Testing Disagreement Models [*]

Yen-Cheng Chang[†] Pei-Jie Hsiao[§] Alexander Ljungqvist[¶]
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 with no accompanying changes in company fundamentals, disclosure quality, or earnings management. The reduction in disagreement leads to lower stock price crash risk. The effect is more pronounced for stocks with binding short-sale constraints and high investor optimism. The reduction in disagreement is followed by higher returns. Our results provide evidence consistent with models of investor disagreement.

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[†] National Taiwan University; Center for Research in Econometric Theory and Applications, National Taiwan University. Email address: yenchengchang@ntu.edu.tw.

[§] National Taiwan University. Email address: f05723094@ntu.edu.tw.

[¶] Stockholm School of Economics, Swedish House of Finance, ABFER, IFN, and CEPR. Email address: alexander.ljungqvist@hhs.se.

[¥] National Taiwan University; Center for Research in Econometric Theory and Applications, National Taiwan University. Email address: kevintseng@ntu.edu.tw.

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Yen-Cheng Chang

The author declares that he has no relevant or material financial interests that relate to the research described in this paper.

Pei-Jie Hsiao

The author declares that she has no relevant or material financial interests that relate to the research described in this paper.

Alexander Ljungqvist

The author declares that he has no relevant or material financial interests that relate to the research described in this paper.

Kevin Tseng

The author declares that he has no relevant or material financial interests that relate to the research described in this paper.

Disagreement among investors is a key ingredient in models of financial market speculation and bubbles. In many such models, investors with identical information have heterogeneous priors and agree to disagree over their inferences. This intuitive design is used to reconcile elevated levels of trading in financial markets in the absence of news (Karpoff 1987, Varian 1989, Kandel and Pearson 1995). 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 stock price crash risk or overvaluation. 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

[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."

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.

There are two (not mutually exclusive) ways in which making a firm's SEC filings available via EDGAR may reduce disagreement among investors, without (as we show) being confounded by changes in firms' fundamentals or disclosure policies. First, disagreement could fall because information diffuses more rapidly (allowing investors to update at similar speed) or more broadly (as prices aggregate information from more investors), or because information is disclosed in a more homogeneous format (allowing for less noisy or error-prone comparison across time and across firms). Second, disagreement could fall as the behavior of a key information intermediary changes. Chang, Ljungqvist, and Tseng (2019) find that EDGAR inclusion leads to analysts behaving less strategically (in the sense of issuing less biased and more accurate earnings forecasts). They conclude that EDGAR inclusion reduces investors' verification costs and thereby constrains analysts' strategic behavior, leading to lower dispersion in analyst forecasts. To the extent that investors form their opinions at least in part based on information provided by analysts, less biased and more accurate analyst forecasts should reduce investor 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.

[2] Table 1 lists the 10 phase-in dates. As Chang, Ljungqvist, and Tseng (2019) note, the SEC assigned firms to waves randomly conditional on firm size.

Using a standard DD approach, we begin by comparing changes in investor disagreement among treated and control firms around EDGAR inclusion. We use two alternative measures of disagreement. The first set of measures is based on divergence of opinions as revealed in analyst earnings forecasts, measured either as the standard deviation of analysts' earnings forecasts (Diether, Malloy, and Scherbina 2002, Cheong and Thomas 2011) or the difference between the highest and lowest forecast (De Bondt and Forbes 1999), and in either case computed for either short-term or long-term earnings forecasts. For each of these four 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: forecast dispersion falls by between 7.5% and 25.3% from its pre-EDGAR mean, with dispersion in long-term forecasts falling more steeply. Our second measure of disagreement uses trading volume around earnings announcements (Kandel and Pearson 1995, Daniel, Hirshleifer, and Subrahmanyam 1998, Barber and Odean 2008). This measure too falls significantly after EDGAR inclusion, by 3.7% from its pre-EDGAR mean.

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, 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 next investigate its effects 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 key cross-sectional predictions of disagreement models. The first concerns short-sale constraints. Using triple-difference models and standard proxies for short-sale constraints, we find that crash risk decreases more following EDGAR inclusion the more binding a firm's short-sale constraints. The second comes from

[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.

Miller's (1977) model, which implies that investor optimism plays a key role in linking disagreement and asset prices. 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.

Finally, we explore return predictability. Assuming short-sale constraints, prices primarily reflect optimistic views, leading to a negative correlation between investor disagreement and subsequent stock returns. This prediction is first derived by Miller (1977) and confirmed in later empirical studies (notably, Diether, Malloy, and Scherbina 2002, Chen, Hong, and Stein 2002, Hong, Scheinkman, and Xiong 2006). Consistent with this argument, we show that firms exhibit higher returns after EDGAR inclusion than matched controls not yet included in EDGAR. This return differential is not due to standard factors such as size, book-to-market, or illiquidity. Using our shock-based IV, we find a robust negative relation between investor disagreement and returns, confirming Diether, Malloy, and Scherbina's cross-sectional finding.

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 (2019), 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]

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

We make three contributions to the literature. First, we exploit a randomly assigned shock to information access and information production and show that it leads to lower disagreement. Our results speak to the premise of disagreement models such as Varian (1989), Harris and Raviv (1993), and Kandel and Pearson (1995), namely that investors with heterogeneous priors agree to disagree even without accompanying fundamental news. Methodologically, our study provides a new test design that enables us to establish a plausibly causal link between investor disagreement and asset prices. We believe future research can benefit from this empirical framework.

Second, our paper contributes to the empirical literature exploring the determinants of stock price crash risk. Our evidence supports a causal link between disagreement and crash risk. While prior studies have shown a positive correlation between disagreement and crash risk in the cross-section, we view this literature as incomplete because, as Chen, Hong, and Stein (2001) note, endogeneity concerns have made it difficult to draw causal inferences. Our paper aims to fill this gap by exploiting a randomly assigned shock to investor disagreement.

Third, 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 so stabilizes markets. This finding should be of interest to securities regulators.

1. Empirical Strategy and Data

1.1 Institutional Background

Testing the empirical relevance of investor disagreement in asset pricing 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.[7] The SEC's announcement included a preliminary phase-in schedule, with registered firms joining EDGAR in ten waves over the three years starting April 26, 1993 and ending May 6, 1996. Firms in waves 5 through 10 did not know their EDGAR join dates until a few months before joining.[8]

As Chang, Ljungqvist, and Tseng (2019) note, electronic *filing* per se would not be expected to affect the 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.[9] 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.[10] 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.

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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.

[7] SEC Release No. 33-6977.

[8] The phase-in schedule included a six-month review, to begin after wave 4 on December 6, 1993. The review took longer than planned, leading to the suspension of waves 5 (preliminarily scheduled for August 1994) and 6 (preliminarily scheduled for November 1994). On December 19, 1994, the SEC announced the final rules on EDGAR implementation, revising the dates for waves 5 and 6 to January 1995 and March 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 December 1994 announcement. In doing so, we follow Chang, Ljungqvist, and Tseng (2019) but depart from Emery and Gulen (2019), Guo et al. (2019), and Gao and Huang (2020), all of which use the preliminary dates.

[9] The SEC's original plan was to allow public access to EDGAR only via dedicated terminals located in the SEC's three reference rooms.

[10] The SEC took over the task of hosting online access to EDGAR from NYU in October 1995.

