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Publicizing Arbitrage

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Abstract

How does greater public disclosure of arbitrage activity and informed trading affect price efficiency? To answer this, we exploit rule amendments in U.S. securities markets, which impose a higher frequency of public disclosure of short positions. Higher public disclosure can hurt the production of information and deteriorate efficiency, or it can be beneficial by mitigating the limits to arbitrage and diffusing arbitrageurs' information faster. With more frequent disclosure, information encapsulated within short interest is incorporated into prices faster, improving price efficiency. We find important reductions in short sellers' horizon risk and increases in short sales with the rule amendments.

I. Introduction

Arbitrageurs' activities are often viewed as essential for bringing prices in line with their fundamental value and creating efficient markets. In the aftermath of the financial market crisis and in particular with the Dodd–Frank Act, there has been increased attention on understanding the role of arbitrageurs and informed traders in financial markets. Specifically, there has been heightened interest and debate as to whether arbitrageurs and informed traders should face more stringent public-disclosure requirements.[1] Regulatory policies aimed at greater public

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[1]See, for instance, Title IV of the Dodd–Frank Act, which broadened the scope of regulatory disclosure requirements on investor advisors, including hedge funds. Currently, hedge funds are required to disclose with regulators; however, there is a discussion on whether they should also be disclosing to the public.

disclosure can help reduce opaque trading; however, these policies may also distort incentives to produce private information and to trade, and this can be harmful to price efficiency.

In this article, we aim to contribute to this debate by analyzing the impact of greater disclosure requirements in the shorting market. The shorting market is a useful laboratory to study our research question for a number of reasons. First, there is ample evidence showing that short sellers are an example of arbitrageurs and informed traders, adept at identifying mispriced securities (Asquith, Pathak, and Ritter (2005), Boehmer, Jones, and Zhang (2008), Jones and Lamont (2002), and Karpoff and Lou (2010)). Moreover, there have been rule amendments in the U.S. securities market that have increased the public-disclosure requirements of short positions. This policy change provides a useful experiment that allows us to identify the impact of greater public-disclosure requirements imposed on arbitrageurs and informed traders.

Specifically, how does greater publicity of arbitrageurs' positions affect price efficiency? Greater public disclosure can potentially have both costs and benefits. A commonly held view is that greater public disclosure of arbitrageurs' positions can be costly because arbitrageurs may lose their informational advantages (Agarwal, Mullally, Tang, and Yang (2015), Christoffersen, Danesh, and Musto (2015), and Easley, O'Hara, and Yang (2014)). For instance, with greater public-disclosure requirements, short sellers may end up having to disclose their positions before they fully build them up, therefore revealing their private information prematurely. Furthermore, short sellers may lose their informational advantage because detailed information on positions can enable other market participants to uncover their underlying proprietary investment strategies. This may prevent short sellers from fully reaping the benefits of their private information, which reduces the incentive to produce information in the first place, thereby worsening efficiency (Grossman and Stiglitz (1980)).

A newly emerging view highlights that increased public-disclosure requirements can actually be beneficial by helping arbitrageurs overcome the limits to arbitrage (Kovbasyuk and Pagano (2015), Ljungqvist and Qian (2016)). Arbitrageurs can be hesitant to attack a mispricing because of horizon risk, the risk that the mispricing can take too long to correct so that potential profits are eroded as a result of accumulating transaction costs or the risk that the mispricing worsens in the short run (Dow and Gorton (1994), Abreu and Brunnermeier (2002), and Barberis and Thaler (2003)).[2] For instance, in Abreu and Brunnermeier, arbitrageurs learn about an arbitrage opportunity sequentially, and arbitrageurs may prefer to wait when they are unsure that other market participants will also attack the mispricing. Public disclosure of short-sales positions can therefore be helpful because it can allow the rest of the investing public to learn from short sellers more promptly. Moreover, if increased public disclosure of short positions hastens the diffusion of short sellers' information, then short sellers' horizon risk would be reduced, thereby increasing short-selling activity and improving price efficiency.

[2]Although the term *arbitrage*, strictly speaking, refers to riskless speculation, we follow the recent literature and use the term as referring to an investor's ability to detect mispriced securities.

