure 4, we graph point estimates and 95% bootstrapped confidence intervals obtained from quantile DD regressions. For obvious reasons, we focus on the two continuous crash measures, $NCSKEW$ and $DUVOL$. This provides important nuance to the findings in Table V: the effect of EDGAR inclusion on crash risk, while negative across all deciles, is only statistically significant for firms in the upper half of the distribution. This suggests that the EDGAR-induced reduction in average crash risk reported in Table V is concentrated among firms with above-average initial crash risk levels.

INSERT FIGURE 5 ABOUT HERE

Given quasi-random assignment, staggered implementation, and the absence of diverging pre-trends, the findings in Table V and Figure 5 permit the plausibly causal interpretation that easier access to corporate information in the form of mandatory SEC filings leads to a reduction in stock price crash risk, as measured by standard proxies. Given our earlier evidence that easier access to corporate information also reduces investor disagreement, it is tempting to conclude that the observed reduction in disagreement *causes* the observed reduction in crash risk – tempting but premature: the reduction in crash risk around EDGAR inclusion could, potentially, be caused by some other contemporaneous change.

## C. Two-stage Least Squares Results

To test if it is reductions in disagreement around EDGAR inclusion that cause crash risk to decline requires a switch from a DD framework (which cannot investigate specific channels) to a 2SLS framework in which the channel of interest – investor disagreement – is instrumented using the EDGAR shock.[24] The DD specifications discussed in Section II form the first-stage of our 2SLS model and establish what in IV terminology is called the "relevance" of the EDGAR shock for disagreement. The remaining identifying assumption is that EDGAR inclusion affects crash risk only through its effect on disagreement and not because it correlates with some other contemporaneous change. We consider challenges to this exclusion restriction in Section IV.E. Based on models of crash risk such as Hong and Stein (2003), we expect a positive coefficient for investor disagreement in our 2SLS model: higher disagreement leads to higher crash risk.

Table VI reports the 2SLS regression results of the impact of investor disagreement on stock price crash risk. The table layout is unusual. Recall from Table III that we use six measures of investor disagreement, and recall from Table V that we use five measures of crash risk. We thus estimate $6 \times 5 = 30$ regressions. Table VI summarizes the results of these 30 regressions by reporting, in matrix form, only the 30 investor-disagreement coefficients (along with standard errors clustered at the firm level), the 30 weak-instrument tests, and the 30 observations counts. Each of the 30 "cells" in the upper half of Table VI thus represents a separate regression.

INSERT TABLE VI ABOUT HERE

Each of the 30 2SLS coefficients is positive, as predicted, and 25 of them are statistically significant at conventional levels.[25] The (relatively) weakest results come from the specifications

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[24] OLS will be biased in the presence of measurement error, simultaneity/reverse causality, or omitted variables. All three could play a role in our setting: investor disagreement is surely measured with error; crash risk could well affect investor disagreement; and alternative explanations for crash risk such as bad-news hoarding (Jin and Myers (2006), Hutton, Marcus, and Tehranian (2009)), which we consider in Section IV.E, could correlate with disagreement. A valid instrument removes these biases, as long as it is statistically strong (which ours is).

[25] They are larger than the corresponding OLS estimates shown in Table IA.II in the Internet Appendix, confirming that OLS yields downward biased estimates in our setting.

using the dispersion or range of quarter-ahead forecasts to proxy for disagreement, whereas the measures based on fiscal-year forecasts and abnormal short interest have a uniformly strong and statistically significant effect on each of our five crash risk measures. The same is true for trading volume around earnings announcement, except in the case of the $DUVOL$ measure ($p$=0.134). The EDGAR-inclusion instrument is statistically strong in each of the 30 specifications, with $F$-statistics exceeding the rule-of-thumb value of 10. This alleviates concerns that our 2SLS estimates are subject to weak-instrument bias.

The positive coefficients reported in Table VI are consistent with Hong and Stein's (2003) model of crash risk. To get a sense of their economic magnitude, we compute elasticities measured as the effect of a 1% increase in each of the six disagreement measures from their respective pre-treatment mean. The estimated elasticities for the forecast-based measures and for abnormal short interest vary between 1.1 and 4.4, except in the case of $CRASH1$ where the elasticities vary between 0.3 and 0.6.[26] The estimated elasticities for the trading-volume measure are considerably larger, varying between 2.0 (for $CRASH1$) and 14.1 (for $NCSKEW$).

Table IA.III in the Internet Appendix explores the robustness of these findings using two less common measures of crash risk: Jin and Myers' (2006) $COUNT$ and $COLLAR$, each evaluated at the $x = 0.01\%, 0.1\%$, or $1\%$ levels.[27] We find a positive effect of disagreement on crash risk in each of the $6 \times 6 = 36$ regressions, and statistically significant effects in 24 of them.

Overall, our 2SLS estimates in Tables VI and IA.III lend support to models in the crash-risk literature that link crash risk to investor disagreement.

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[26] The less demanding our definition of $CRASHx$, the lower the elasticity, which makes sense economically: presumably, greater disagreement increases the incidence of otherwise rare negative-tail events by more than the incidence of relatively more common negative-tail events.

[27] $COUNTx$ captures the difference between the number of extreme negative returns and extreme positive returns, evaluated at the $x = 0.01\%, 0.1\%$, or $1\%$ levels. $COLLARx$ accounts for the magnitude as well as the frequency of extreme returns by computing the profit or loss of a hypothetical strategy of going long an out-of-the-money put option on the residual return and shorting a call option on the residual return, with the strike price of the put chosen such that it would be in the money with frequencies of $x = 0.01\%, 0.1\%$, and $1\%$ in a lognormal distribution.

*D. Asymmetry*

Models of the effect of investor disagreement on stock price crash risk predict an asymmetric relation: changes in disagreement affect crash risk but not price jumps. We next test whether or not extreme positive returns also become more likely after EDGAR inclusion. As noted in Section IV.A.2, we use Hutton, Marcus, and Tehranian's (2009) $JUMPx$ measures for this test. Table VII reports the $6 \times 3 = 18$ 2SLS estimates. Each of the 18 estimates is statistically no different from zero, and most are economically small. We can therefore reject the hypothesis that EDGAR inclusion leads to a symmetric increase in the incidence of extreme returns.


INSERT TABLE VII ABOUT HERE


Asymmetry implies that EDGAR inclusion does not simply reduce volatility; it reduces *downside* volatility (the occurrence of extreme negative returns). Asymmetry thus supports the interpretation that EDGAR inclusion reduces crash risk. This, in turn, supports the asymmetric effect of disagreement on crash risk predicted by models such as Hong and Stein (2003).

*E. Alternative Channel: Bad-News Hoarding*

A causal interpretation of our 2SLS estimates in Tables VI and IA.III requires that the instrument (EDGAR inclusion) affect crash risk only through its effect on disagreement and not directly or through another channel. While it is never possible to "prove" that an instrument satisfies the exclusion restriction, we investigate potential violations of the exclusion restriction through the lens of the leading alternative explanation for crash risk: bad-news hoarding.

Jin and Myers (2006) propose a model of crash risk that does not involve disagreement. Managers control the firm's transparency and have incentives to stockpile bad news (withholding information or managing earnings).[28] A sudden release of bad news, perhaps once a tipping point

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[28] The accounting literature has long recognized managers' tendency to withhold bad news (Graham, Harvey, and Rajgopal (2005)). Disappointing earnings news can adversely affect managers' career prospects or compensation (Verrecchia (2001)).

is reached, can lead to a stock price crash. The identification question then becomes whether EDGAR inclusion reduces the risk of pent-up bad news being released in future, either directly or through any effect EDGAR inclusion may have on managerial behavior. To investigate if bad-news hoarding might contribute to the observed reduction in crash risk, we study six standard financial reporting measures: return on assets, two measures of discretionary accruals, the tendency for reported earnings to narrowly "meet or beat" analyst consensus, earnings restatements, and breaks in strings of earnings increases. We find no evidence suggesting that managers vary what news they release or how transparent their financial reporting is.

Table VIII reports the results. Column 1 shows that return on assets is no different after EDGAR inclusion. In other words, we see no sudden release of bad news – in the form of lower earnings – that might reduce the risk of pent-up bad news being released in future. Columns 2 and 3 show that earnings management (discretionary accruals obtained from a modified Jones model and performance-matched discretionary accruals) is unchanged. In other words, firms do not manage earnings less aggressively after joining EDGAR. Column 4 considers an alternative measure of transparency: the tendency for a firm's earnings to narrowly meet-or-beat analyst consensus.[29] We find no evidence that firms become any less likely to meet-or-beat consensus when they become EDGAR filers. Column 5 shows that the likelihood of subsequent earnings restatements – a key way for firms to release bad news – is unchanged following EDGAR inclusion. Column 6, finally, shows that the likelihood that a firm breaks a run of earnings increases – another indication of bad news coming out – is similarly unchanged following EDGAR inclusion.[30]

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[29] Malmendier and Tate (2009) show that the 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 a zero to one cent difference between reported earnings and consensus.

[30] We choose not to replicate Guo et al.'s (2019) test of accounting conservatism. The reason is that their test involves regressing EPS on two endogenous variables – signed stock returns and negative stock returns – each interacted with the EDGAR treatment. Given that EDGAR causes crash risk to fall (see Section IV.B), negative stock returns are clearly endogenous, and as we show in Section III, so are signed stock returns. The positive coefficient Guo et al. (2019) find for the interaction of negative stock returns and EDGAR inclusion thus may or may not imply that firms manage their earnings more conservatively post-EDGAR. Our own tests – which do not suffer from this type of endogeneity problem – suggest that firms do not change how they manage their earnings.

INSERT TABLE VIII ABOUT HERE

The findings in Table VIII suggest that firms saw little need to alter their financial reporting behavior, perhaps because they did not feel more closely monitored by investors as a result of joining EDGAR. Assuming that monitoring intensity increases in investor size, this pattern fits the fact that EDGAR inclusion reduced the cost of accessing corporate filings primarily among small investors (both retail and institutional), as large investors likely already had access to corporate filings via commercial data vendors (Chang, Ljungqvist, and Tseng (2021)).

It is not our intention in this paper to run a horse race between disagreement-based models and bad-news hoarding models of stock price crash risk. The results reported in Tables V, VI, VII, IA.III, and Figure 5 are consistent with disagreement-based models of crash risk while the results in Table VIII appear to lend no support to bad-news hoarding models. Still, given that absence of evidence is not evidence of absence, we cannot rule out that alternative measures of transparency might change around EDGAR inclusion in ways that would support bad-news hoarding models. Our findings in this section should be interpreted with this caveat in mind.

*F. Cross-sectional Analyses*

To provide corroborating evidence in support of a disagreement-based interpretation of the observed reduction in stock price crash risk following EDGAR inclusion reported in Sections IV.B and IV.C, we test two key determinants of crash risk in disagreement models: how tightly binding a firm's short-sale constraints are, and how optimistic investors are.