1.2 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. Online access to corporate filings makes stock prices more informative (Gao and Huang 2020), which in and of itself should reduce disagreement, and reduces information asymmetries between investors and analysts (Chang, Ljungqvist, and Tseng 2019). Chang, Ljungqvist, and Tseng 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 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.[11] In support of their prediction, Chang, Ljungqvist, and Tseng show that analysts' earnings forecasts become less biased and more accurate after a firm's filings become available online and that these effects are economically and statistically stronger among those analysts with greater reason to behave strategically in the first place (such as affiliated analysts and those serving predominantly retail clients).

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 2019). 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

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

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 test for 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 3.

1.3 Sample and Data

1.3.1 Treated and Control Firms

We construct our samples of treated and control firms as follows. With one important exception, firms are treated from the fiscal quarter in which they are included in EDGAR. The exception concerns firms in phase-in waves 1 through 4, whose electronic EDGAR filings did not become publicly available *online* until January 17, 1994, and so are considered treated for our purposes only from that date onwards.[12]

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

[12] 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.

firms with CRSP share codes greater than 11 (foreign issuers, real estate investment trusts, master limited partnerships, and the like). We follow each treated firm for nine fiscal quarters centered on its EDGAR inclusion quarter.

Eventually, all SEC-registered firms are treated, as every issuer is obliged to file through EDGAR starting on May 6, 1996. Control firms are thus selected from the set of to-be-treated firms. Naturally, the last EDGAR wave lacks controls and – due to bunching towards the end of the SEC's phase-in schedule – so do waves 8 and 9. 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 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.

As Table 1 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.

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

1.3.2 Investor Disagreement Measures

Investor disagreement is not observed directly. To proxy for investor disagreement, we follow two 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 (2019) document post-EDGAR is a reduction in dispersion in analyst forecasts and, by this argument, in investor disagreement.

The literature operationalizes analyst forecast dispersion in two ways, using either the standard deviation of analyst earnings forecasts[14] or the difference between the highest and lowest forecast,[15] 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 for the current fiscal year. This gives us four analyst-based measures of investor disagreement. (Variable definitions and details of their construction can be found in Appendix A.)

The other 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

[14] Standard-deviation based measures of forecast dispersion are widely used to examine the effects of disagreement on both asset pricing and corporate finance issues. See Diether, Malloy, and Scherbina (2002), Sadka and Scherbina (2007), Moeller, Schlingemann, and Stulz (2007), Berkman et al. (2009), and Yu (2011), among others.
[15] See, for example, De Bondt and Forbes (1999).

are (among) the most important drivers of share prices and trading volume as investors process the information such announcements contain. Importantly, 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 a large number of alternative explanations, Kandel and Pearson propose a model in which investors agree to disagree in their interpretations of identical public signals.[16] 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.

Following Kandel and Pearson (1995), we measure trading volume over a three-day window around an earnings announcement. Comiskey et al. (1987) and Ziebart (1990) find that trading volume around earning announcements correlates positively with analyst forecast dispersion.

Table 2 reports summary statistics of our five disagreement measures, separately for treated and control firms and measured in either levels or changes as of the fiscal quarter before treatment. Treated and control firms have near-identical dispersion, range, and trading volume 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.

1.3.3 Stock Price Crash Risk Measures

To proxy for stock price crash risk, we follow the literature and use a total of five widely used measures. The first two, $NCSKEW$ and $DUVOL$, are based on the influential work of Chen, Hong, and Stein (2001). $NCSKEW$ is the negative coefficient of return skewness. A higher value of $NCSKEW$ corresponds to greater stock price crash risk. $DUVOL$ is "down-to-up volatility,"

[16] Other models that allow investors to agree to disagree include Harrison and Kreps (1978), Varian (1989), Kim and Verrecchia (1991a), Kim and Verrecchia (1991b), Romer (1993), Harris and Raviv (1993), Odean (1998), Hong and Stein (2003), Scheinkman and Xiong (2003), and Banerjee and Kremer (2010).

that is, the ratio of the return volatility during "down" days to the return volatility during "up" days. A higher value of $DUVOL$ corresponds to greater stock price crash risk. Our final three measures of crash risk follow Hutton, Marcus, and Tehranian (2009) and are based on 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. Higher values correspond to greater stock price crash risk. (See Appendix A for formal definitions.)

As the summary statistics in Table 2 show, treated and control firms have very similar levels of $NCSKEW$, $DUVOL$, and $CRASHx$ in the fiscal quarter before treatment.[17] 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.

1.3.4 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 share price jumps, evaluated at the 0.01%, 0.1%, and 1% levels. We refer to these measures as $JUMP001$, $JUMP01$, and $JUMP1$. They are constructed analogously to $CRASH001$, $CRASH01$, and $CRASH1$, except that they capture the incidence of extreme positive returns. Table 2 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.

[17] 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.

1.3.5 Control Variables

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

The second issue is that some of our variables of interest – namely, those based on analyst forecasts – are known to exhibit seasonalities over the course of the fiscal year. Earnings forecasts tend to become more accurate the later in a firm's fiscal year they are made (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. 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.

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

2. Investor Disagreement and Information Access

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

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

where $DISAGREEMENT_{it}$ for firm i in fiscal quarter t is measured using either dispersion or range of forecasts or trading volume around earnings announcements; $SHOCK_{it}$ and $POSTSHOCK_{it}$ are indicator variables that equal one if firm i is included in EDGAR in quarter t and $t-1$ to $t-4$, respectively; $\mathbf{X}_{it-1}$ is a vector of control variables summarized in Section 1.3.5; and c_i, c_q, and c_f are firm, time, and fiscal-quarter fixed effects, respectively. Standard errors are clustered at the firm level, given that we exploit a firm-level shock.

Table 3 reports the results. The effect of EDGAR inclusion is uniformly negative across our five disagreement measures. It is statistically significantly negative in the treatment quarter for three of our five disagreement measures (dispersion in fiscal-year forecasts and the range of quarterly and fiscal-year forecasts) and consistently statistically significantly negative for all five measures in the four quarters following treatment.[18]

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

[18] Chang, Ljungqvist, and Tseng (2019) 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.

inclusion reduces 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. Assuming, as the literature does, that analyst forecast dispersion is a reasonable proxy 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.

For trading volume, we also find negative (albeit economically smaller) treatment effects. Column 5 shows that trading volume in the three days around earnings announcements declines on average by half a percent in the treatment quarter (p=0.691) and by 3.7% over the next four quarters (p=0.001), 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).[19] The reduction in trading intensity we find following EDGAR inclusion is thus strong evidence of 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 announcement as a proxy for news, column 6 shows that trading volume decreases following EDGAR inclusion even in the absence of news (i.e., when absolute announcement returns are zero) and that the effect of trading volume is not significantly related to the size of the returns.

Table 3 also reports formal tests of diverging pre-trends between treated and controls. These confirm the absence of pre-treatment effects for all five of our disagreement measures, as required for the internal validity of our DD approach, with the possible exception of the range of fiscal-year forecasts in column 4 (p=0.102). Figure 3 plots dynamic DD estimates of the effects of inclusion in EDGAR on each of the five disagreement measures over the nine-quarter window

[19] 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).

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 $t-1$.