We provide an empirical examination of these two views by studying the effects of amendments approved by the U.S. Securities and Exchange Commission (SEC) that increased the frequency of short-interest reporting requirements from once a month to twice a month, effective Sept. 7, 2007. U.S. securities exchanges publicize each stock's total short interest, which is defined as the total outstanding short positions in a given stock.

Prior to the amendments, investors in the United States received new information on short interest only after the settlement date on the 15th of each month. In the postamendment period, investors receive additional new information on short interest after the settlement date at the end of each month. Our identification strategy exploits the fact that in the postamendment period, additional information on short interest is publicly reported after the settlement date at the end of each month, whereas in the preamendment period, short sellers were not required to disclose their positions on these dates.

We therefore generate "placebo dates," that is, dates where short interest would have been publicly reported had broker-dealers been required to report the short positions at the end of the month in the preamendment period. Our methodology is a differences-in-differences test in which we test the difference in price efficiency after the end-of-month report dates (including the placebo dates) between the pre- and postamendment periods, over and above the differences in price efficiency after the mid-month report dates between the pre- and postamendment periods. By taking the difference over and above the differences in price efficiency after the mid-month report dates (which are available in both the pre- and postamendment periods), we control for the possible market-wide changes in price efficiency from the pre- to postamendment periods. This methodology therefore allows us to isolate the impact of the extra short-interest announcement from potential confounding effects.

Our results show that the new disclosure regime has an important impact on a stock's informational environment. Information encapsulated within short interest, which contains information about future company news, is more quickly incorporated into prices, thereby increasing price efficiency. Our estimates show that in the preamendment period, price efficiency is on average 7%–10% worse in the 2-week period after the placebo end-of-month report dates. However, in the postamendment period, this difference almost completely dissipates.

The results are asymmetric in that the effects are larger for stocks with negative information, and they are pronounced for stocks with higher arbitrage risk, indicating that the rule change helps with overcoming limits to arbitrage. The regime change matters more for observations that are further away from the last short-interest announcement, consistent with the impact of the disclosure of the short-interest announcement decaying over time. If the findings are driven by the mechanism that, with greater disclosure, the wider investing public learns about short sellers' private information more promptly, then it should be that short-interest announcements reveal new information to which the market reacts. An alternative idea is that investors may gather information on short-selling activity from alternative sources (e.g., through access to proprietary data sets or informal contacts with brokers) and thus short-interest announcements do not really add to investors' information set. Result support the learning channel. There are significant price adjustments to *changes* in short interest on announcement dates and such pricing effects are long-lasting. Interestingly, price reactions are pronounced in the post period, arguably due to short interest becoming more informative. In extended analyses, we find no evidence that short sellers use public announcements to manipulate the market.

We next examine the impact of the more frequent public-disclosure requirements on the horizon risk that short sellers face and their trading activities. If the improvements in price efficiency that we find after the amendments are being driven by short sellers' private information diffusing faster, we would expect a decline in their holding periods as a result of them cashing their positions more quickly. Furthermore, in response to there being lower horizon risk, we would expect them to take larger positions.

Our results confirm these predictions. We find that short sellers' holding period (approximately 80 calendar days for a typical stock) is reduced by 10 calendar days under the new disclosure regime and that short-selling activity is higher after the announcement days in the postamendment period. We also find that there is a higher reward-to-risk ratio following the days after the public disclosure of the additional short-interest announcement in the postamendment period, consistent with the idea that public disclosure of short interest accelerates the diffusion of short sellers' information, enabling more reliable profits.

Our article contributes to the literature that studies the effects of increased publicity for arbitrage activity and informed trading. The views in this literature are mixed. Some authors argue that greater publicity can be harmful to price efficiency. With more disclosure, arbitrageurs may lose their informational advantages and therefore diminish their activities (e.g., Agarwal et al. (2015), Christoffersen et al. (2015), and Easley et al. (2014)). This then negatively affects price efficiency.

A manifestation of this view in the context of the shorting market has been documented by Jones, Reed, and Waller (2016), who study public-disclosure rules in the European Union (EU). The EU rules require short sellers to immediately disclose their positions to the public when their positions cross a threshold (0.5% of shares outstanding). The authors find that the EU's disclosure regime negatively affects the amount of short selling, and therefore it deteriorates price efficiency.