*F.1. The Tightness of Short-sale Constraints*

Disagreement models imply that cross-sectionally, the effect of investor disagreement on crash risk should be stronger when short-sale constraints are tighter. To test this implication, we use two proxies for short-sale constraints: beta and institutional ownership. The use of beta is motivated by theory. In Hong and Sraer's (2016) model, investors disagree over the common component in firms' cash flows. Disagreement is naturally stronger for high-beta stocks than for

low-beta stocks, as the cash flows of high-beta stocks covary more with the macroeconomy and thus have a larger common component. A key implication of Hong and Sraer's model is that high-beta stocks have tighter short-sale constraints than low-beta stocks, all else equal.[31]

As noted in Section III, firms with high institutional ownership are thought to have lower short-sale constraints, given that institutions are the main suppliers of stock loans (D'Avolio (2002)). Because the level of institutional ownership (IO for short) correlates strongly with firm size, we follow Nagel (2005) and construct residual IO from quarterly cross-sectional regressions of logit transformed IO on firm size. We predict that firms with high residual IO (those not in the bottom three deciles) experience a smaller reduction in crash risk after joining EDGAR.

We use a triple-differences approach to examine the role of short-sale constraints in mediating the effect of disagreement on crash risk. In Table IX, Panel A, we measure $beta$ using daily stock returns in the fiscal quarter before EDGAR inclusion and interact $beta$ with the usual $treat$ and $post$ variables used in DD models. The variable of interest is the triple-interaction $treat \times post \times beta$.[32] Assuming that higher-beta stocks are harder to short, as Hong and Sraer (2016) argue, we predict a larger (more negative) treatment effect the higher is $beta$. Consistent with this prediction, the triple-diff estimates of the effect of EDGAR inclusion on crash risk are negative, suggesting that stocks experience a more pronounced reduction in crash risk following EDGAR inclusion the higher their $beta$. The triple-diff coefficients are statistically significantly different from zero for $NCSKEW$ ($p$=0.001), $DUVOL$ ($p$=0.006), and $CRASH01$ ($p$=0.065).


INSERT TABLE IX ABOUT HERE

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[31] We did not use beta as a proxy for short-sale constraints in the contemporaneous-returns tests in Table IV, given that beta affects returns directly according to standard asset pricing models.

[32] Even though Table IX only reports the effects of interest ($treated \times post$ and $treated \times post \times beta$), our triple-diff specifications include all necessary interactions. As $beta$ is time-invariant (it is measured as of the pre-treatment quarter), both $beta$ and $treated \times beta$ are collinear with the firm fixed effects and so drop out of the estimation.

Table IX, Panel B reports similar findings for institutional ownership. Firms with high residual IO pre-treatment experience substantially smaller reductions in crash risk after joining EDGAR, as expected. The triple-diff coefficients in Panel B are statistically significantly different from zero for four of the five crash-risk measures.

Assuming that beta and institutional ownership are valid proxies for how binding a firm's short-sale constraints are, we interpret these patterns as being at least weakly supportive of the role short-sale constraints play in transmitting investor disagreement to stock price crash risk.

### F.2. Investor Optimism

Miller's (1977) model offers a second testable cross-sectional implication: the effect of disagreement on stock price crash risk increases in the marginal investor's optimism. Intuitively, the marginal investor's valuation exceeds the average belief by the level of disagreement multiplied by the level of her optimism. To investigate the role of optimism, we interact the EDGAR treatment with a measure of investor optimism, namely the firm's pre-treatment PVGO index (Benveniste et al. (2003)). PVGO measures the importance of growth opportunities relative to that of assets in place as priced by the marginal investor. All else equal, a higher PVGO index indicates that the marginal investor values a firm's stock more on the basis of expected future growth opportunities than on the basis of cash flows from assets in place. Prior work suggests that optimism is primarily related to future growth rather than assets in place (Lakonishok, Shleifer, and Vishny (1994), Diether, Malloy, and Scherbina (2002)). We thus expect a larger (more negative) treatment effect the higher a firm's PVGO index.

Table IX, Panel C reports the results. The triple-diff estimates of the effect of EDGAR inclusion on our five crash risk measures are consistently negative, as expected, suggesting that stocks experience a more pronounced reduction in crash risk following EDGAR inclusion the higher their pre-treatment PVGO index. The triple-diff coefficients are statistically significantly different from zero for all proxies except $CRASH01$. Assuming that investor optimism increases in the relative importance of future growth opportunities compared to assets in place, we

interpret these patterns as supporting the prediction from Miller's (1977) model that the effect of investor disagreement on stock price crash risk increases in the marginal investor's optimism.

## V. Conclusions

We investigate the role played by investor disagreement in asset pricing, with particular focus on returns and stock price crash risk. We leverage a randomly assigned, exogenous shock to the informativeness of stock prices and to investors' costs of accessing mandatory corporate disclosures, namely the SEC's staggered roll-out of the EDGAR system between 1993 and 1996 and parallel efforts by the National Science Foundation to put SEC filings online.

We show that standard measures of investor disagreement decrease when a firm's SEC filings are made available to investors online, compared to matched control firms with unchanged information-access costs. This occurs even though neither firms' fundamentals nor their accounting transparency change in detectable ways. EDGAR inclusion helps resolve investor disagreement around earnings announcements and analyst forecasts. At the same time as standard measures of investor disagreement decrease, standard measures of stock price crash risk decrease. Using inclusion in EDGAR as an instrument for disagreement, we report 2SLS results which plausibly permit a causal interpretation of the effect of disagreement on crash risk.

Our plausibly causal findings support the fundamental prediction of a broad class of disagreement models such as Miller (1977), namely that disagreement among investors can lead to overvaluation when coupled with short-sale constraints. Our findings further support the predictions of models that link crash risk to disagreement, such as Hong and Stein (2003).

Beyond allowing us to explore the asset pricing effects of investor disagreement in a more plausibly identified way than has previously been possible, the natural experiment that we exploit helps us investigate the benefits of mandatory disclosure from a novel angle. Our central finding that improved mandatory disclosure leads to less investor disagreement and reduced crash risk highlights a previously undocumented benefit of mandatory-disclosure regulations.

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# Appendix. Variable Definitions

**Measures of investor disagreement**

***Dispersion (next quarter)*** is the standard deviation of analysts' earnings forecasts made in fiscal quarter $t$ for fiscal quarter $t+1$ (I/B/E/S variable $stdev$ with forecast horizon $fpi = 7$), scaled by the end-of-quarter stock price (CRSP variable $prc$) and multiplied by 100 for ease of exposition. I/B/E/S data are obtained from the unadjusted summary history files. See Lehavy, Li, and Merkley (2011) for further details.

***Dispersion (fiscal year)*** is the standard deviation of analysts' earnings forecasts made in fiscal quarter $t$ for the current fiscal year (I/B/E/S variable $stdev$ with forecast horizon $fpi = 1$), scaled by the end-of-quarter stock price (CRSP variable $prc$) and multiplied by 100 for ease of exposition. I/B/E/S data are obtained from the unadjusted summary history files. See Lehavy, Li, and Merkley (2011) for further details.

***Range (next quarter)*** is the difference between the highest and lowest earnings forecasts made by analysts in fiscal quarter $t$ for fiscal quarter $t+1$ (I/B/E/S variable $highest$ and $lowest$ with forecast horizon $fpi = 7$), scaled by the end-of-quarter stock price (CRSP variable $prc$) and multiplied by 100 for ease of exposition. I/B/E/S data are obtained from the unadjusted summary history files. See De Bondt and Forbes (1999) for further details.

***Range (fiscal year)*** is the difference between the highest and lowest earnings forecast made by analysts in fiscal quarter $t$ for the current fiscal year (I/B/E/S variable $highest$ and $lowest$ with forecast horizon $fpi = 1$), scaled by the end-of-quarter stock price (CRSP variable $prc$) and multiplied by 100 for ease of exposition. I/B/E/S data are obtained from the unadjusted summary history files. See De Bondt and Forbes (1999) for further details.

***Abnormal short interest*** is estimated as the fiscal-quarterly average difference between a firm's actual and predicted monthly short interest ratio, multiplied by 100 for ease of exposition. A firm's actual short interest ratio is defined as the number of shares sold short in month $t$ (Compustat variable $shortint$) divided by shares outstanding in month $t$ (CRSP variable $shrout$). A firm's predicted short interest ratio is the fitted value obtained from a cross-sectional regression estimated at a monthly frequency of its actual short interest ratio on a set of indicators for 27 size-, book-to-market-, and momentum-based portfolios, and a set of two-digit SIC indicators (derived from CRSP variable $siccd$). Every month $t$, we assign each stock into one of 27 portfolios, which are formed by independently sorting our sample firms into low, medium, and high partitions by size, book-to-market, and momentum as of month $t-1$. Size is average daily market capitalization, defined as stock price (CRSP variable $prc$) times the number of outstanding shares (CRSP variable $shrout$). Book-to-market is the ratio of book value of equity (Compustat variable $ceqq$) to the market value of equity (Compustat variables $prccq \times cshoq$). Momentum is the cumulative return over the previous twelve months (CRSP variable $ret$). See Karpoff and Lou (2010) for further details. Because the short interest ratio is bounded on the interval [0,1], we estimate the regression as a fractional logit.

***Trading volume around EA*** is the natural logarithm of total trading volume (CRSP variable $vol$) in a three-day window centered on a firm's earnings announcement in fiscal quarter $t$. For NASDAQ-traded stocks, trading volume is adjusted using the Gao and Ritter (2010) procedure.

**Return measures**

$R_{raw}$ is the three-day cumulative return in fiscal quarter $t$ around either the firm's earnings announcement (I/B/E/S variable $anndats$) or the first analyst forecast for the firm's current fiscal year (I/B/E/S variable $fpi = 1$). Daily returns are from CRSP variable $ret$.

$R_e$ is the three-day cumulative market-adjusted return in fiscal quarter $t$ around either the firm's earnings announcement (I/B/E/S variable $anndats$) or the first analyst forecast for the firm's current fiscal year (I/B/E/S variable $fpi = 1$). The daily market-adjusted return is the difference between the daily raw return (CRSP variable $ret$) and the value-weighted market return (CRSP variable $vwretd$).

**Measures of stock price crash risk**

*Skewness (NCSKEW)* is the negative coefficient of skewness for firm $i$ in fiscal quarter $t$, defined as $-\left(n(n-1)^{3/2}\sum R_{it}^3\right)/\left((n-1)(n-2)(\sum R_{it}^2)^{3/2}\right)$, where $R_{it}$ is the daily market-adjusted log return of firm $i$ in fiscal quarter $t$, defined as $R_{it} = \log(1 + \varepsilon_{it})$. $\varepsilon_{it}$ is the residual from the following regression: $r_{it} = \alpha_i + \beta_{1,i}r_{mt-1} + \beta_{2,i}r_{kt-1} + \beta_{3,i}r_{mt} + \beta_{4,i}r_{kt} + \beta_{5,i}r_{mt+1} + \beta_{6,i}r_{kt+1} + \varepsilon_{it}$, where $r_{it}$ is the return of stock $i$ on day $t$ (CRSP variable $ret$), $r_{mt}$ is the daily return on the CRSP value-weighted market index (CRSP variable $vwretd$), and $r_{kt}$ is the Fama and French value-weighted 48-industry index return obtained from Kenneth French's website. See Chen, Hong, and Stein (2001) for further details.