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 five measures of investor disagreement on EDGAR inclusion. This generates two important insights. First, disagreement is reduced post-EDGAR inclusion regardless of the initial level of disagreement: the estimated treatment effects are significantly negative across all deciles for each of our five measures. Second, the slope is negative across deciles, meaning that the reduction 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 a much flatter slope for the trading-volume measure.

Overall, the results in Table 3 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. We next investigate what happens to stock price crash risk.

3. 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 – i.e., 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.[20]

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 as measured using $NCSKEW$ and $DUVOL$. 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.

3.1 Difference-in Differences Results

The starting point of our investigation of stock price crash risk is a DD regression of the following general form:

$$CRASH\ RISK_{it} = \alpha + \delta_1 SHOCK_{it} + \delta_2 POSTSHOCK_{it} + \alpha\mathbf{X}_{it-1} + c_i + c_q + c_f + \xi_{it}, \tag{2}$$

where $CRASH\ RISK_{it}$ is measured using one of our five proxies introduced in Section 1.3.3 and all right-hand-side variables are the same as in equation (1).

Table 4 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 0.04 (p=0.048) 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 36.7% reduction relative to the sample mean of $NCSKEW$ reported in Table 2.[21] Column 2 shows that $DUVOL$ falls by an average of 0.026 (p=0.041) following

[20] Related models of crash risk outside the stock market include Geanakoplos (2010), who studies loans, and Fostel and Geanakoplos (2012), who study credit default swaps.
[21] 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

EDGAR inclusion, equivalent to a 38.2% reduction from $DUVOL$'s sample mean. The incidence of extreme negative returns ($CRASHx$) falls by 31.6%, 22.6%, and 6.1% from the corresponding mean, for returns in the 0.01%, 0.1%, and 1% left tail (each highly statistically significant).

Table 4 also reports formal tests of diverging pre-trends. These confirm the absence of pre-treatment effects for all five of our crash risk measures, as required for the internal validity of our DD approach, with the possible exception of $DUVOL$ (p=0.095).

Figure 5 investigates how the effect of EDGAR inclusion on crash risk varies with the level of pre-treatment crash risk. For obvious reasons, we focus on our two continuous crash measures, $NCSKEW$ and $DUVOL$. As in Figure 4, we graph point estimates and 95% bootstrapped confidence intervals obtained from quantile DD regressions. This provides important nuance to the findings shown in Table 4: the effect of EDGAR inclusion on crash risk as measured by $NCSKEW$ and $DUVOL$, while negative across all deciles, is only statistically significant for firms in the upper half of the distribution. The slope across deciles is negative, implying that the reduction in crash risk is larger the larger its initial level. Overall, Figure 5 suggests that the EDGAR-induced reduction in average crash risk reported in Table 4 is concentrated among firms with the highest initial crash risk levels.

Given quasi-random assignment, staggered implementation, and the absence of diverging pre-trends, the findings in Table 4 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.

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.

3.2 Two-stage Least Squares Results

To test whether 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.[22] The DD specifications discussed in Section 2 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 3.4. Based on models of crash risk such as Hong and Stein (2003) and others, we expect a positive coefficient for investor disagreement in our 2SLS model: higher disagreement leads to higher crash risk and (in our context) vice versa.

Table 5 reports the 2SLS regression results of the impact of investor disagreement on stock price crash risk. The way the table presents the estimates deserves comment for being somewhat unusual. Recall from Table 3 that we use five measures of investor disagreement, and recall from Table 4 that we use five measures of crash risk. We thus estimate $5 \times 5 = 25$ regressions. Table 5 summarizes the results of these 25 regressions by reporting, in matrix form, only the 25 investor-disagreement coefficients (along with heteroskedasticity consistent standard errors clustered at the firm level), the 25 weak-instrument tests, and the 25 observations counts. Each of the 25 "cells" in the upper half of Table 5 thus represents a separate regression.

Each of the 25 2SLS coefficients is positive, as predicted, and 20 of them are statistically significant at conventional levels.[23] The (relatively) weakest results come from the specifications using the dispersion or range of quarter-ahead forecasts to proxy for disagreement, whereas the

[22] OLS will be biased in the presence of measurement error, simultaneity or reverse causality, and 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 3.4, could correlate with disagreement. A valid instrument removes these biases, as long as it is statistically strong (which ours is).

[23] They are larger than the corresponding OLS estimates shown in Table IA.1 in the Internet Appendix, confirming that OLS yields downward biased estimates in our setting.

measures based on fiscal-year forecasts 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 25 specifications, with F-statistics well above 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 5 are consistent with Hong and Stein's (2003) model of stock price 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 five disagreement measures from their respective pre-treatment mean. The estimated elasticities for the forecast-based measures vary between 1.1 and 4.4, except in the case of $CRASH1$ where the elasticities vary between 0.3 and 0.6.[24] The estimated elasticities for the trading-volume measure are considerably larger, varying between 2.0 (for $CRASH1$) and 14.1 (for $NCSKEW$).

Table IA.2 in the Internet Appendix explores the robustness of these findings using two less commonly used measures of crash risk: Jin and Myers' (2006) $COUNT$ and $COLLAR$ measures, in either case evaluated at the $x = 0.01\%, 0.1\%$, or 1% levels.[25] We find a positive effect of disagreement on crash risk in each of the $5 \times 6 = 30$ regressions, and statistically significant effects in 20 of them.

Overall, our 2SLS estimates in Tables 5 and IA.2 lend support to models in the crash-risk literature that link crash risk to investor disagreement.

3.3 Asymmetry

Models of the effect of investor disagreement on stock price crash risk predict an asymmetric

[24] 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.

[25] $COUNTx$ 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.

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 1.3.4, we use Hutton, Marcus, and Tehranian's (2009) $JUMPx$ measures for this test. Table 6 reports the $3 \times 5 = 15$ 2SLS estimates. Each of the 15 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.

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).

3.4 Alternative Channel: Bad-News Hoarding

A causal interpretation of our 2SLS estimates in Table 5 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 among investors. Instead, managers control how transparent the firm is and have incentives to stockpile bad news, say by withholding information or by managing their earnings.[26] A sudden release of bad news, perhaps once a tipping point 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

[26] The accounting literature has long recognized managers' tendency to withhold bad news (Graham, Harvey, and Rajgopal 2005, Kothari, Shu, and Wysocki 2009, Ball 2009). Disappointing earnings news can adversely affect managers' career prospects or compensation (Verrecchia 2001, Hermalin and Weisbach 2007).

measures of discretionary accruals, the tendency for reported earnings to narrowly "meet or beat" analyst consensus, earnings restatements, and breaks in streaks of earnings increases. We find no evidence suggesting that managers vary what news they release or how transparent their financial reporting is.

Table 7 reports the results. Column 1 shows that return on assets is no different after EDGAR inclusion than before. 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 (measured either as discretionary accruals obtained from a modified Jones model or as performance-matched discretionary accruals) is unchanged around EDGAR inclusion. In other words, firms do not manage their earnings less aggressively after joining EDGAR. Column 4 considers an alternative measure of the transparency of financial reporting: the tendency for a firm's reported earnings to narrowly meet-or-beat analyst consensus forecasts.[27] 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.[28]

The findings in Table 7 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

[27] 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.