We study the policy adopted in the United States, which is different from the EU rules in a number of ways. Different from their EU counterparts, U.S. regulators require public disclosure of each stock's total short interest as opposed to releasing trader-level information. Furthermore, in contrast with the immediate-disclosure requirements of the EU, U.S. regulators publicly disclose short-interest information on a bimonthly basis on prescheduled announcement dates, which arguably provides enough time and flexibility to short sellers to execute their trades without revealing them prematurely. The new finding that emerges from our analysis is that the U.S. rules can alleviate the potential negative consequences such that the benefits of public disclosures outweigh the potential costs.

In this regard, our findings are consistent with articles that emphasize the benefits that publicizing arbitrageurs' positions can provide (e.g., Kovbasyuk and Pagano (2015), Ljungqvist and Qian (2016), and Makarov and Plantin (2012)). These articles argue that public disclosures can help arbitrageurs overcome the limits to arbitrage arising from horizon risk, and subsequently, public disclosures can improve price efficiency. Our article contributes to the literature by providing new evidence using the mandatory disclosure rules implemented in the U.S. shorting market. To date, the only evidence consistent with the latter view is provided by Ljungqvist and Qian, who document that some boutique hedge funds occasionally share their information with the public voluntarily. While Ljungqvist and Qian are interested in understanding whether the market responds to these voluntary disclosures, we examine the broader efficiency implications of mandatory public disclosures.

II. Methodology and Data Sources

A. Methodology

On Mar. 6, 2007, the SEC approved amendments to revise the short-interest reporting requirements of all major securities exchanges and the National Association of Securities Dealers (NASD), now known as the Financial Industry Regulatory Authority (FINRA). The amendments required that as of Sept. 7, 2007, member firms of these securities exchanges and FINRA must increase the frequency of short-interest reporting from once per month to twice per month.[3] Prior to the amendments, member firms were required to submit a mid-month short-interest report that was based on short positions held on the settlement date, namely, the 15th of each month. If the 15th happened to fall on a weekend, the designated settlement date was the previous business day on which the transactions settled. After the amendments, however, in addition to the mid-month short-interest report, member firms are also required to submit an end-of-month short-interest report based on short positions held on the last business day of the month on which transactions settle. Member firms have until 6:00PM (E.T.) 2 business days after the settlement date to report their short positions. Short interest is then aggregated on a stock-by-stock basis across all member firms and publicly disseminated after 4:00PM (E.T.), 8 business days later, on prescheduled announcement days.[4] We denote the date of public dissemination of short interest as REP_DATE. The time of public dissemination of short interest is after the market close, so the next business day after REP_DATE is the date of interest in this article because the next business day is when the market is able to react to this public information.

The objective of this article is to understand whether increased public disclosure of short interest has an impact on price efficiency. The SEC-approved amendments provide a useful setting for identifying the impact of short-interest disclosure because in the preamendment period, the short-interest announcement occurred on a fixed date in the middle of the month, and in the postamendment

[3]The entities that were affected by these SEC-approved amendments include the Boston Stock Exchange, the Chicago Board Options Exchange, the Chicago Stock Exchange, FINRA, the International Stock Exchange, the National Association of Securities Dealers Automated Quotations (NASDAQ), the National Stock Exchange, the New York Stock Exchange (NYSE), NYSE Arca, the American Stock Exchange (now known as NYSE MKT), and the Philadelphia Stock Exchange (see https://www.finra.org/sites/default/files/NoticeDocument/p019161.pdf).

[4]Publication schedules for short interest dissemination are available at: http://www.nasdaqtrader.com/Trader.aspx?id=ShortIntPubSch.

period, there is an extra short-interest announcement occurring on a fixed date at the end of the month. Our analysis therefore focuses on whether this extra short-interest disclosure affects efficiency.