*Down-to-up volatility (DUVOL)* is the down-to-up volatility for firm $i$ in fiscal quarter $t$, defined as $\log\left\{(n_u-1)\sum_{DOWN}R_{it}^2 / \left((n_d-1)\sum_{UP}R_{it}^2\right)\right\}$, where $n_u$ and $n_d$ are the number of up and down days in fiscal quarter $t$, respectively, and $R_{it}$ is the daily market-adjusted log return of firm $i$ in fiscal quarter $t$, defined as $R_{it} = \log(1 + \varepsilon_{it})$. $\varepsilon_{it}$ is the residual from the following regression: $r_{it} = \alpha_i + \beta_{1,i}r_{mt-1} + \beta_{2,i}r_{kt-1} + \beta_{3,i}r_{mt} + \beta_{4,i}r_{kt} + \beta_{5,i}r_{mt+1} + \beta_{6,i}r_{kt+1} + \varepsilon_{it}$, where $r_{it}$ is the return of stock $i$ on day $t$ (CRSP variable $ret$), $r_{mt}$ is the daily return on the CRSP value-weighted market index (CRSP variable $vwretd$), and $r_{kt}$ is the Fama and French value-weighted 48-industry index return obtained from Kenneth French's website. See Chen, Hong, and Stein (2001) for further details.

*Extreme negative returns, 0.01% (CRASH001)* is an indicator variable set equal to one if firm $i$ experiences one or more daily log market-adjusted returns falling $k$ standard deviations below its mean return in fiscal quarter $t$, with $k$ chosen to generate frequencies of 0.01% in the normal distribution. Log market-adjusted returns are calculated as $R_{it} = \log(1 + \varepsilon_{it})$. $\varepsilon_{it}$ is the residual from the following regression: $r_{it} = \alpha_i + \beta_{1,i}r_{mt-1} + \beta_{2,i}r_{kt-1} + \beta_{3,i}r_{mt} + \beta_{4,i}r_{kt} + \beta_{5,i}r_{mt+1} + \beta_{6,i}r_{kt+1} + \varepsilon_{it}$, where $r_{it}$ is the return of stock $i$ on day $t$ (CRSP variable $ret$), $r_{mt}$ is the daily return on the CRSP value-weighted market index (CRSP variable $vwretd$), and $r_{kt}$ is the Fama and French value-weighted 48-industry index return obtained from Kenneth French's website. See Hutton, Marcus, and Tehranian (2009) for further details.

*Extreme negative returns, 0.1% (CRASH01)* is an indicator variable set equal to one if firm $i$ experiences one or more daily log market-adjusted returns falling $k$ standard deviations below its mean return in fiscal quarter $t$, with $k$ chosen to generate frequencies of 0.1% in the normal distribution. Log market-adjusted returns are calculated as $R_{it} = \log(1 + \varepsilon_{it})$. $\varepsilon_{it}$ is the residual from the following regression: $r_{it} = \alpha_i + \beta_{1,i}r_{mt-1} + \beta_{2,i}r_{kt-1} + \beta_{3,i}r_{mt} + \beta_{4,i}r_{kt} + \beta_{5,i}r_{mt+1} + \beta_{6,i}r_{kt+1} + \varepsilon_{it}$, where $r_{it}$ is the return of stock $i$ on day $t$ (CRSP variable $ret$), $r_{mt}$ is the daily return on the CRSP value-weighted market index (CRSP variable $vwretd$), and $r_{kt}$ is the Fama and French value-weighted 48-industry index return obtained from Kenneth French's website. See Hutton, Marcus, and Tehranian (2009) for further details.

*Extreme negative returns, 1% (CRASH1)* is an indicator variable set equal to one if firm $i$ experiences one or more daily log market-adjusted returns falling $k$ standard deviations below its mean return in fiscal quarter $t$, with $k$ chosen to generate frequencies of 1% in the normal distribution. Log market-adjusted returns are calculated as $R_{it} = \log(1 + \varepsilon_{it})$. $\varepsilon_{it}$ is the residual from the following regression: $r_{it} = \alpha_i + \beta_{1,i}r_{mt-1} + \beta_{2,i}r_{kt-1} + \beta_{3,i}r_{mt} + \beta_{4,i}r_{kt} + \beta_{5,i}r_{mt+1} + \beta_{6,i}r_{kt+1} + \varepsilon_{it}$, where $r_{it}$ is the return of stock $i$ on day $t$ (CRSP variable $ret$), $r_{mt}$ is the daily return on the CRSP value-weighted market index (CRSP variable $vwretd$), and $r_{kt}$ is the Fama and French value-weighted 48-industry index return obtained from Kenneth French's website. See Hutton, Marcus, and Tehranian (2009) for further details.

*Extreme positive returns, 0.01% (JUMP001)* is an indicator variable set equal to one if firm $i$ experiences one or more daily log market-adjusted returns exceeding its mean return in fiscal quarter $t$ by $k$ standard deviations, with $k$ chosen to generate frequencies of 0.01% in the normal distribution. Log market-adjusted returns are calculated as $R_{it} = \log(1 + \varepsilon_{it})$. $\varepsilon_{it}$ is the residual from the following regression: $r_{it} = \alpha_i + \beta_{1,i}r_{mt-1} + \beta_{2,i}r_{kt-1} + \beta_{3,i}r_{mt} + \beta_{4,i}r_{kt} + \beta_{5,i}r_{mt+1} + \beta_{6,i}r_{kt+1} + \varepsilon_{it}$, where $r_{it}$ is the return of stock $i$ on day $t$ (CRSP variable $ret$), $r_{mt}$ is the daily return on the CRSP value-weighted market index (CRSP variable

$vwretd$), and $r_{kt}$ is the Fama and French value-weighted 48-industry index return obtained from Kenneth French's website. See Hutton, Marcus, and Tehranian (2009) for further details.

***Extreme positive returns, 0.1% (JUMP01)*** is an indicator variable set equal to one if firm $i$ experiences one or more daily log market-adjusted returns exceeding its mean return in fiscal quarter $t$ by $k$ standard deviations, with $k$ chosen to generate frequencies of 0.1% in the normal distribution. Log market-adjusted returns are calculated as $R_{it} = \log(1 + \varepsilon_{it})$. $\varepsilon_{it}$ is the residual from the following regression:
$r_{it} = \alpha_i + \beta_{1,i}r_{mt-1} + \beta_{2,i}r_{kt-1} + \beta_{3,i}r_{mt} + \beta_{4,i}r_{kt} + \beta_{5,i}r_{mt+1} + \beta_{6,i}r_{kt+1} + \varepsilon_{it}$, where $r_{it}$ is the return of stock $i$ on day $t$ (CRSP variable $ret$), $r_{mt}$ is the daily return on the CRSP value-weighted market index (CRSP variable $vwretd$), and $r_{kt}$ is the Fama and French value-weighted 48-industry index return obtained from Kenneth French's website. See Hutton, Marcus, and Tehranian (2009) for further details.

***Extreme positive returns, 1% (JUMP1)*** is an indicator variable set equal to one if firm $i$ experiences one or more daily log market-adjusted returns exceeding its mean return in fiscal quarter $t$ by $k$ standard deviations, with $k$ chosen to generate frequencies of 1% in the normal distribution. Log market-adjusted returns are calculated as $R_{it} = \log(1 + \varepsilon_{it})$. $\varepsilon_{it}$ is the residual from the following regression:
$r_{it} = \alpha_i + \beta_{1,i}r_{mt-1} + \beta_{2,i}r_{kt-1} + \beta_{3,i}r_{mt} + \beta_{4,i}r_{kt} + \beta_{5,i}r_{mt+1} + \beta_{6,i}r_{kt+1} + \varepsilon_{it}$, where $r_{it}$ is the return of stock $i$ on day $t$ (CRSP variable $ret$), $r_{mt}$ is the daily return on the CRSP value-weighted market index (CRSP variable $vwretd$), and $r_{kt}$ is the Fama and French value-weighted 48-industry index return obtained from Kenneth French's website. See Hutton, Marcus, and Tehranian (2009) for further details.

## Other variables

***Absolute abnormal return*** is the absolute value of the cumulative daily abnormal return in a three-day window centered on a firm's earnings announcement in fiscal quarter $t$. Daily abnormal return is defined as the daily holding period return (CRSP variable $ret$) minus the value-weighted market return with dividends (CRSP variable $vwretd$).

***Market capitalization*** is firm $i$'s equity market capitalization (Compustat variable $prccq$ times Compustat variable $cschoq$) on the last trading day of fiscal quarter $t$.

***Momentum*** is firm $i$'s compounded stock return over the previous six months. Monthly stock returns are from CRSP variable $ret$.

***Lagged share price*** is firm $i$'s stock price (Compustat variable $prccq$) at the end of the previous fiscal quarter.

***Change in institutional ownership*** is the quarterly change in institutional ownership between fiscal quarters $t = -2$ and $t = -1$, where quarter $t = 0$ is the quarter of EDGAR inclusion. Institutional ownership is taken from the Thomson Financial Institutional Holdings (13F) database, defined as the sum of shares held by institutional investors (variable $shares$) divided by total number of shares outstanding (CRSP variable $shrout$).

***Return on assets (ROA)*** is firm $i$'s return on assets in fiscal quarter $t$, defined as earnings (Compustat variable $niq$) divided by the firm's total assets as of the end of the previous fiscal quarter (Compustat variable $atq$).

***Jones discretionary accruals*** 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 $atq$), $\Delta REV_{iq}$ 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 $DA_{iq} = (TA_{iq}/ASSET_{iq-1}) - NA_{iq}$, where $NA_{iq} = \widehat{\beta_0} + \widehat{\beta_1} + 1/ASSET_{iq-1} + \widehat{\beta_2}(\Delta REV_{iq} - \Delta AR_{iq})/ASSET_{iq-1} + \widehat{\beta_3} \, PPE_{iq}/ASSET_{iq-1}$ and $AR_{iq}$ is accounts receivable (Compustat variable $rectq$).

***Performance-matched discretionary accruals*** is firm $i$'s discretionary accruals in fiscal quarter $t$ following Kothari, Leone, and Wasley (2005), defined as a firm's discretionary accruals from a modified Jones model minus the discretionary accruals of a matched firm in the same Fama-French 48 industry with the closest return on assets.

***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 of analysts' earnings forecasts.

***Earnings restatement*** is an indicator variable set equal to one if the absolute difference between firm $i$'s quarter $t$ I/B/E/S earnings per share (variable $value$) and Compustat earnings per share (variable $epspxq$) is equal to or greater than 0.015. This definition follows Livnat and Mendenhall (2006).