[28] 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 3.1), negative stock returns are clearly endogenous, and as we will show in Section 4, 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 problems – suggest that firms do not change how they manage their earnings.

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 2019).

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 4, 5, 6, IA.2, and Figure 5 are consistent with disagreement-based models while the results in Table 7 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.[29]

3.5 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 Section 3.1, 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.

3.5.1 The Tightness of Short-sale Constraints

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 that the effect of disagreement on stock price

[29] Chang, Chen, and Zolotoy (2017) show that more liquid firms have higher stock price crash risk. The mechanism they have in mind is that greater liquidity attracts transient investors such as hedge funds (Porter 1992, Fang, Tian, and Tice 2014), whose presence may put pressure on managers to hoard bad news. Since Chang, Ljungqvist, and Tseng (2019) find that EDGAR inclusion leads to improved liquidity, we would expect, based on Chang, Chen, and Zolotoy's reasoning, that crash risk should increase. In fact, we find the opposite. This does not disprove Chang, Chen, and Zolotoy's mechanism, but to the extent that their mechanism is at work in our setting, our estimates of the effects of disagreement on crash risk would be conservative.

crash risk is stronger when short-sale constraints are tighter, and vice versa.

Short-sale constraints are notoriously difficult to measure, and especially so in the early 1990s.[30] We use three alternative proxies: beta, institutional ownership, and membership in the S&P500 index. 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. The extent of disagreement is naturally larger for high-beta stocks than for low-beta stocks, because 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.

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). To avoid capturing firm size effects by using raw institutional ownership (IO for short), we follow Nagel (2005) and measure residual IO by running quarterly cross-sectional regressions of logit transformed IO on firm size. We predict that firms with high residual IO (those in the top three deciles) experience a smaller reduction in crash risk after joining EDGAR. Similarly, firms in the S&P500 index have lower short-sale constraints to the extent that they have higher institutional ownership and so should experience a smaller reduction in crash risk.[31]

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 8, 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

[30] Markit's database of stock lending fees starts in July 2006. OptionMetrics' database, which Muravyev, Pearson, and Pollet (2020) use to estimate option-implied lending fees, starts in 1996, after the EDGAR roll-out.

[31] Other proxies for short-sale constraints sometimes used in the literature are ambiguous or not suited to our setting. Short interest is an equilibrium quantity and thus uninformative about any potential *excess* demand to short a stock. 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.

[32] Even though Table 8 only reports the effects of interest ($treated \times post$ and $treated \times post \times beta$), our triple-diff specifications include all necessary interactions. Note that because $beta$ is time-invariant (it is measured as of

(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=0.004), $DUVOL$ (p=0.013), and $CRASH01$ (p=0.098).

Table 8, Panels B and C report similar findings for institutional ownership and S&P500 index firms. 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. The results for S&P500 index firms in Panel C are similar but more noisily estimated (likely because our sample tilts towards smaller, non-S&P500 firms).

Assuming that beta, institutional ownership, and S&P500 index membership 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.

3.5.2 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

the pre-treatment quarter), both $beta$ and $treated \times beta$ are collinear with the firm fixed effects and so drop out of the estimation.

that optimism is primarily related to future growth rather than assets in place (Lakonishok, Shleifer, and Vishny 1994, La Porta 1996, Diether, Malloy, and Scherbina 2002). We thus expect a larger (more negative) treatment effect the higher a firm's PVGO index.

Table 8, Panel D 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 Miller's (1977) prediction that the effect of investor disagreement on stock price crash risk increases in the marginal investor's optimism.

4. Investor Disagreement, Stock Prices, and Return Predictability

In this section, we turn our focus to the effect of EDGAR inclusion on expected returns. There is currently no unifying disagreement model that combines crash risk and expected returns.[33] The analysis that follows therefore focuses on the effect of disagreement on returns, without regard to crash risk. The workhorse model in the literature is Miller (1977). Miller shows that in a market with short-sale constraints, the prices of stocks with high investor disagreement primarily reflect the valuations of optimists, leading to low subsequent returns. Supporting this hypothesis, 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. In this section, we revisit this evidence using our conditionally randomly assigned shock to investor disagreement.

The price dynamics we expect are as follows. Pre-EDGAR, treated and control firms have similar levels of disagreement pre-treatment (see Table 2). When a firm joins EDGAR, investor

[33] In Hong and Stein (2003), arbitrageurs are risk-neutral and so expected returns are zero.

disagreement about its prospects falls (see Table 3). Thus, 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. Translated into our quarterly DD setup, we thus expect a positive DD coefficient for the four quarters after joining EDGAR. Whether the DD coefficient in the quarter a firm joins EDGAR is negative or positive depends on how quickly the fall in disagreement resolves itself. Slow resolution implies a negative coefficient. Fast resolution implies a positive coefficient.

Table 9, Panel A reports DD estimates for two measures of quarterly stock returns: raw returns (R_{raw}) and market-adjusted returns (R_e). In either case, we find that returns are higher post-treatment, controlling for standard cross-sectional risk factors such as size, book-to-market, and liquidity. Specifically, raw returns are on average 3.0 percentage points higher in the fiscal quarter of EDGAR inclusion (p=0.001), suggesting fast resolution, and remain 2.2 percentage points higher over the next four quarters (p=0.017), consistent with Miller (1977). Market-adjusted returns are on average 2.2 percentage points higher in the quarter of EDGAR inclusion (p=0.004) and remain 2.1 percentage points higher over the next four quarters (p=0.014).

We next use our 2SLS setup to estimate the effect of investor disagreement on stock returns. The results are reported in Table 9, Panel B. With five disagreement measures and two measures of stock returns, we report the $5 \times 2 = 10$ coefficients in compact matrix form. In all ten specifications, we find the expected negative effect, confirming that returns are lower the higher is disagreement among investors. Eight of the ten coefficients are statistically significant. The exceptions are the two coefficients for trading volume around earnings announcement.

We view the 2SLS coefficients in Table 9, Panel B 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.

Finally, we estimate standard calendar-time portfolio alphas to test for return predictability. 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 10 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. These patterns mirror our DD and IV findings. 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.

5. Conclusions

We investigate the role played by investor disagreement in asset pricing, with particular focus on stock price crash risk and return predictability. 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. 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. Finally, we show that subsequent returns are higher, for a time, when disagreement has decreased.

Our findings support the premise of disagreement models such as Varian (1989), Harris and Raviv (1993), and Kandel and Pearson (1995), namely that investors have heterogeneous priors that cause them to "agree to disagree" when faced with the same signal. Our findings further support the predictions of models that link crash risk to disagreement, such as Hong and Stein (2003), and are consistent with models of overvaluation, such as Miller (1977).

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 A: 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). 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). 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). 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). I/B/E/S data are obtained from the unadjusted summary history files. See De Bondt and Forbes (1999) for further details.

Trading volume 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.

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 $R_{it} = \log(1+\varepsilon_{it})$. ε_{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 $R_{it} = \log(1+\varepsilon_{it})$. ε_{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 $R_{it} = \log(1+\varepsilon_{it})$. ε_{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 $R_{it} = \log(1 + \varepsilon_{it})$. ε_{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 $R_{it} = \log(1 + \varepsilon_{it})$. ε_{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 $R_{it} = \log(1 + \varepsilon_{it})$. ε_{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 $R_{it} = \log(1 + \varepsilon_{it})$. ε_{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 $R_{it} = \log(1 + \varepsilon_{it})$. ε_{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.