Our identification strategy relies on generating "placebo dates," that is, dates when short interest would have been publicly reported had broker-dealers been required to report short-interest positions at the end of the month in the preamendment period. We generate the placebo dates in the preamendment period following the disclosure rules explained previously. Using both the actual and placebo REP_DATEs, we estimate the impact of more frequent reporting of short interest on price efficiency. To estimate the effect the additional short-interest disclosure has on price efficiency, we estimate the following:

$$(1) \qquad \text{EFF}_{i,t} \quad = \quad \alpha_i + \beta_0 e_{i,t} + \beta_1 \text{POST}_{i,t} + \beta_2 [e \times \text{POST}]_{i,t} + \gamma X_{i,t} + \varepsilon_{i,t},$$

where EFF denotes our measures of price efficiency for stock i at time t. For the independent variables, we include e, which is a dummy variable that equals 1 for observations after the end-of-month REP_DATE and before the mid-month REP_DATE the following month, and equals 0 for observations after the mid-month REP_DATE and before the end-of-month REP_DATE. POST is a dummy variable that equals 1 for observations in the postamendment period (i.e., after Sept. 7, 2007), and 0 otherwise, and the variable $e \times \text{POST}$ is the interaction term between POST and e. In extended tests, we include a vector of control variables, which the previous literature shows to be related to our dependent variable, along with Fama–French industry, year, month, and day-of-week time fixed effects. Standard errors are double-clustered by stock and day.

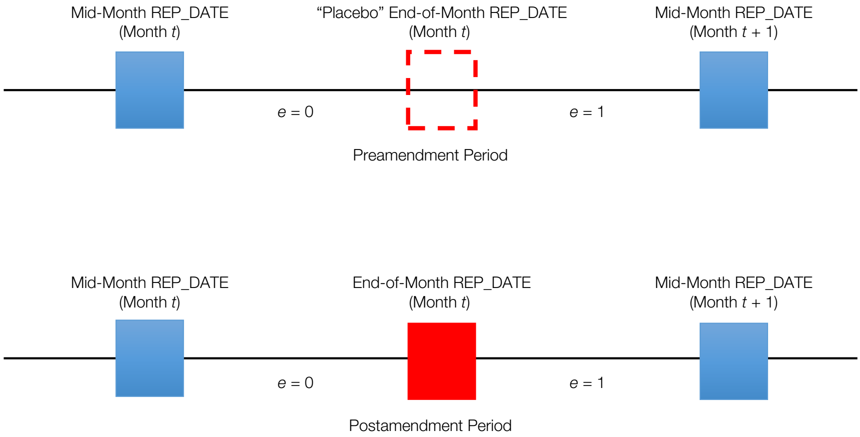
β_2 is the main variable of interest because it quantifies the impact of the extra short-interest disclosure in the postamendment period. This coefficient captures the differences in price efficiency after the end-of-month REP_DATE between the pre- and postamendment periods, over and above the differences in price efficiency measured after the mid-month REP_DATE between the pre- and postamendment periods. Although mid-month short-interest announcements take place in both the pre- and postamendment periods, end-of-month short-interest announcements take place only in the postamendment period. By calculating the effect as over and above the differences in price efficiency measured after the mid-month short-interest announcements, we control for the possible aggregate changes in efficiency from the pre- to postamendment period. Therefore, this methodology allows us to isolate the impact of the extra short-interest announcement from potential confounding effects arising from market-wide changes. Figure 1 provides a graphical representation of the empirical methodology. If greater public disclosure of short sales negatively affects the production of information by short sellers, price efficiency would worsen; if it does not negatively affect information production and instead helps with incorporating short sellers' information into prices faster, then price efficiency is expected to increase.

The important identifying assumption is that bimonthly report dates are specific to short-interest announcements. We don't assume that the rule change does not affect short sellers' trading strategies. In fact, a potential change in short seller's trading strategies would be part of the mechanism leading to results. For instance, after the regulatory regime, if short sellers reduce (increase) their

FIGURE 1

Diagrammatic Explanation of Empirical Methodology

Figure 1 looks at the differences between the end-of-month and placebo end-of-month short-interest announcements in the preamendment period (dashed square). The identification in our empirical design comes from the additional end-of-month short-interest announcement in the postamendment period (light gray square). There is no change in reporting regime for mid-month short-interest announcements in the pre- and postamendment periods. $e=0$ when the firm's earnings announcement occurs between the mid-month REP_DATE and the end-of-month REP_DATE, and $e=1$ occurs when the firm's earnings announcement occurs between the end-of-month REP_DATE and the mid-month REP_DATE of the following month.