***Breaks in string of earnings increases*** is an indicator variable set equal to one if firm $i$'s quarter $t$ earnings (Compustat variable $niq$) decrease after having increased in each of the previous four quarters. This definition follows Andreou, Louca, and Petrou (2017).

***Beta*** is the coefficient on the market index (CRSP variable $vwretd$) obtained from a market model estimated using daily stock returns (CRSP variable $ret$) over the four fiscal quarters preceding quarter $t$.

***IO*** is an indicator variable set equal to one if firm $i$'s residual institutional ownership is in the top seven deciles in fiscal quarter $t$. Following Nagel (2005), firm $i$'s residual institutional ownership in fiscal quarter $t$ is defined as the residual from a cross-sectional regression of the logit-transformed level of institutional ownership on the log and the squared log of the firm's market value of equity. Market value of equity is defined as the end-of-quarter stock price (Compustat variable $prccq$) multiplied by the number of shares outstanding (Compustat variable $cshoq$). Institutional ownership is taken from the Thomson Financial Institutional Holdings (13F) database, defined as the sum of shares held by institutional investors (variable $shares$) divided by total number of shares outstanding (CRSP variable $shrout$). Institutional ownership below 0.0001 and above 0.9999 are replaced with 0.0001 and 0.9999, respectively.

***PVGO Index*** is a proxy for the relative importance of growth opportunities and earnings from assets in place in investors' valuation of a company's stock (Benveniste et al. (2003)). It is calculated as $PVGO/P \equiv (P - EPS/R)/P$, where $P$ is firm $i$'s share price (Compustat variable $prccq$) on the last trading day of fiscal quarter $t$, $EPS$ is diluted earnings per share in fiscal quarter $t$ (Compustat variable $epsfxq$ divided by CRSP variable $prc$), and $R$ is firm $i$'s industry cost of capital, measured as the sum of the risk-free rate (from Kenneth French's website) and the Fama-French 48-industry risk premium (from Fama and French (1997)). If $EPS$ is negative, we set $PVGO/P$ equal to one. If $EPS/R$ is greater than $P$, we set $PVGO/P$ equal to zero.
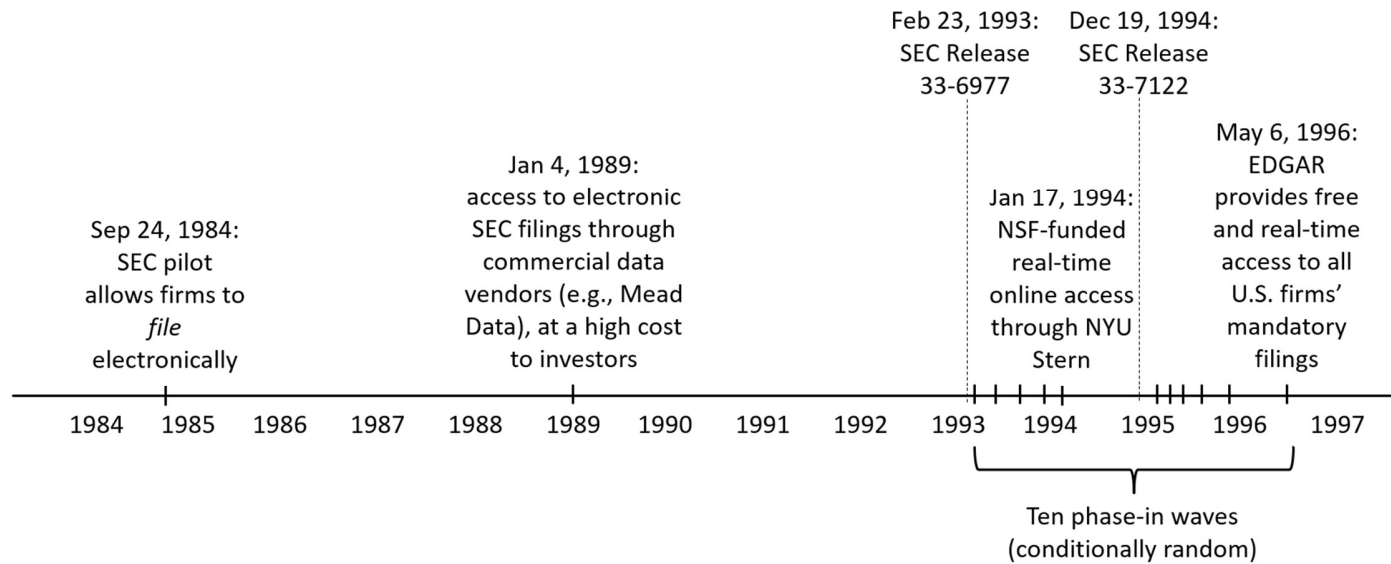
**Figure 1. Timeline of EDGAR Implementation.** The figure shows the major milestones in the SEC's implementation of EDGAR. SEC Release 33-6977 is the SEC's announcement of its plan to require all registered firms to submit their filings electronically, in ultimately ten waves. The release contains the 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.
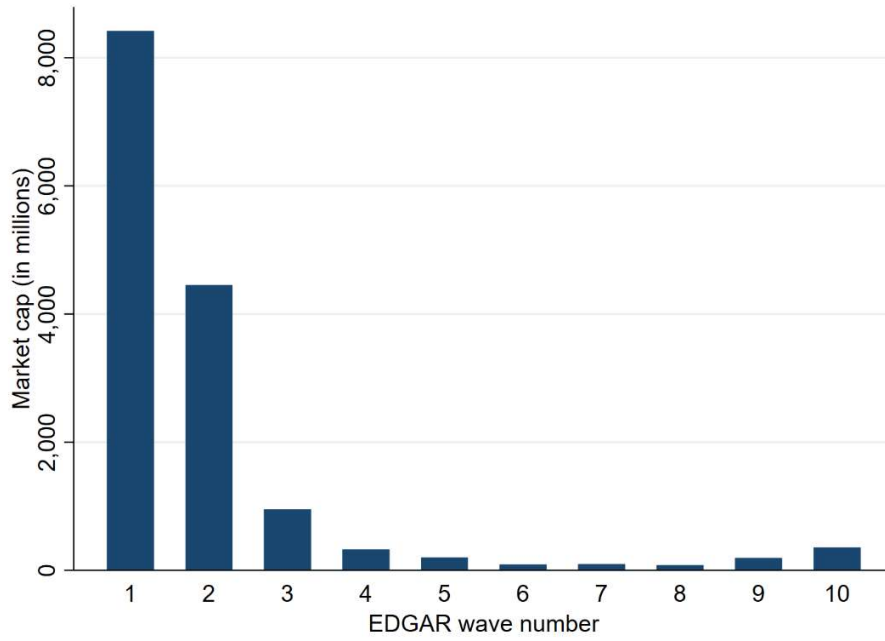
**Figure 2. EDGAR Phase-in Waves.** The figure shows the average equity market capitalization of firms included in each of the ten EDGAR phase-in waves. See Table I for further details.
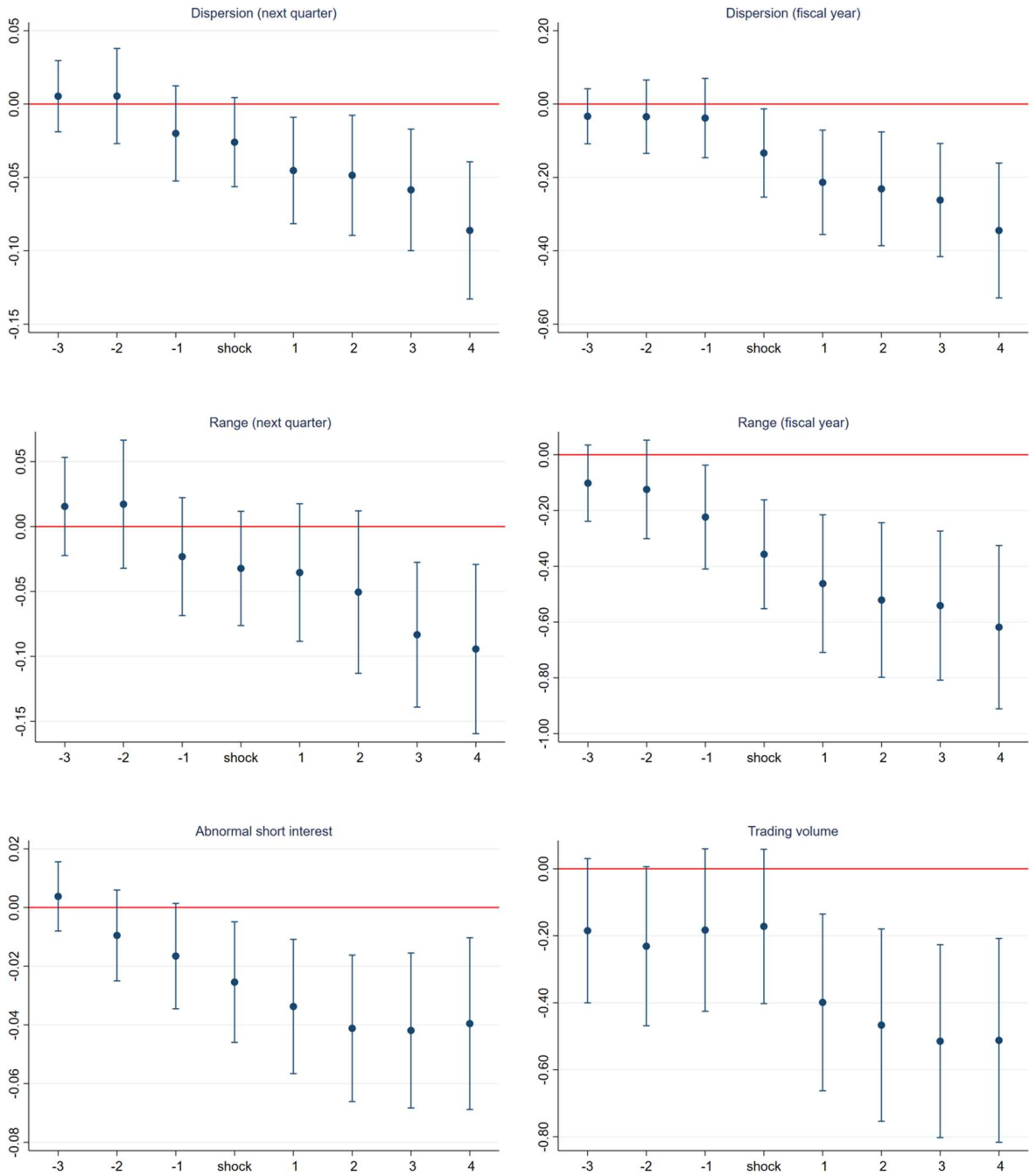
**Figure 3. Dynamics of the Treatment Effect.** The figure graphs dynamic difference-in-differences estimates of the effects of inclusion in EDGAR on investor disagreement. Treated firms are those included in EDGAR at time 0; control firms are nearest-neighbor propensity-scored matched on equity market capitalization (in levels and logs) 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 as well as fixed effects for calendar-quarter, fiscal-quarter, and firm). For variable definitions and details of their construction see the Appendix.