Return measures

$\boldsymbol{R_{raw}}$ is firm i's return in fiscal quarter t, defined as the firm's compounded return using monthly stock returns (CRSP variable ret).

$\boldsymbol{R_e}$ is firm i's market-adjusted return in fiscal quarter t, defined as the compounded difference between the firm's daily return (CRSP variable ret) and the CRSP value-weighted market index return (CRSP variable $vwretd$).

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.

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), ΔREV_{iq} is the change in quarterly revenue (Compustat variable $saleq$), and PPE_{iq} is gross property, plant, and equipment (Compustat variable $ppegtq$). Jones discretionary accruals is defined as $DA_{iq} = \left(TA_{iq}/ASSET_{iq-1}\right) - NA_{iq}$, where $NA_{iq} = \widehat{\beta_0} + \widehat{\beta_1} + 1/ASSET_{iq-1} + \widehat{\beta_2}\left(\Delta REV_{iq} - \Delta AR_{iq}\right)/ASSET_{iq-1} + \widehat{\beta_3}\, PPE_{iq}/ASSET_{iq-1}$ and AR_{iq} is accounts receivable (Compustat variable $rectq$).

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 streaks 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).

$\boldsymbol{Beta}$ 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.

$\boldsymbol{IO}$ is an indicator variable set equal to one if firm i's residual institutional ownership is in the top three 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.

$\boldsymbol{S\&P500\ index}$ is an indicator variable set equal to one if firm i is an index member of the S&P 500 in fiscal quarter t. S&P 500 index membership data are taken from Compustat's Index Constituents database.

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.

$\boldsymbol{B/M}$ is firm i's book value of equity (Compustat variable $ceqq$) divided by its market value of equity (Compustat variable $prccq$ times Compustat variable $cschoq$) as of the last trading day of fiscal quarter t.

$\boldsymbol{AIM}$ equals the natural log of one plus Amihud's (2002) illiquidity measure. For each fiscal quarter, we calculate the daily ratio of absolute return to dollar-valued trading volume, $[1{,}000{,}000 \times |ret|/(|prc| \times vol)]$. We then average the daily values over the fiscal quarter, add 1, and take logs. For Nasdaq-traded stocks, trading volume is adjusted using the Gao and Ritter (2010) procedure.

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.

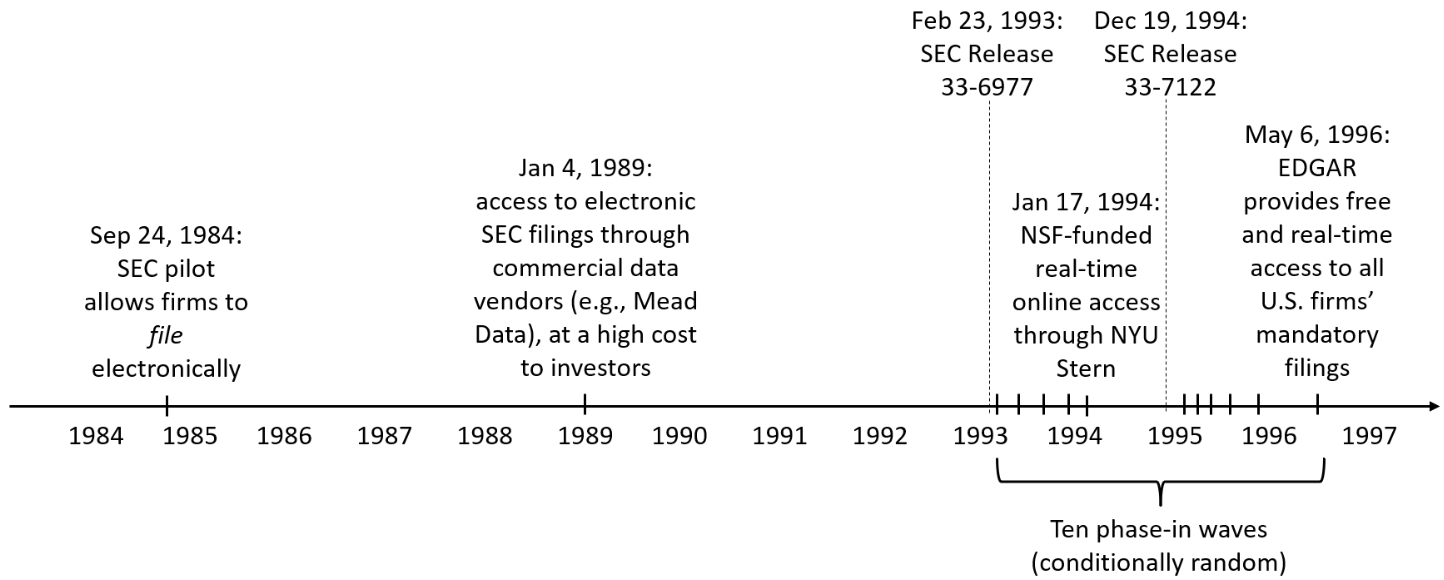

Figure 2. EDGAR Phase-in Waves.
The figure shows the average equity market capitalization of firms included in each of the ten EDGAR phase-in waves. See Table 1 for further details.

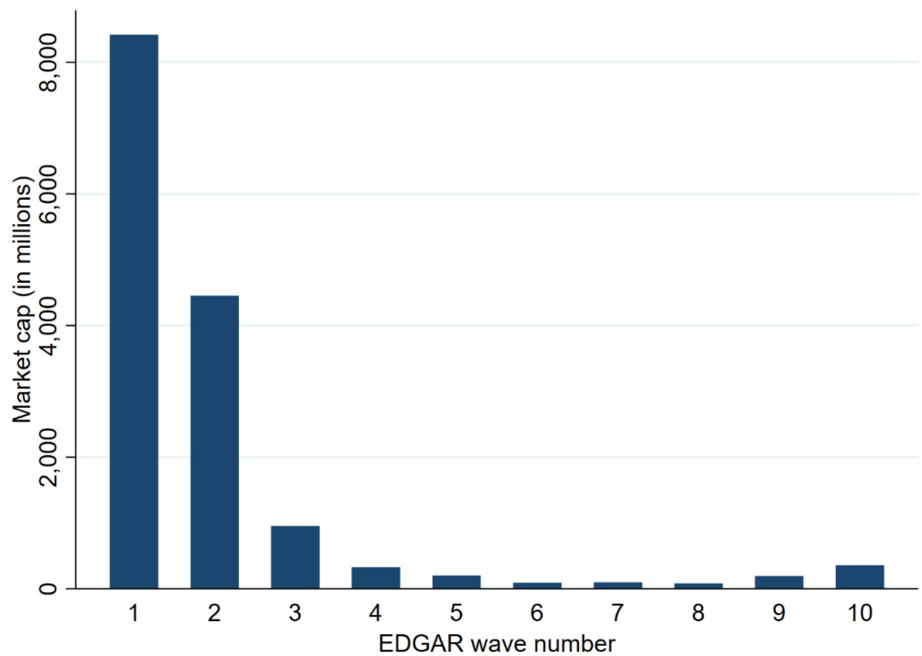

Figure 3. Dynamics of the Treatment Effect.