activities, price informativeness is expected to worsen (improve). Later in the article, we examine the change in short-selling activity. To the extent that a potential change in short sellers' trading strategies has spillover effects that also affect price efficiency in the 2-week period after the mid-month short-interest announcements, the differences-in-differences estimate (β_2) would be attenuated. Therefore, a cautious interpretation of the findings is that β_2 provides a lower bound for the total economic effect.[5]

Our main measure of price efficiency is the cumulative abnormal returns (CARs) around quarterly firm earnings announcements, specifically, the absolute value of CARs to earnings news that arrive after the actual or placebo REP_DATE. There are a number of advantages of using this measure in our setting. First, this measure of price efficiency nicely ties in with the related literature showing that short sellers possess information about upcoming earnings announcements (e.g., Boehmer, Jones, Wu, and Zhang (2020), Christophe, Ferri, and Angel (2004), and Christophe, Ferri, and Hsieh (2010)). For instance, short-interest announcements can provide an informed signal to investors from which they can learn about a firm's news more readily. Second, earnings announcements allow us to analyze the asymmetric effects of positive versus negative information, a feature that cannot be easily captured by other measures of price efficiency. If prices become more (less) informative with the new regulatory regime, then market surprises to

[5]Because there is no end-of-month short-interest announcement in the preamendment period, it is reasonable to expect that the marginal impact of the rule change would be larger for the 2-week period after the end-of-month short-interest announcement.

earnings announcements thereafter are expected to be smaller (larger). We use earnings-announcements returns as the main measure, but later in the article, we broaden the analysis to include alternative measures of price efficiency.

B. Data Sources and Variables

The sample consists of common stocks (with share codes of 10 or 11) from the Center for Research in Security Prices (CRSP) and Compustat universe. Market data are obtained from the CRSP, and financial-statement-related information is obtained from Compustat. Analyses that are based on earnings announcements use additional data from the Institutional Brokers' Estimate System (IBES). When the earnings-announcement date is included in both the Compustat and IBES databases and the IBES date is different from the Compustat date, we use the earlier date as the date of the earnings announcement.[6] Earnings announcements released after 4:00PM (E.T.) are moved to the next trading day. Short- and long-term market reactions to earnings announcements are measured using different windows, namely, [0,1] and [2,61] days after the earnings announcement.[7]

We measure market reactions to earnings announcements by the absolute value of CARs to earnings announcements. Similar to DellaVigna and Pollet (2009), we compute the difference between the buy-and-hold return of the firm and beta multiplied by the buy-and-hold return of the market, and then take the absolute value:

$$(2)\qquad \text{CAR}[m,n]_{i,q} = \left| \left[\prod_{k=t}^{t+n} (1+R_{i,k}) - 1 \right] - \hat{\beta}_{i,q} \left[\prod_{k=t}^{t+n} (1+R_{m,k}) - 1 \right] \right|,$$

where $R_{i,k}$ is the return of stock i on day k; $R_{m,k}$ is the return on the market on day k; and $\hat{\beta}_{i,q}$ for stock i in quarter q is obtained from the regression $R_{i,u} = \alpha_{i,q} + \beta_{i,q} R_{m,u} + \varepsilon_{i,u}$ for the days $u \in [t-300, t-46]$, where t is the date of the earnings announcement. We use the absolute value of CARs because we are interested in examining the change in the size of earnings reactions after short-interest announcements. Later in the article, we also report results with size and book-to-market (BM) matched portfolios.

To analyze the impact of the new disclosure regime, we divide the sample into 2 subperiods. The "preamendment period" runs from Jan. 1, 2003, to Sept. 6, 2007, and the "postamendment period" runs from Sept. 7, 2007, to Dec. 31, 2012. We aim to choose a sample period that is long enough to provide empirical power (because firms announce their earnings news quarterly, we have only 4 observations per firm in each year) but also narrow enough to capture the effect attributable to regulatory amendments. Later in the article, we show that our results are robust to alternative sample periods.

[6]DellaVigna and Pollet (2009) report that the earlier of the two dates is almost always the correct announcement date in the post-1994 period.

[7]We calculate abnormal returns to earnings announcements both in a short-horizon as well as a longer-horizon window. Changes in the degree of price informativeness can affect both the immediate abnormal returns and the abnormal returns going forward. For instance, if price fully reflects the upcoming earnings news, there would be no market surprise when the news arrives and also no postannouncement drift after the arrival of the news.