**Figure 4. Quantile Regressions: Disagreement Measures.** The figure graphs quantile-regression DD estimates of the effects of inclusion in EDGAR on investor disagreement. 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) and fiscal quarter using a 0.05 caliper. All specifications are estimated using quantile regressions (Koenker and Bassett (1978)) and include controls (the one-quarter lag of log market cap as well as fixed effects for calendar-quarter, fiscal-quarter, and firm). The dashed lines indicate 95% bootstrapped confidence intervals. For variable definitions and details of their construction see the Appendix.

**Figure 5. Quantile Regressions: Stock Price Crash Risk Measures.** The figure graphs quantile-regression DD estimates of the effects of inclusion in EDGAR on stock price crash risk for our two continuous crash measures. 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) and fiscal quarter using a 0.05 caliper. All specifications are estimated using quantile regressions (Koenker and Bassett (1978)) and include controls (the one-quarter lag of log market cap, momentum, and stock price as well as fixed effects for calendar-quarter, fiscal-quarter, and firm). The dashed lines indicate 95% bootstrapped confidence intervals. For variable definitions and details of their construction see the Appendix.

## Table I
## 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 exclude financials (SIC code 6) and utilities (SIC code 49) and require the existence of a valid control firm using a nearest-neighbor propensity-score method matching on equity market capitalization (in levels and logs) 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.

| Phase-in wave no. | SEC designation | Phase-in date | All listed U.S. firms | | All listed U.S. firms (excluding financials and utilities) | | Treated firms | |
|---|---|---|---|---|---|---|---|---|
| | | | No. of firms | Mean market cap ($m) | No. of firms | Mean market cap ($m) | No. of firms | Mean market cap ($m) |
| 1 | CF-01 | April 26, 1993 | 105 | 8,418.5 | 79 | 10,114.1 | 23 | 538.1 |
| 2 | CF-02 | July 19, 1993 | 405 | 4,450.6 | 272 | 5,139.6 | 50 | 1,007.8 |
| 3 | CF-03 | October 4, 1993 | 416 | 952.0 | 325 | 961.4 | 171 | 374.3 |
| 4 | CF-04 | December 6, 1993 | 599 | 326.7 | 474 | 354.5 | 397 | 217.4 |
| 5 | CF-05 | January 30, 1995 | 664 | 198.6 | 564 | 189.6 | 441 | 119.5 |
| 6 | CF-06 | March 6, 1995 | 566 | 91.4 | 486 | 80.0 | 372 | 66.6 |
| 7 | CF-07 | May 1, 1995 | 458 | 97.1 | 343 | 55.3 | 240 | 55.2 |
| 8 | CF-08 | August 7, 1995 | 246 | 79.1 | 182 | 84.9 | | |
| 9 | CF-09 | November 6, 1995 | 132 | 191.1 | 63 | 141.1 | | |
| 10 | CF-10 | May 6, 1996 | 905 | 356.9 | 677 | 336.4 | | |
| All | | | 4,496 | 860.5 | 3,465 | 890.2 | 1,694 | 179.4 |

## Table II
## Summary Statistics

The table reports summary statistics for the variables used in our analysis, separately for treated and control firms and measured in either levels or changes as of the quarter before treatment. Treated firms are those included in EDGAR; control firms are nearest-neighbor propensity-scored matched on equity market capitalization (in levels and logs) and fiscal quarter using a 0.05 caliper. All variables are measured at the firm/fiscal-quarter level. For variable definitions and details of their construction see the Appendix. The final two columns report a test of diverging pre-trends, that is, whether the average 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 - Controls | |
| | Treated firms | | | Control firms | | | Treated firms | | | Control firms | | | | |
| | # obs. | Mean | Std. dev. | # obs. | Mean | Std. dev. | # obs. | Mean | Std. dev. | # obs. | Mean | Std. dev. | Diff-erence | $t$-stat |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Disagreement measures:** | | | | | | | | | | | | | | |
| Dispersion (next quarter) | 452 | 0.268 | 0.354 | 583 | 0.250 | 0.386 | 399 | 0.000 | 0.289 | 517 | 0.022 | 0.257 | -0.022 | -1.196 |
| Dispersion (fiscal year) | 794 | 0.915 | 1.497 | 948 | 0.907 | 1.631 | 762 | -0.017 | 0.903 | 900 | 0.009 | 1.177 | -0.026 | -0.502 |
| Range (next quarter) | 452 | 0.375 | 0.444 | 583 | 0.331 | 0.439 | 399 | -0.040 | 0.437 | 517 | 0.009 | 0.329 | -0.049 | -1.942 |
| Range (fiscal year) | 794 | 1.757 | 2.594 | 948 | 1.709 | 2.891 | 762 | -0.178 | 1.830 | 900 | -0.040 | 2.014 | -0.138 | -1.455 |
| Abnormal short interest | 1,685 | -0.152 | 0.438 | 1,664 | -0.235 | 0.388 | 1,671 | -0.004 | 0.165 | 1,633 | -0.003 | 0.173 | -0.001 | -0.103 |
| Trading volume around EA | 1,336 | 8.337 | 4.732 | 1,299 | 8.838 | 4.782 | 1,178 | 0.458 | 3.256 | 1,125 | 0.481 | 3.275 | -0.023 | -0.172 |
| **Crash risk measures:** | | | | | | | | | | | | | | |
| Skewness (*NCSKEW*) | 1,677 | -0.109 | 0.701 | 1,653 | -0.038 | 0.718 | 1,668 | -0.034 | 0.947 | 1,626 | -0.049 | 1.026 | 0.015 | 0.441 |
| Down-to-up vola (*DUVOL)* | 1,677 | -0.068 | 0.438 | 1,653 | -0.015 | 0.450 | 1,668 | -0.023 | 0.579 | 1,626 | -0.026 | 0.618 | 0.003 | 0.149 |
| Extreme negative returns | | | | | | | | | | | | | | |
| 0.01% (*CRASH*001) | 1,677 | 0.079 | 0.270 | 1,653 | 0.085 | 0.279 | 1,668 | -0.002 | 0.362 | 1,626 | -0.008 | 0.380 | 0.006 | 0.433 |
| 0.1% (*CRASH*01) | 1,677 | 0.168 | 0.374 | 1,653 | 0.174 | 0.379 | 1,668 | -0.011 | 0.520 | 1,626 | -0.012 | 0.517 | 0.001 | 0.050 |
| 1% (*CRASH*1) | 1,677 | 0.544 | 0.498 | 1,653 | 0.565 | 0.496 | 1,668 | -0.043 | 0.706 | 1,626 | -0.018 | 0.688 | -0.025 | -1.017 |
| **Jump measures:** | | | | | | | | | | | | | | |
| Extreme positive returns | | | | | | | | | | | | | | |
| 0.01% (*JUMP*001) | 1,677 | 0.113 | 0.316 | 1,653 | 0.098 | 0.297 | 1,668 | 0.014 | 0.416 | 1,626 | 0.014 | 0.407 | 0.000 | 0.018 |
| 0.1% (*JUMP*01) | 1,677 | 0.274 | 0.446 | 1,653 | 0.255 | 0.436 | 1,668 | 0.017 | 0.618 | 1,626 | 0.017 | 0.585 | 0.000 | 0.008 |
| 1% (*JUMP*1) | 1,677 | 0.705 | 0.456 | 1,653 | 0.687 | 0.464 | 1,668 | -0.007 | 0.614 | 1,626 | 0.003 | 0.649 | -0.010 | -0.439 |
| **Controls:** | | | | | | | | | | | | | | |
| Market capitalization | 1,694 | 179.4 | 362.5 | 1,694 | 181.0 | 378.3 | 1,678 | 6.805 | 50.927 | 1,647 | 6.389 | 60.070 | 0.417 | 0.216 |

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**Table III**
**The Effect of Mandatory Disclosure on Investor Disagreement: DD Estimates**

The table reports difference-in-differences estimates of the effects of inclusion in EDGAR on six standard measures of investor disagreement: dispersion in analysts' earnings forecasts for the next fiscal quarter or the next fiscal year, the high-minus-low range of analysts' earnings forecasts for the next fiscal quarter or the next fiscal year, abnormal short interest, and share trading volume in the three days around earnings announcements. Treated firms are those included in EDGAR; control firms are nearest-neighbor propensity-scored matched on equity market capitalization (in levels and logs) 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 as well as fixed effects for calendar-quarter, fiscal-quarter, and firm). The test for pre-trends is a Wald test of the null that the coefficients in each pre-treatment quarter estimated in the corresponding event-study dynamic DD specification are jointly zero. For variable definitions and details of their construction see the Appendix. 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.

| | Dispersion (next quarter) (1) | Dispersion (year-ahead) (2) | Range (next quarter) (3) | Range (year-ahead) (4) | Abnormal short interest (5) | Trading volume around EA (6) | Trading volume around EA (7) |
|---|---|---|---|---|---|---|---|
| Quarter of EDGAR inclusion | -0.020 | -0.099** | -0.030* | -0.242*** | -0.020*** | -0.038 | -0.108 |
| | *0.012* | *0.050* | *0.017* | *0.079* | *0.007* | *0.095* | *0.119* |
| × *absolute abnormal return* | | | | | | | 0.022 |
| | | | | | | | *0.015* |
| Next four quarters | -0.054*** | -0.228*** | -0.064*** | -0.406*** | -0.033*** | -0.307*** | -0.255** |
| | *0.015* | *0.058* | *0.020* | *0.101* | *0.010* | *0.093* | *0.106* |
| × *absolute abnormal return* | | | | | | | -0.005 |
| | | | | | | | *0.009* |
| *absolute abnormal return* | | | | | | | -0.004 |
| | | | | | | | *0.005* |
| Controls? | yes | yes | yes | yes | yes | yes | yes |
| Calendar quarter FE? | yes | yes | yes | yes | yes | yes | yes |
| Fiscal quarter FE? | yes | yes | yes | yes | yes | yes | yes |
| Firm FE? | yes | yes | yes | yes | yes | yes | yes |
| *R*-squared | 67.2% | 66.6% | 64.3% | 69.7% | 79.6% | 75.9% | 76.4% |
| Pre-trends (*p*-value) | 0.333 | 0.825 | 0.258 | 0.102 | 0.091 | 0.224 | 0.317 |
| No. of firms | 1,582 | 2,059 | 1,582 | 2,059 | 3,388 | 3,235 | 3,235 |
| No. of firm-quarters | 9,237 | 15,141 | 9,237 | 15,141 | 28,907 | 23,099 | 22,126 |