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 Appendix A.

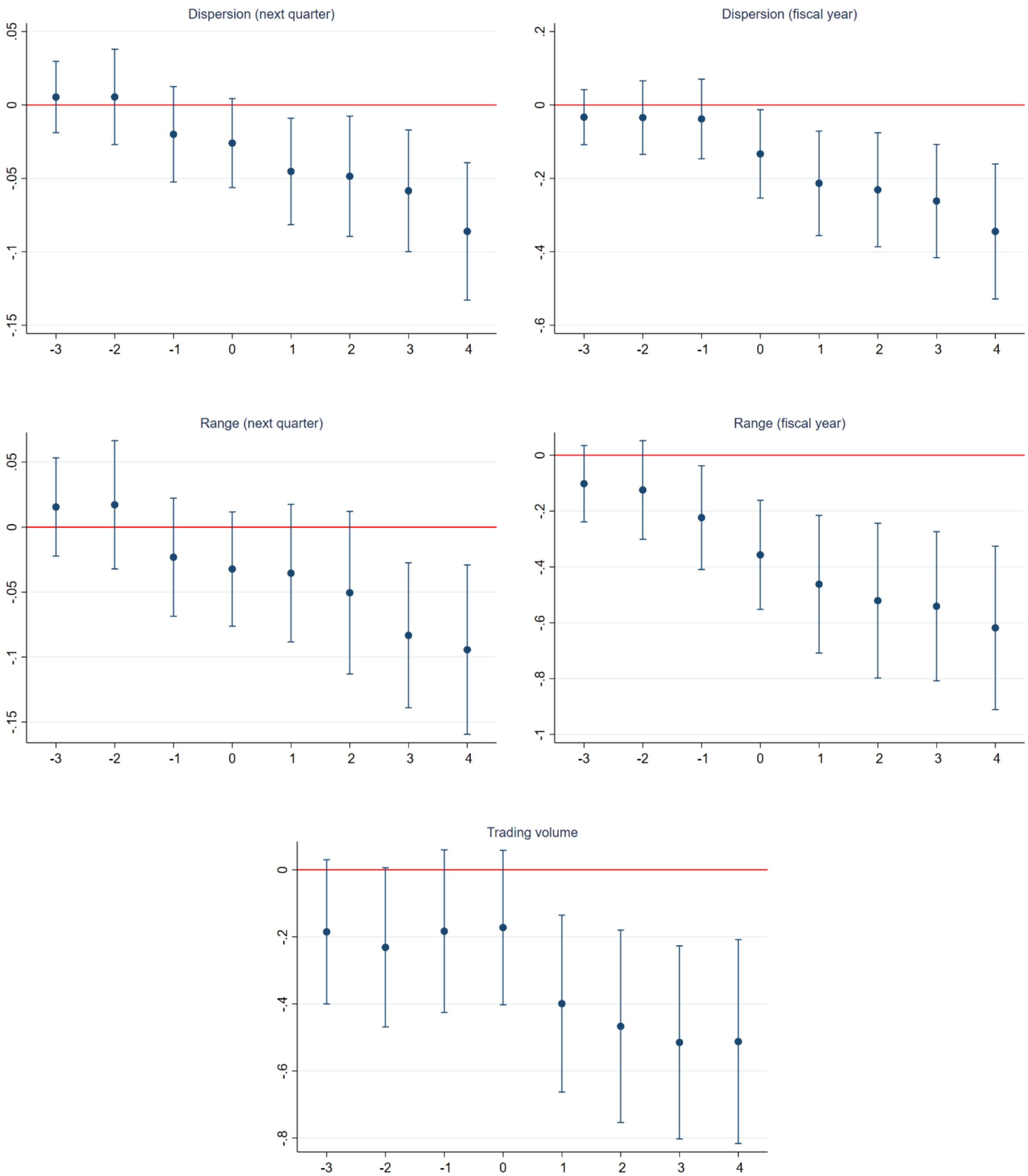

Figure 4. Quantile Regressions: Disagreement Measures.
The figure graphs quantile-regression DD estimates of the effects of inclusion in EDGAR on investor disagreement. Treated firms are those included in EDGAR at time 0; control firms are nearest-neighbor propensity-scored matched on equity market capitalization (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 Appendix A.