Shortly after the SEC approved the amendments, markets experienced dramatic turbulence, and the SEC implemented temporary prohibitions and bans on short selling. Although our methodology would take into account the impact of market-wide changes between the pre- and postamendment periods, we exclude the 2008 calendar year and financial stocks to prevent some extreme observations during this period from affecting our findings. Additionally, following the literature, we exclude stocks with a price of less than $1 (before split adjustment) to minimize the possibility of data errors.

In robustness tests, we control for numerous variables that the previous literature shows to be related to earnings reactions (e.g., DellaVigna and Pollet (2009), Hirshleifer, Lim, and Teoh (2009)). We control for the number of analysts following the stock (NUM_EST), earnings persistence (EARNINGS_PERSIST), earnings volatility (EARNINGS_VOL), forecast error (FORECAST_ERROR), the number of earnings announcements on the given day of a firm's own earnings announcement (NUM_ANN), and institutional ownership (IO). Definitions of all variables are provided in the Appendix.

Table 1 present the descriptive statistics for our main analysis. We examine firm characteristics that the previous literature shows to be related to the size of earnings reactions. The main result from Table 1 is that there are no meaningful differences between firms that issue their quarterly earnings announcements after the mid-month or end-of-month short-interest announcement. For instance, the number of analysts giving earnings-per-share (EPS) forecasts, the analyst forecast error, the earnings persistence, and the earnings volatility are almost identical between the two samples. Although some variables, such as institutional ownership as a fraction of shares outstanding and the number of concurrent earnings announcements, are slightly higher when $e=1$ (60.57% and 4.67, respectively) than when $e=0$ (56.93% and 4.09, respectively), the differences appear to be

TABLE 1

Descriptive Statistics

Table 1 presents the descriptive statistics for our main analysis. We divide our sample into 2 subsamples: $e=0$ pertains to observations where the firm's earnings announcement occurs after the mid-month REP_DATE and before the end-of-month REP_DATE, and $e=1$ pertains to observations where the firm's earnings announcement occurs after the end-of-month REP_DATE and before the mid-month REP_DATE of the following month. NUM_EST is the natural logarithm of 1 plus the number of analysts giving earnings-per-share (EPS) forecasts for the given firm in that quarter, IO is the fraction of all shares outstanding held by institutional investors for a given stock at the end of the quarter (in %), FORECAST_ERROR is the difference between the announced earnings and the consensus EPS forecast normalized by the firm's stock price at the end of the corresponding quarter, EARNINGS_PERSIST is the first-order autocorrelation coefficient of quarterly EPS during the past 4 years, EARNINGS_VOL is the standard deviation of quarterly EPS in the past 4 years, and NUM_ANN is the natural logarithm of 1 plus the number of concurrent earnings announcements that occur on the same day as the earnings announcement for the given stock.

	Variables	Mean	Median	Std. Dev.
$e=0$	NUM_EST	1.5093	1.6094	0.8896
	IO	56.9318	59.9246	26.7693
	FORECAST_ERROR	0.0073	0.0023	0.0170
	EARNINGS_PERSIST	0.2489	0.2370	0.3044
	EARNINGS_VOL	0.4646	0.2229	0.8796
	NUM_ANN	4.0884	4.2047	0.8442
$e=1$	NUM_EST	1.5143	1.6094	0.8181
	IO	60.5778	63.7538	25.2301
	FORECAST_ERROR	0.0074	0.0027	0.0162
	EARNINGS_PERSIST	0.2449	0.2252	0.2971
	EARNINGS_VOL	0.4951	0.2469	0.9265
	NUM_ANN	4.6722	4.8978	0.8584

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rather small. Nevertheless, in our empirical specifications, we control for these firm characteristics.

III. Results

A. Main Results on Price Efficiency

1. Short-Term Price Reactions to Earnings Announcements

In Table 2, we estimate equation (1) using the absolute value of the CARs in the [0,1] days around earnings announcements. Column 1 shows the baseline results. Importantly, our main variable of interest, $e \times \text{POST}$, is significantly negative. The coefficient estimate equals -30 basis points (bps). What this indicates is that with more frequent reporting of short interest, the market is less surprised after end-of-month short-interest announcements in the postamendment period. This is consistent with short-interest announcements serving as an informative signal for investors, a signal that helps them more readily learn about future news related to company earnings.