**The Effect of Mandatory Disclosure on Contemporaneous Returns**

The table reports cross-sectional event-study regressions of share price returns around two information events, earnings announcements and analyst forecasts, using two return metrics, $R_{raw}$ (a firm's raw stock return) and $R_e$ (its market-adjusted stock return), each measured from the trading day before the information event to the trading day after, [-1,1]. The coefficient of interest captures the average return differential between treated and control firms. Models such as Miller (1977) predict a negative return differential. EDGAR inclusion facilitates investor access to historical corporate filings and so reduces the scope for heterogeneous interpretations of current earnings news or analyst forecasts among investors. Earnings news and analyst forecasts therefore resolve more investor disagreement for treated stocks (those included in EDGAR) than for control stocks (those not yet included in EDGAR), triggering larger share price falls for treated than for control stocks. Panel B reports placebo tests estimated in the quarter before EDGAR inclusion, when the return differential ought to be zero. Panel C uses the change in institutional ownership ahead of the treatment quarter to proxy for short-sale constraints. Treated firms are those included in EDGAR; control firms are nearest-neighbor propensity-scored matched on equity market capitalization (in levels and logs) and fiscal quarter using a 0.05 caliper. All specifications are estimated using OLS and control for the one-quarter lag of log market cap. For variable definitions and details of their construction see the Appendix. 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.

| | Earnings announcements | | Analyst forecasts | |
| --- | --- | --- | --- | --- |
| | $R_{raw}$ [-1,1] | $R_e$ [-1,1] | $R_{raw}$ [-1,1] | $R_e$ [-1,1] |
| | (1) | (2) | (3) | (4) |
| Panel A: Treatment quarter ($t=0$) | | | | |
| Treated minus control | -0.019** | -0.019** | -0.007* | -0.009** |
| | *0.009* | *0.009* | *0.004* | *0.004* |
| | | | | |
| *R*-squared | 1.2% | 1.1% | 0.4% | 0.5% |
| No. of firms | 1,253 | 1,253 | 1,217 | 1,217 |
| Panel B: Pre-treatment quarter ($t=-1$) | | | | |
| Treated minus control | -0.007 | -0.008 | -0.001 | -0.001 |
| | *0.006* | *0.006* | *0.004* | *0.004* |
| | | | | |
| *R*-squared | 0.4% | 0.4% | 0.0% | 0.0% |
| No. of firms | 1,188 | 1,188 | 1,049 | 1,049 |
| Panel C: Treatment quarter ($t=0$) | | | | |
| Treated minus control | -0.022** | -0.021** | -0.008** | -0.010** |
| | *0.010* | *0.009* | *0.004* | *0.004* |
|   x change in institutional ownership | 0.416*** | 0.469*** | 0.141 | 0.140 |
| | *0.159* | *0.167* | *0.095* | *0.092* |
| | | | | |
| *R*-squared | 1.6% | 1.7% | 0.6% | 0.8% |
| No. of firms | 1,253 | 1,253 | 1,217 | 1,217 |

**Table V**
**The Effect of Mandatory Disclosure on Stock Price Crash Risk: DD Estimates**

The table reports difference-in-differences estimates of the effects of inclusion in EDGAR on five standard measures of stock price crash risk: Chen, Hong, and Stein's (2001) two measures – the negative skewness of returns (*NCSKEW*) and the down-to-up volatility of returns (*DUVOL*) – as well as Hutton, Marcus, and Tehranian's (2009) measures of the frequency of crashes evaluated at the 0.01%, 0.1%, and 1% levels (*CRASH*001, *CRASH*01, and *CRASH*1). Treated firms are those included in EDGAR; control firms are nearest-neighbor propensity-scored matched on equity market capitalization (in levels and logs) 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. A All specifications are estimated using OLS and include controls (the one-quarter lag of log market cap, momentum, and stock price as well as fixed effects for calendar-quarter, fiscal-quarter, and firm). The test for pre-trends is a Wald test of the null that the coefficients in each pre-treatment quarter estimated in the corresponding event-study dynamic DD specification are jointly zero. For variable definitions and details of their construction see the Appendix. 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.

| | Skewness (*NCSKEW*) (1) | Down-to-up volatility (*DUVOL*) (2) | Extreme negative returns, 0.01% (*CRASH*001) (3) | Extreme negative returns, 0.1% (*CRASH*01) (4) | Extreme negative returns, 1% (*CRASH*1) (5) |
|---|---|---|---|---|---|
| Quarter of EDGAR inclusion | 0.007 | 0.000 | -0.003 | -0.005 | -0.011 |
| | *0.022* | *0.014* | *0.008* | *0.012* | *0.016* |
| Next four quarters | -0.049** | -0.031** | -0.027*** | -0.042*** | -0.038*** |
| | *0.020* | *0.012* | *0.007* | *0.010* | *0.014* |
| | | | | | |
| 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 | 17.4% | 19.1% | 15.8% | 15.8% | 13.9% |
| Pre-trends (*p*-value) | 0.562 | 0.141 | 0.957 | 0.816 | 0.397 |
| No. of firms | 3,366 | 3,366 | 3,366 | 3,366 | 3,366 |
| No. of firm-quarters | 28,652 | 28,652 | 28,652 | 28,652 | 28,652 |

**Table VI**
**The Effect of Investor Disagreement on Stock Price Crash Risk: IV Estimates**

The table reports 2SLS regression results of the impact of investor disagreement on stock price crash risk. As in Table III, we use six measures of investor disagreement; as in Table V, we use five measures of crash risk. The table summarizes these $5 \times 6 = 30$ regressions by reporting, in matrix form, only the 30 investor-disagreement coefficients (along with heteroskedasticity consistent standard errors clustered at the firm level), the 30 weak-instrument tests, and the 30 observations counts. Each of the 30 "cells" in the upper half of the table thus represents a separate regression. All specifications are estimated using 2SLS and include controls (the one-quarter lag of log market cap and stock price as well as fixed effects for calendar-quarter, fiscal-quarter, and firm). The instrument in each specification is an indicator set equal to 1 if the firm was included in EDGAR in the previous four fiscal quarters. Table III, columns 1 through 6 report the corresponding first-stage results. For variable definitions and details of their construction see the Appendix. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

**Table VI (Continued)**

| | Crash measure | | | | |
|---|---|---|---|---|---|
| | Skewness (*NCSKEW*) (1) | Down-to-up volatility (*DUVOL*) (2) | Extreme negative returns, 0.01% (*CRASH*001) (3) | Extreme negative returns, 0.1% (*CRASH*01) (4) | Extreme negative returns, 1% (*CRASH*1) (5) |
| Disagreement measure | | | | | |
| Dispersion (next quarter) | 1.063 | 0.833* | 0.573* | 0.621 | 0.972* |
| | *0.801* | *0.491* | *0.319* | *0.410* | *0.545* |
| Dispersion (fiscal year) | 0.340** | 0.189** | 0.159*** | 0.199** | 0.193** |
| | *0.146* | *0.086* | *0.060* | *0.077* | *0.091* |
| Range (next quarter) | 0.946 | 0.741* | 0.510* | 0.552 | 0.864* |
| | *0.719* | *0.450* | *0.293* | *0.372* | *0.502* |
| Range (fiscal year) | 0.209** | 0.116** | 0.098*** | 0.122** | 0.119** |
| | *0.090* | *0.053* | *0.037* | *0.048* | *0.056* |
| Abnormal short interest | 1.822** | 1.103** | 0.954** | 1.470** | 1.226** |
| | *0.904* | *0.556* | *0.389* | *0.574* | *0.616* |
| Trading volume around EA | 0.184** | 0.077 | 0.101*** | 0.138*** | 0.131** |
| | *0.092* | *0.051* | *0.038* | *0.052* | *0.063* |
| Weak-instrument test statistics | | | | | |
| Dispersion (next quarter) | 15.2 | 15.2 | 15.2 | 15.2 | 15.2 |
| Dispersion (fiscal year) | 20.4 | 20.4 | 20.4 | 20.4 | 20.4 |
| Range (next quarter) | 10.3 | 10.3 | 10.3 | 10.3 | 10.3 |
| Range (fiscal year) | 17.2 | 17.2 | 17.2 | 17.2 | 17.2 |
| Abnormal short interest | 10.8 | 10.8 | 10.8 | 10.8 | 10.8 |
| Trading volume around EA | 13.9 | 13.9 | 13.9 | 13.9 | 13.9 |
| No. of firm-quarters | | | | | |
| Dispersion (next quarter) | 9,034 | 9,034 | 9,034 | 9,034 | 9,034 |
| Dispersion (fiscal year) | 14,947 | 14,947 | 14,947 | 14,947 | 14,947 |
| Range (next quarter) | 9,034 | 9,034 | 9,034 | 9,034 | 9,034 |
| Range (fiscal year) | 14,947 | 14,947 | 14,947 | 14,947 | 14,947 |
| Abnormal short interest | 28,589 | 28,589 | 28,589 | 28,589 | 28,589 |
| Trading volume around EA | 22,789 | 22,789 | 22,789 | 22,789 | 22,789 |

**Table VII**
**Asymmetry: Stock Price Jumps**

The table reports 2SLS tests of the effects of investor disagreement on stock price jumps, which disagreement models predict should be zero. We follow Hutton, Marcus, and Tehranian (2009) and measure the frequency of share price jumps at the 0.01%, 0.1%, and 1% levels ($JUMP001$, $JUMP01$, and $JUMP1$). Given six measures of investor disagreement (see Table III), we estimate $6 \times 3 = 18$ regressions. The table summarizes these 18 regressions by reporting, in matrix form, only the 18 investor-disagreement coefficients (along with heteroskedasticity consistent standard errors clustered at the firm level), the 18 weak-instrument tests, and the 18 observations counts. Each of the 18 "cells" in the upper half of the table thus represents a separate regression. All specifications are estimated using 2SLS and include controls (the one-quarter lag of log market cap and stock price as well as fixed effects for calendar-quarter, fiscal-quarter, and firm). The instrument in each specification is an indicator set equal to 1 if the firm was included in EDGAR in the previous four fiscal quarters. Table III, columns 1 through 6 report the corresponding first-stage results. For variable definitions and details of their construction see the Appendix. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

| | Jump measure | | |
| --- | --- | --- | --- |
| | Extreme positive returns, 0.01% ($JUMP001$) | Extreme positive returns, 0.1% ($JUMP01$) | Extreme positive returns, 1% ($JUMP1$) |
| | (1) | (2) | (3) |
| **Disagreement measure** | | | |
| Dispersion (next quarter) | -0.023 | -0.417 | -0.239 |
| | *0.280* | *0.413* | *0.431* |
| Dispersion (fiscal year) | -0.004 | -0.022 | -0.048 |
| | *0.049* | *0.069* | *0.074* |
| Range (next quarter) | -0.020 | -0.371 | -0.212 |
| | *0.249* | *0.370* | *0.384* |
| Range (fiscal year) | -0.003 | -0.013 | -0.029 |
| | *0.030* | *0.042* | *0.045* |
| Abnormal short interest | 0.237 | 0.244 | -0.093 |
| | *0.297* | *0.423* | *0.425* |
| Trading volume around EA | 0.009 | 0.015 | 0.013 |
| | *0.030* | *0.043* | *0.044* |
| | | | |
| **Weak-instrument test statistics** | | | |
| Dispersion (next quarter) | 15.2 | 15.2 | 15.2 |
| Dispersion (fiscal year) | 20.4 | 20.4 | 20.4 |
| Range (next quarter) | 10.3 | 10.3 | 10.3 |
| Range (fiscal year) | 17.2 | 17.2 | 17.2 |
| Abnormal short interest | 10.8 | 10.8 | 10.8 |
| Trading volume around EA | 13.9 | 13.9 | 13.9 |
| | | | |
| **No. of firm-quarters** | | | |
| Dispersion (next quarter) | 9,034 | 9,034 | 9,034 |
| Dispersion (fiscal year) | 14,947 | 14,947 | 14,947 |
| Range (next quarter) | 9,034 | 9,034 | 9,034 |
| Range (fiscal year) | 14,947 | 14,947 | 14,947 |
| Abnormal short interest | 28,589 | 28,589 | 28,589 |
| Trading volume around EA | 22,789 | 22,789 | 22,789 |