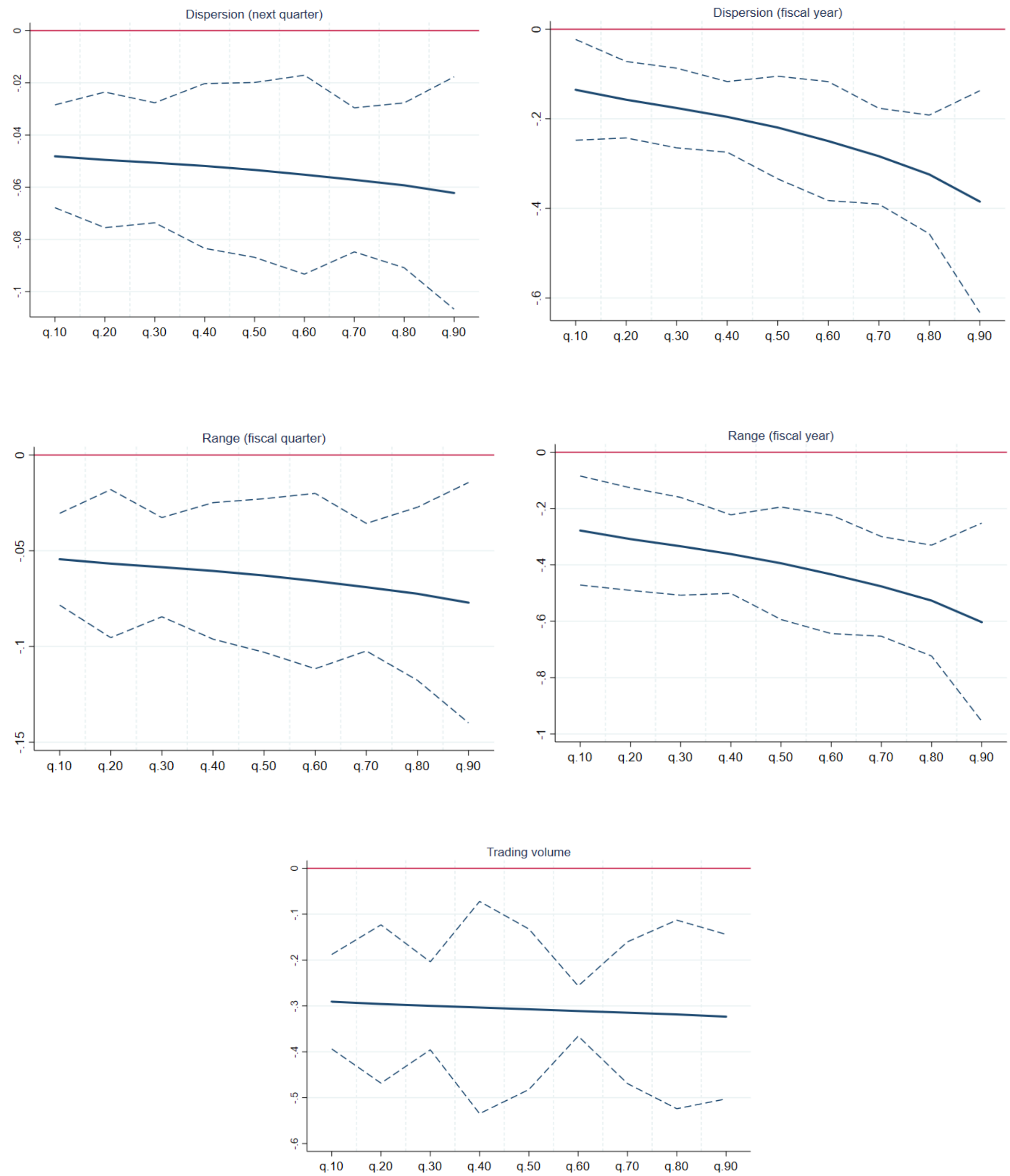

Figure 5. Quantile Regressions: Stock Price Crash Risk Measures.
The figure graphs quantile-regression DD estimates of the effects of inclusion in EDGAR on stock price crash risk for our two continuous crash measures. Treated firms are those included in EDGAR at time 0; control firms are nearest-neighbor 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 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 Appendix A.

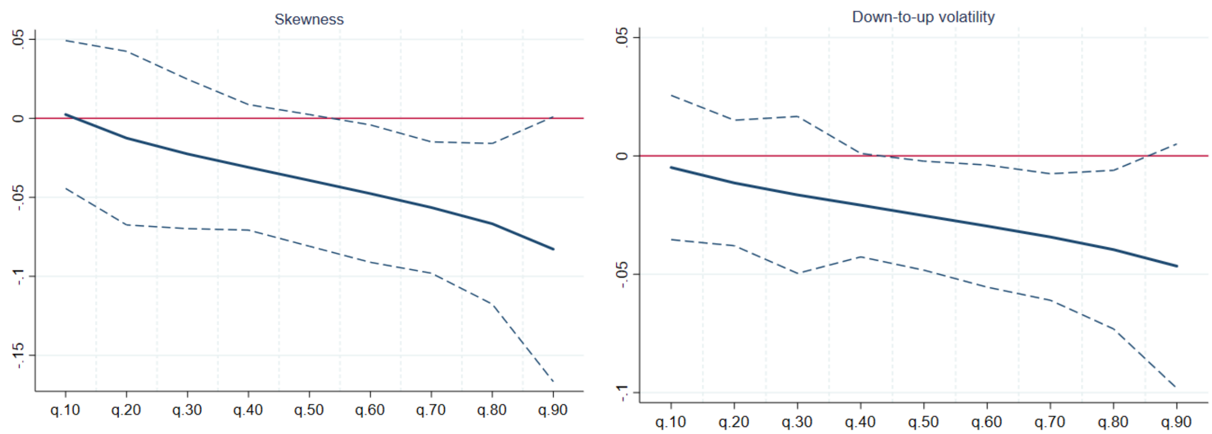

Table 1. EDGAR Phase-in Waves.

The table provides a breakdown of the universe of listed U.S. firms and of the sample of treated firms by EDGAR phase-in wave. Listed U.S. firms are those listed on the NYSE, NASDAQ, or AMEX with CRSP share codes of 10 or 11. Treated firms 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.

			All listed U.S. firms		All listed U.S. firms (excluding financials and utilities)		Treated firms	
Phase-in wave no.	SEC designation	Phase-in date	No. of firms	Mean market cap ($m)	No. of firms	Mean market cap ($m)	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 2. 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 Appendix A. 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 firms			Control firms			Treated firms			Control firms			Treated - Controls	
	# obs.	Mean	Std. dev.	# obs.	Mean	Std. dev.	# obs.	Mean	Std. dev.	# obs.	Mean	Std. dev.	Diff-erence	*t*-stat
Disagreement measures:														
Dispersion (next quarter)	452	0.003	0.004	583	0.002	0.004	399	0.000	0.003	517	0.000	0.003	0.000	-1.196
Dispersion (fiscal year)	794	0.009	0.015	948	0.009	0.016	762	0.000	0.009	900	0.000	0.012	0.000	-0.502
Range (next quarter)	452	0.004	0.004	583	0.003	0.004	399	0.000	0.004	517	0.000	0.003	0.000	-1.942
Range (fiscal year)	794	0.018	0.026	948	0.017	0.029	762	-0.002	0.018	900	0.000	0.020	-0.001	-1.455
Trading volume	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% ($CRASH001$)	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% ($CRASH01$)	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% ($CRASH1$)	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% ($JUMP001$)	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% ($JUMP01$)	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% ($JUMP1$)	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

Table 3. 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 five 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, 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 an alternative dynamic DD specification are jointly zero. For variable definitions and details of their construction see Appendix A. Heteroskedasticity consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level respectively.

	Dispersion (next quarter) (1)	Dispersion (year-ahead) (2)	Range (next quarter) (3)	Range (year-ahead) (4)	Trading volume (5)	Trading volume (6)
Quarter of EDGAR inclusion	-0.020	-0.099**	-0.030*	-0.242***	-0.038	-0.108
	0.012	*0.050*	*0.017*	*0.079*	*0.095*	*0.119*
$\times$ *absolute abnormal return*						0.022
						0.015
Next four quarters	-0.054***	-0.228***	-0.064***	-0.406***	-0.307***	-0.255**
	0.015	*0.058*	*0.020*	*0.101*	*0.093*	*0.106*
$\times$ *absolute abnormal return*						-0.005
						0.009
absolute abnormal return						-0.004
						0.005
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	67.2%	66.6%	64.3%	69.7%	75.9%	76.4%
Pre-trends (p-value)	0.333	0.825	0.258	0.102	0.224	0.317
No. of firms	1,582	2,059	1,582	2,059	3,235	3,235
No. of firm-quarters	9,237	15,141	9,237	15,141	23,099	22,126

Table 4. 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 ($CRASH001$, $CRASH01$, and $CRASH1$). 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 an alternative dynamic DD specification are jointly zero. For variable definitions and details of their construction see Appendix A. Heteroskedasticity consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level respectively.

	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)
Quarter of EDGAR inclusion	0.011	0.003	-0.002	-0.003	-0.009
	0.022	*0.014*	*0.008*	*0.012*	*0.016*
Next four quarters	-0.040**	-0.026**	-0.025***	-0.038***	-0.033**
	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	17.0%	18.7%	15.5%	15.5%	13.7%
Pre-trends (p-value)	0.425	0.095	0.918	0.771	0.374
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 5. 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 3, we use five measures of investor disagreement; as in Table 4, we use five measures of crash risk. The table summarizes these $5 \times 5 = 25$ regressions by reporting, in matrix form, only the 25 investor-disagreement coefficients (along with heteroskedasticity consistent standard errors clustered at the firm level), the 25 weak-instrument tests, and the 25 observations counts. Each of the 25 "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 3, columns 1 through 5 report the corresponding first-stage results. For variable definitions and details of their construction see Appendix A. 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)	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*
Trading volume	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
Trading volume	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
Trading volume	22,789	22,789	22,789	22,789	22,789

Table 6. 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 five measures of investor disagreement (see Table 3), we estimate $5 \times 3 = 15$ regressions. The table summarizes these 15 regressions by reporting, in matrix form, only the 15 investor-disagreement coefficients (along with heteroskedasticity consistent standard errors clustered at the firm level), the 15 weak-instrument tests, and the 15 observations counts. Each of the 15 "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 3 reports the corresponding first-stage results. For variable definitions and details of their construction see Appendix A. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level respectively.