The coefficient on $e \times \text{POST}$ shows that in the postamendment period, the average market reactions to earnings announcements that occur after the end-of-month REP_DATE are 30 bps lower than the average market reactions after the mid-month REP_DATE in the preamendment period. Because the mean and median reaction to earnings announcements (in absolute value) in our sample are 4.3% and 2.8%, respectively, the economic magnitude of a 30-bps reduction translates to an approximately 7% (11%) reduction in the mean (median) market reaction to earnings announcements.

Furthermore, we find that the coefficient on e is 32 bps, and it is statistically significant. This means that in the preamendment period, the average market reactions to earnings announcements that take place after the placebo REP_DATE are 32 bps higher than the average market reactions that take place after the mid-month REP_DATE. This result provides further support for the hypothesis that the public dissemination of short interest allows investors to learn about firm fundamentals more readily. The lack of information on short interest at the end of the month in the preamendment period leads to larger market reactions to earnings announcements that come afterward.

Altogether, the estimates reported in Table 2 imply that the differences in the efficiency measured after the mid-month REP_DATE and the efficiency measured after the end-of-month REP_DATE almost entirely dissipate in the postamendment period. This is because in the postamendment period, investors receive information about short interest both in the middle of the month and at the end of the month. Therefore, there is no longer a difference between the periods that come after a mid-month or an end-of-month REP_DATE.

We find such sign-flipping patterns throughout our tests. This strengthens the conclusion that it is the change in the reporting frequency of short interest that drives our results. An alternative hypothesis should be able to explain not only the negative estimates on $e \times \text{POST}$ but also the positive estimates on e. This can be difficult because short-interest public-disclosure announcement dates, to the best of our knowledge, are specific to the short-interest-reporting regime.

TABLE 2
Short-Term Price Reactions to Earnings Announcements

Table 2 presents the regression results for the short-term price reactions to earnings announcements. The dependent variable, CAR[0,1], is the absolute value of 2-day cumulative abnormal returns in the [0,1] days around the earnings announcement, defined as the difference between the buy-and-hold returns of the stock and beta multiplied by the buy-and-hold return of the market. The explanatory variables are as follows: POST is a dummy variable that equals 1 for the firm's earnings-announcement dates after Sept. 7, 2007, and 0 otherwise; *e* is a dummy variable that equals 1 when the firm's earnings announcement occurs after the end-of-month REP_DATE and before the mid-month REP_DATE of the following month, and 0 otherwise; and POST × *e* is an interaction term between POST and *e*. In columns 2–4, we control for NUM_EST, IO, FORECAST_ERROR, EARNINGS_PERSIST, EARNINGS_VOL, and NUM_ANN (which are defined in the Appendix), and we include industry and time (year, month-of-year, and day-of-week) fixed effects (FE). In column 4, we also include stock FE. All regressions include a constant term, whose coefficient is suppressed for reporting purposes. We present ordinary least squares estimates with standard errors (in parentheses) double-clustered by stock and earnings-announcement day. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	CAR[0,1]			
Variables	1	2	3	4
POST × *e*	−0.0030***	−0.0025***	−0.0023**	−0.0021***
	(0.0010)	(0.0010)	(0.0010)	(0.0007)
POST	0.0124***	0.0121***	0.0122***	0.0120***
	(0.0018)	(0.0019)	(0.0019)	(0.0014)
e	0.0032***	0.0027***	0.0034***	0.0028***
	(0.0007)	(0.0006)	(0.0007)	(0.0005)
NUM_EST		−0.0051***	−0.0051***	−0.0016***
		(0.0004)	(0.0004)	(0.0005)
IO		0.0000***	0.0000***	0.0000*
		(0.0000)	(0.0000)	(0.0000)
FORECAST_ERROR		0.1565***	0.1564***	0.0973***
		(0.0141)	(0.0141)	(0.0132)
EARNINGS_PERSIST		0.0031***	0.0032***	0.0033***
		(0.0008)	(0.0008)	(0.0008)
EARNINGS_VOL		0.0005*	0.0005*	0.0016***
		(0.0003)	(0.0003)	(0.0003)
NUM_ANN			−0.0015***	−0.0017***
			(0.0004)	(0.0003)
No. of obs.	78,317	59,020	59,020	59,020
R^2	0.071	0.121	0.121	0.063
Controls	No	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
Stock FE	No	No	No	Yes