## Table VIII
## Firms' Reporting Responses to the EDGAR Treatment

The table reports difference-in-differences estimates of changes in firms' reporting choices around the time of their inclusion in EDGAR, using six standard reporting measures: return on assets, two measures of discretionary accruals, whether reported earnings per share equal analyst consensus ("meet") or exceed consensus by at most one cent ("beat"), earnings restatements, and breaks in strings of earnings increases. Treated firms are those included in EDGAR; control firms are nearest-neighbor propensity-scored matched on equity market capitalization (in levels and logs) 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 as well as fixed effects for calendar-quarter, fiscal-quarter, and firm). The test for pre-trends is a Wald test of the null that the coefficients in each pre-treatment quarter estimated in the corresponding event-study dynamic DD specification are jointly zero. For variable definitions and details of their construction see the Appendix. 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.

| | Return on assets (1) | Jones discretionary accruals (2) | Performance-matched discretionary accruals (3) | Meet-or-beat (4) | Earnings restatements (5) | Breaks in strings of earnings increases (6) |
|---|---|---|---|---|---|---|
| Quarter of EDGAR inclusion | 0.000 | 0.003 | -0.003 | 0.022 | 0.008 | -0.001 |
| | *0.001* | *0.003* | *0.003* | *0.017* | *0.010* | *0.005* |
| Next four quarters | 0.002 | -0.003 | -0.003 | 0.004 | -0.017 | -0.003 |
| | *0.002* | *0.002* | *0.002* | *0.014* | *0.011* | *0.003* |
| | | | | | | |
| 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 | 62.8% | 17.7% | 10.5% | 25.9% | 34.1% | 11.2% |
| Pre-trends (*p*-value) | 0.146 | 0.629 | 0.360 | 0.658 | 0.224 | 0.406 |
| No. of firms | 3,388 | 3,336 | 3,323 | 2,214 | 3,388 | 3,388 |
| No. of firm-quarters | 28,872 | 27,999 | 26,402 | 13,741 | 28,976 | 28,976 |

## Table IX
## The Effect of Mandatory Disclosure on Crash Risk: Heterogeneous Treatments

The table reports triple-difference estimates of the effect of EDGAR inclusion on stock price crash risk. For the corresponding difference-in-differences models, see Table V. In Panels A through C, we interact treatment with two measures of how tightly binding a firm's short-sale constraints are: the firm's pre-treatment CAPM beta (Panel A) and its residual institutional ownership (Panel B). Short-sale constraints are predicted to increase in beta and decrease in institutional ownership (IO). In Panel C, we interact treatment with a measure of investor optimism, namely the firm's pre-treatment PVGO index. A higher PVGO index indicates that investors value the firm's stock more on the basis of expected future growth opportunities than on the basis of cash flows from assets in place, leading to a larger (more negative) expected treatment effect. The interaction variables in all panels are measured as of some time before treatment and so do not vary within firm across time. They and their interaction with $treated$ are thus collinear with the firm fixed effects and excluded from the estimation. (All other interactions are included, though to conserve space, only the coefficients of interest are shown.) Specifically, beta and the PVGO index are measured as of the fiscal quarter before treatment. Following Nagel (2005) and Shleifer (1986), IO is measured as of three quarters before treatment. All specifications are estimated using OLS and include controls (the one-quarter lag of log market cap, momentum, and stock price as well as fixed effects for calendar-quarter, fiscal-quarter, and firm). For variable definitions and details of their construction see the Appendix. 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.

| | Skewness (*NCSKEW*) (1) | Down-to-up volatility (*DUVOL*) (2) | Extreme negative returns, 0.01% (*CRASH*001) (3) | Extreme negative returns, 0.1% (*CRASH*01) (4) | Extreme negative returns, 1% (*CRASH*1) (5) |
|---|---|---|---|---|---|
| | | | Panel A: Pre-treatment beta | | |
| $treated \times post$ | 0.023 | 0.008 | -0.015* | -0.017 | -0.024 |
| | *0.026* | *0.017* | *0.009* | *0.014* | *0.020* |
| $treated \times post \times beta$ | -0.082*** | -0.044*** | -0.009 | -0.024* | -0.015 |
| | *0.026* | *0.016* | *0.009* | *0.013* | *0.019* |
| 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 | 17.5% | 19.1% | 15.8% | 15.8% | 13.9% |
| No. of firms | 3,364 | 3,364 | 3,364 | 3,364 | 3,364 |
| No. of firm-quarters | 28,642 | 28,642 | 28,642 | 28,642 | 28,642 |
| | | | Panel B: Pre-treatment IO | | |
| $treated \times post$ | -0.107*** | -0.066*** | -0.061*** | -0.051** | -0.095*** |
| | *0.041* | *0.026* | *0.014* | *0.021* | *0.030* |
| $treated \times post \times IO$ | 0.098** | 0.060** | 0.049*** | 0.022 | 0.082** |
| | *0.046* | *0.029* | *0.016* | *0.024* | *0.033* |
| 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 | 17.2% | 18.9% | 15.7% | 15.6% | 13.8% |
| No. of firms | 3,226 | 3,226 | 3,226 | 3,226 | 3,226 |
| No. of firm-quarters | 27,845 | 27,845 | 27,845 | 27,845 | 27,845 |

**Table IX (Continued)**

|  | Skewness (*NCSKEW*) (1) | Down-to-up volatility (*DUVOL*) (2) | Extreme negative returns, 0.01% (*CRASH*001) (3) | Extreme negative returns, 0.1% (*CRASH*01) (4) | Extreme negative returns, 1% (*CRASH*1) (5) |
|---|---|---|---|---|---|
| *Panel C: Pre-treatment PVGO index* | | | | | |
| *treated × post* | 0.051 | 0.028 | -0.001 | -0.017 | 0.006 |
|  | *0.034* | *0.021* | *0.012* | *0.018* | *0.025* |
| *treated × post × PVGO index* | -0.133*** | -0.079** | -0.032* | -0.025 | -0.062* |
|  | *0.049* | *0.031* | *0.017* | *0.025* | *0.035* |
| 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 | 17.4% | 19.0% | 15.9% | 15.8% | 13.9% |
| No. of firms | 3,271 | 3,271 | 3,271 | 3,271 | 3,271 |
| No. of firm-quarters | 27,854 | 27,854 | 27,854 | 27,854 | 27,854 |

# INTERNET APPENDIX

# for

# "Testing Disagreement Models" [*]

# TABLE OF CONTENTS

**IA.I. Investor Disagreement and Return Predictability**

In this section, we examine the effect of EDGAR inclusion on return predictability. Miller (1977) shows that in a market with short-sale constraints, the prices of stocks with high investor disagreement primarily reflect the valuations of optimists. As Table IV in the paper shows, joining EDGAR causes the treated firm's stock price to decline initially. Going forward, the treated firm has lower disagreement than the control firm and so has a higher expected return in a Miller (1977) world. In support of this prediction, Diether, Malloy, and Scherbina (2002), Chen, Hong, and Stein (2002), and Yu (2011) find a robust negative correlation between the degree of investor disagreement and subsequent returns in the cross-section. We revisit this evidence using our conditionally randomly assigned shock to investor disagreement.

To test how EDGAR inclusion affects long-run returns, we estimate standard calendar-time portfolio alphas. We form two portfolios, one comprised of treated stocks and the other comprised of control stocks, and a hedge portfolio that goes long in treated stocks and short in control stocks. Portfolio construction closely follows Ljungqvist, Malloy, and Marston (2009). A treated firm enters the treatment portfolio on the last day of the month in which it is included in EDGAR (and analogously for control firms). Each firm remains in the portfolio for three, six, or 12 months. Assuming an equal dollar investment in each stock, we compute monthly calendar-time buy-and-hold portfolio returns as $\sum_{i=1}^{n_t} R_{it} x_{it} / \sum_{i=1}^{n_t} x_{it}$, where $R_{it}$ is stock $i$'s return in month $t$, $n_t$ is the number of stocks in the portfolio in month $t$, and $x_{it}$ is $i$'s compounded monthly return from entering the portfolio through month $t-1$. (In the month a stock enters the portfolio, $x_{it} = 1$.)

Table IA.I reports excess portfolio returns (portfolio returns minus the risk-free rate) as well as abnormal portfolio returns (or alphas) computed as the intercept from a time series regression of the monthly excess portfolio returns on the risk factors suggested by six popular factor models: the Fama-French three-factor model (Fama and French (1993)), the Carhart four-factor model (Carhart (1997)), the Carhart model plus the Pastor-Stambaugh (2003) liquidity factor, the

Fama-French five-factor model (Fama and French (2015)), the q-factor model (Hou, Xue, and Zhang (2015)), and the mispricing factor model (Stambaugh and Yuan (2017)).

Over three- and six-month holding periods, excess returns are significantly greater for firms included in EDGAR than for matched controls that are yet to join EDGAR. The equal-weighted monthly excess return differential in column 1 is 1.33% and 1.06%, respectively. Economically, these return differentials are fairly large, likely because our sample is tilted towards smaller firms for which limits to arbitrage are expected to be stronger. The return differentials do not appear to be driven by differential risk exposures among treated and controls: across the six factor models shown in columns 2 through 7, the long-short portfolio abnormal returns are of a similar magnitude as the excess returns in column 1, and they are consistently statistically significant. Over 12-month holding periods, treated and control stocks have statistically similar abnormal returns, suggesting that the price adjustment triggered by joining EDGAR is eventually completed.

We view the results in Table IA.I as the structural counterparts to Diether, Malloy, and Scherbina's (2002) reduced-form estimates, in the sense that we are able to exploit an exogenous shock to disagreement while they rely on cross-sectional variation. As is well known, cross-sectional variation is open to the challenge that it may reflect unobserved determinants of share price returns (notably, risk factors) that correlate in unknown ways with disagreement. It is reassuring, therefore, that both our structural estimates and their cross-sectional estimates point to a negative effect of disagreement on returns.