	Jump measure		
	Extreme positive returns, 0.01% ($JUMP001$) (1)	Extreme positive returns, 0.1% ($JUMP01$) (2)	Extreme positive returns, 1% ($JUMP1$) (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*
Trading volume	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
Trading volume	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
Trading volume	22,789	22,789	22,789

Table 7. 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 streaks 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 an alternative dynamic DD specification are jointly zero. For variable definitions and details of their construction see Appendix A. Heteroskedasticity consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level respectively.

	Return on assets (1)	Jones discretionary accruals (2)	Performance-matched discretionary accruals (3)	Meet-or-beat (4)	Earnings restatements (5)	Breaks in streaks 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 8. 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 4. In Panels A through C, we interact treatment with three measures of how tightly binding a firm's short-sale constraints are: the firm's pre-treatment CAPM beta (Panel A), its residual institutional ownership (Panel B), and membership in the S&P500 index (Panel C). Short-sale constraints are predicted to increase in beta and decrease in institutional ownership (IO) and S&P500 index membership. In Panel D, 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 and S&P500 index membership are measured as of three quarters before treatment. 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 Appendix A. Heteroskedasticity consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level respectively.

	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)
Panel A: Pre-treatment beta					
$treated \times post$	0.027	0.011	-0.014	-0.015	-0.023
	0.027	*0.017*	*0.009*	*0.014*	*0.020*
$treated \times post \times beta$	-0.075***	-0.041**	-0.007	-0.022*	-0.012
	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	17.0%	18.7%	15.5%	15.5%	13.7%
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					
$treated \times post$	-0.100**	-0.063**	-0.059***	-0.049**	-0.093***
	0.041	*0.026*	*0.014*	*0.022*	*0.030*
$treated \times post \times IO$	0.102**	0.063**	0.050***	0.023	0.085**
	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	16.8%	18.5%	15.4%	15.4%	13.5%
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 8. Continued.

	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)
Panel C: Pre-treatment S&P500					
$treated \times post$	-0.025	-0.017	-0.020***	-0.030***	-0.027**
	0.019	*0.012*	*0.007*	*0.010*	*0.013*
$treated \times post \times S\&P500\ index$	0.352*	0.172	0.091	0.119*	-0.096
	0.191	*0.126*	*0.065*	*0.071*	*0.123*
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	16.8%	18.6%	15.4%	15.4%	13.6%
No. of firms	3,288	3,288	3,288	3,288	3,288
No. of firm-quarters	28,236	28,236	28,236	28,236	28,236
Panel D: Pre-treatment PVGO index					
$treated \times post$	0.062*	0.034	0.002	-0.013	0.013
	0.034	*0.022*	*0.012*	*0.018*	*0.025*
$treated \times post \times PVGO\ index$	-0.138***	-0.082***	-0.034*	-0.027	-0.066*
	0.050	*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	16.9%	18.6%	15.6%	15.6%	13.7%
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

Table 9. Investor Disagreement, Stock Prices, and Return Predictability: DD and IV Tests.
Panel A reports difference-in-differences estimates of the effects of inclusion in EDGAR on two measures of quarterly stock price returns: R_{raw}, a firm's raw stock return, and R_e, its market-adjusted stock return. 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. Panel B reports 2SLS regression results of the impact of investor disagreement on the same two measures of stock price returns. As in Table 5, we summarize the $5 \times 2 = 10$ regressions by reporting, in matrix form, only the 10 investor-disagreement coefficients (along with heteroskedasticity consistent standard errors clustered at the firm level). To conserve space, we suppress the 10 weak-instrument tests and observations counts, which are identical to those shown in Table 5. Table 3, columns 1 through 5 report the corresponding first-stage results. All specifications in both panels include controls (the one-quarter lags of log market cap, book/market B/M, and Amihud's (2002) illiquidity measure AIM, as well as fixed effects for calendar-quarter, fiscal-quarter, and firm). For variable definitions and details of their construction see Appendix A. Heteroskedasticity consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level respectively.

	R_{raw} (1)	R_e (2)
Panel A: DD estimates		
Quarter of EDGAR inclusion	0.030***	0.022***
	0.008	*0.008*
Next four quarters	0.022**	0.021**
	0.009	*0.008*
Controls?	yes	yes
Calendar quarter FE?	yes	yes
Fiscal quarter FE?	yes	yes
R-squared	27.9%	25.7%
No. of firms	3,343	3,343
No. of observations	27,410	27,413
Panel B: IV estimates		
Disagreement measure:		
Dispersion (quarter-ahead)	-0.647*	-0.655*
	0.344	*0.336*
Dispersion (year-ahead)	-0.096**	-0.107**
	0.048	*0.048*
Range (quarter-ahead)	-0.629*	-0.636*
	0.368	*0.363*
Range (year-ahead)	-0.062**	-0.069**
	0.032	*0.032*
Trading volume	-0.040	-0.045
	0.031	*0.031*

Table 10. 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)***

INTERNET APPENDIX

for

Testing Disagreement Models *

Yen-Cheng Chang† Pei-Jie Hsiao§ Alexander Ljungqvist¶
Kevin Tseng¥

(NOT INTENDED FOR PUBLICATION)

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† National Taiwan University; Center for Research in Econometric Theory and Applications, National Taiwan University. Email address: yenchengchang@ntu.edu.tw.
§ National Taiwan University. Email address: f05723094@ntu.edu.tw.
¶ Stockholm School of Economics, Swedish House of Finance, ABFER, and CEPR. Email address: alexander.ljungqvist@hhs.se.
¥ National Taiwan University; Center for Research in Econometric Theory and Applications, National Taiwan University. Email address: kevintseng@ntu.edu.tw.

Table IA.1. 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 3, we use five measures of investor disagreement; as in Table 4, we use five measures of crash risk. The table summarizes these $5 \times 5 = 25$ regressions by reporting, in matrix form, only the 25 investor-disagreement coefficients (along with heteroskedasticity consistent standard errors clustered at the firm level). Each of the 25 "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 Appendix A. 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*
Trading volume	-0.002	-0.005***	0.008***	0.009***	0.003*
	0.003	*0.001*	*0.001*	*0.001*	*0.002*

Table IA.2. 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 five measures of investor disagreement (see Table 3), we estimate $5 \times 6 = 30$ regressions. The table summarizes these 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 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 3, columns 1 through 5 report the corresponding first-stage results. For variable definitions and details of their construction see Appendix A. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level respectively.

Table IA.2. Continued.

	Crash measure					
	Extreme negative returns net of extreme positive returns, 0.01% ($COUNT001$) (1)	Extreme negative returns net of extreme positive returns, 0.1% ($COUNT01$) (2)	Extreme negative returns net of extreme positive returns, 1% ($COUNT1$) (3)	Put-call profit, 0.01% ($COLLAR001$) (4)	Put-call profit, 0.1% ($COLLAR01$) (5)	Put-call profit, 1% ($COLLAR1$) (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*
Trading volume	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
Trading volume	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
Trading volume	22,789	22,789	22,789	22,789	22,789	22,789