## Table IA.I
## Investor Disagreement, Stock Prices, and Return Predictability: Calendar-time Portfolio Alphas

This table reports average monthly buy-and-hold excess returns and factor model alphas (along with *t*-statistics in parentheses) for portfolios consisting of firms joining EDGAR and of control firms. A treated firm's stock enters the treatment portfolio on the last day of the month it is included in EDGAR (and analogously for control firms' stocks). Control firms are nearest-neighbor propensity-scored matched on equity market capitalization (in levels and logs) and fiscal quarter using a 0.05 caliper. The monthly portfolio return in month $t$ is computed as $\sum_{i=1}^{n_t} R_{it} x_{it} / \sum_{i=1}^{n_t} x_{it}$, where $R_{it}$ is stock $i$'s return in month $t$, $n_t$ is the number of stocks in the portfolio in month $t$, and $x_{it}$ is $i$'s compounded monthly return from entering the portfolio through month $t-1$. In the month a stock enters the portfolio, $x_{it} = 1$. The hedge portfolio goes long the treated portfolio and short the control portfolio. We report results for holding periods of three, six, and twelve months. Factor model alphas are computed as the intercept from time series regressions of monthly excess portfolio returns on risk factors. We use the Fama-French three-factor model (Fama and French (1993)), the Carhart four-factor model (Carhart (1997)), the Carhart model including the Pastor-Stambaugh (2003) liquidity factor, the Fama-French five-factor model (Fama and French (2015)), the q-factor model (Hou, Xue, and Zhang (2015)), and the mispricing factor model (Stambaugh and Yuan (2017)).

| Portfolio | Holding period | Excess return (1) | FF 3-factor alpha (2) | FF 4-factor alpha (3) | FF 4-factor + Pastor-Stambaugh alpha (4) | FF 5-factor alpha (5) | HXZ (q-factor) alpha (6) | Mispricing alpha (7) |
|---|---|---|---|---|---|---|---|---|
| controls | 3 months | 0.61% | -1.21% | -1.21% | -0.97% | -0.76% | -0.75% | -0.95% |
|  |  | (0.52) | (-4.06) | (-3.68) | (-2.10) | (-2.34) | (-2.25) | (-1.53) |
| treated | 3 months | 1.94% | 0.39% | 0.36% | 0.36% | 0.71% | 0.88% | 0.74% |
|  |  | (1.90) | (1.83) | (1.71) | (1.12) | (4.21) | (5.16) | (1.69) |
| treated – controls | 3 months | 1.33% | 1.60% | 1.58% | 1.33% | 1.47% | 1.63% | 1.69% |
|  |  | (5.98) | (7.23) | (6.99) | (4.65) | (5.01) | (6.91) | (5.85) |
| controls | 6 months | -0.23% | -0.99% | -1.26% | -1.11% | -0.95% | -1.55% | -1.00% |
|  |  | (-0.25) | (-2.08) | (-2.90) | (-2.41) | (-1.40) | (-2.55) | (-1.37) |
| treated | 6 months | 0.82% | 0.15% | 0.17% | 0.39% | 0.07% | 0.51% | 0.54% |
|  |  | (0.90) | (0.41) | (0.40) | (0.97) | (0.12) | (1.25) | (0.98) |
| treated – controls | 6 months | 1.06% | 1.14% | 1.43% | 1.51% | 1.02% | 2.06% | 1.54% |
|  |  | (2.93) | (3.40) | (6.62) | (6.63) | (2.15) | (5.09) | (3.20) |
| controls | 12 months | 1.39% | 0.07% | 0.10% | 0.13% | -0.02% | -0.02% | 0.07% |
|  |  | (1.59) | (0.15) | (0.21) | (0.26) | (-0.03) | (-0.02) | (0.09) |
| treated | 12 months | 1.56% | 0.42% | 0.44% | 0.45% | 0.40% | 0.36% | 0.60% |
|  |  | (2.14) | (1.49) | (1.46) | (1.50) | (1.02) | (1.03) | (1.21) |
| treated – controls | 12 months | 0.17% | 0.35% | 0.33% | 0.32% | 0.42% | 0.38% | 0.53% |
|  |  | (0.52) | (1.04) | (0.93) | (0.88) | (0.90) | (0.76) | (1.12) |

**Table IA.II**
**The Effect of Investor Disagreement on Stock Price Crash Risk: OLS Estimates**

The table reports OLS regression results of the impact of investor disagreement on stock price crash risk. As in Table III, we use six measures of investor disagreement; as in Table V, we use five measures of crash risk. The table summarizes these $6 \times 5 = 30$ regressions by reporting, in matrix form, only the 30 investor-disagreement coefficients (along with heteroskedasticity consistent standard errors clustered at the firm level). Each of the 30 "cells" thus represents a separate regression. All specifications are estimated using OLS and include controls (the one-quarter lag of log market cap as well as fixed effects for calendar-quarter, fiscal-quarter, and firm). For variable definitions and details of their construction see the Appendix. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

| | Crash measure | | | | |
| --- | --- | --- | --- | --- | --- |
| | Skewness ($NCSKEW$) (1) | Down-to-up volatility ($DUVOL$) (2) | Extreme negative returns, 0.01% ($CRASH001$) (3) | Extreme negative returns, 0.1% ($CRASH01$) (4) | Extreme negative returns, 1% ($CRASH1$) (5) |
| Disagreement measure | | | | | |
| Dispersion (next quarter) | 0.133** | 0.074** | 0.045** | 0.055** | 0.007 |
| | *0.055* | *0.031* | *0.020* | *0.027* | *0.032* |
| Dispersion (fiscal year) | 0.021** | 0.017*** | 0.005 | 0.006 | 0.004 |
| | *0.009* | *0.005* | *0.003* | *0.004* | *0.005* |
| Range (next quarter) | 0.081** | 0.047** | 0.033** | 0.042** | 0.003 |
| | *0.040* | *0.022* | *0.015* | *0.019* | *0.023* |
| Range (fiscal year) | 0.011** | 0.009*** | 0.003 | 0.003 | 0.004 |
| | *0.005* | *0.003* | *0.002* | *0.002* | *0.003* |
| Abnormal short interest | 0.043 | 0.025 | 0.003 | 0.033** | 0.005 |
| | *0.027* | *0.016* | *0.010* | *0.013* | *0.015* |
| Trading volume around EA | -0.002 | -0.005*** | 0.008*** | 0.009*** | 0.003* |
| | *0.003* | *0.001* | *0.001* | *0.001* | *0.002* |

**Table IA.III**
**Robustness Tests: Alternative Crash Risk Measures**

The table reports robustness tests in the form of 2SLS regression results of the impact of investor disagreement on stock price crash risk using two alternative measures of crash risk: Jin and Myers' (2006) $COUNT$ and $COLLAR$ measures, each evaluated at the 0.01%, 0.1%, and 1% levels. Given six measures of investor disagreement (see Table III), we estimate $6 \times 6 = 36$ regressions. The table summarizes these 36 regressions by reporting, in matrix form, only the 36 investor-disagreement coefficients (along with heteroskedasticity consistent standard errors clustered at the firm level), the 36 weak-instrument tests, and the 36 observations counts. Each of the 36 "cells" in the upper half of the table thus represents a separate regression. All specifications are estimated using 2SLS and include controls (the one-quarter lag of log market cap as well as fixed effects for calendar-quarter, fiscal-quarter, and firm). The instrument in each specification is an indicator set equal to 1 if the firm was included in EDGAR in the previous four fiscal quarters. Table III, columns 1 through 6 report the corresponding first-stage results. For variable definitions and details of their construction see the Appendix. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

| | Crash measure | | | | | |
|---|---|---|---|---|---|---|
| | Extreme negative returns net of extreme positive returns, 0.01% (*COUNT*001) | Extreme negative returns net of extreme positive returns, 0.1% (*COUNT*01) | Extreme negative returns net of extreme positive returns, 1% (*COUNT*1) | Put-call profit, 0.01% (*COLLAR*001) | Put-call profit, 0.1% (*COLLAR*01) | Put-call profit, 1% (*COLLAR*1) |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Disagreement measure | | | | | | |
| Dispersion (next quarter) | 0.595 | 0.951 | 1.763 | 0.043** | 0.042** | 0.042** |
| | *0.449* | *0.682* | *1.205* | *0.017* | *0.017* | *0.017* |
| Dispersion (fiscal year) | 0.162** | 0.205* | 0.432** | 0.021* | 0.020* | 0.019* |
| | *0.080* | *0.117* | *0.211* | *0.011* | *0.011* | *0.010* |
| Range (next quarter) | 0.530 | 0.846 | 1.569 | 0.038** | 0.037** | 0.038** |
| | *0.400* | *0.616* | *1.091* | *0.017* | *0.017* | *0.016* |
| Range (fiscal year) | 0.099** | 0.126* | 0.265** | 0.013* | 0.012* | 0.011* |
| | *0.049* | *0.071* | *0.131* | *0.007* | *0.007* | *0.006* |
| Abnormal short interest | 0.730 | 1.366* | 1.954 | 0.571* | 0.539* | 0.463* |
| | *0.460* | *0.746* | *1.265* | *0.319* | *0.304* | *0.268* |
| Trading volume around EA | 0.092* | 0.140* | 0.163 | 0.026 | 0.024 | 0.019 |
| | *0.050* | *0.077* | *0.126* | *0.018* | *0.017* | *0.015* |
| Weak-instrument test statistics | | | | | | |
| Dispersion (next quarter) | 15.2 | 15.2 | 15.2 | 15.2 | 15.2 | 15.2 |
| Dispersion (fiscal year) | 20.4 | 20.4 | 20.4 | 20.4 | 20.4 | 20.4 |
| Range (next quarter) | 10.3 | 10.3 | 10.3 | 10.3 | 10.3 | 10.3 |
| Range (fiscal year) | 17.2 | 17.2 | 17.2 | 17.2 | 17.2 | 17.2 |
| Abnormal short interest | 10.8 | 10.8 | 10.8 | 10.8 | 10.8 | 10.8 |
| Trading volume around EA | 13.9 | 13.9 | 13.9 | 13.9 | 13.9 | 13.9 |
| No. of firm-quarters | | | | | | |
| Dispersion (next quarter) | 9,034 | 9,034 | 9,034 | 9,034 | 9,034 | 9,034 |
| Dispersion (fiscal year) | 14,947 | 14,947 | 14,947 | 14,947 | 14,947 | 14,947 |
| Range (next quarter) | 9,034 | 9,034 | 9,034 | 9,034 | 9,034 | 9,034 |
| Range (fiscal year) | 14,947 | 14,947 | 14,947 | 14,947 | 14,947 | 14,947 |
| Abnormal short interest | 28,589 | 28,589 | 28,589 | 28,589 | 28,589 | 28,589 |
| Trading volume around EA | 22,789 | 22,789 | 22,789 | 22,789 | 22,789 | 22,789 |

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