In column 2 of Table 2, we include several stock characteristics that are shown to be related to reactions to earnings announcements. Consistent with the literature, we find that these characteristics are related to reactions to earnings announcements; however, the inclusion of these variables in our empirical specification does not change our conclusions. In column 3, we control for the total number of earnings announcements in a day. Consistent with Hirshleifer et al. (2009), the total number of earnings announcements in a day is in fact negatively related to earnings reactions, yet our results remain similar. Finally, in column 4, as an extended check, we include stock fixed effects to control for the potential impact of unobserved stock characteristics and find that our results remain robust. The results in Table 2 indicate that the coefficient on $e \times$ POST is negative and statistically significant across all specifications.[8]

[8]Including stock fixed effects enables us to control for time-invariant unobserved firm characteristics. However, the analysis with stock fixed effects also implicitly conditions the test on firms with variation in *e*, which can be a result of a change in the earnings-announcement day. Throughout the article, we report all of our main tests both with and without stock fixed effects. Furthermore, in the

An additional observation is that POST is significant and positive. Although our differences-in-differences estimator does not require POST to be 0, for completeness, we explore this further, and we show in Table IA.1 of the Supplementary Material that this is attributable to higher aggregate uncertainty in the postamendment period. POST becomes insignificant once we include empirical proxies of aggregate uncertainty in the regressions. Importantly, $e \times$ POST and e remain virtually unchanged by such modifications.

Arguably, the impact of the disclosure of short-interest announcements decays over time; the absolute value of abnormal returns is therefore expected to be higher for earnings announcements that arrive much later after the disclosure of short-interest filings. One implication is that for observations with $e=1$ and POST$=0$, the more time that elapses after the most recent short-interest announcement, the higher CAR[0,1] is expected to be. Moreover, our hypothesis predicts that the impact of the change in the reporting regime (decrease in CAR[0,1] in the postamendment period) should be more pronounced in such cases.

To test this idea, in Table 3, we introduce interaction terms with HIGH_DAYS_SINCE, a dummy variable that equals to 1 when DAYS_SINCE is above the sample median, and 0 otherwise, where DAYS_SINCE is defined as the number of trading days between an earnings announcement and the most recent short-interest announcement prior to it. For earnings announcements that take place after the (placebo) end-of-month short-interest-announcement date in the preamendment period, DAYS_SINCE is defined using the placebo REP_DATE.[9]

The results are quite informative and consistent with our proposed mechanism. $e \times$ HIGH_DAYS_SINCE is positive and significant, indicating that market reactions are indeed larger for earnings announcements that are much later after the disclosure of short-interest filings. Importantly, POST $\times e \times$ HIGH_DAYS_SINCE is negative and statistically significant, showing that the new reporting regime matters the most for such cases. As in the main regression results, the estimates of $e \times$ HIGH_DAYS_SINCE and POST $\times e \times$ HIGH_DAYS_SINCE are similar in absolute values, such that POST $\times e \times$ HIGH_DAYS_SINCE nearly offsets $e \times$ HIGH_DAYS_SINCE. These results provide strong support for our proposed mechanism.

2. Other Short-Term Effects Around Earnings Announcements

If more frequent disclosure of short interest improves the price efficiency of stock prices, we would expect that gains to price efficiency are also manifested through trading activity. Furthermore, we would also expect that the end-of-month short-interest disclosure reveals additional private information by short sellers, reducing asymmetric information. To that effect, we estimate the regression model

next section, we conduct additional robustness tests to control for the timing of a firm's earnings-announcement day. Our results remain similar.

[9]Panel A of Table IA.2 in the Supplementary Material reports the summary statistics for DAYS_SINCE for observations after the mid- and end-of-month short-interest announcements in the pre- and postamendment periods. We observe that DAYS_SINCE is typically 6 days, with no noticeable differences across different subsamples. Panel B of Table IA.2 in the Supplementary Material formally shows this in a regression analysis.

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TABLE 3
Number of Days since the Last Short-Interest Announcement

Table 3 examines the role of the time elapsed since the most recent short-interest announcement before an earnings announcement. We first define DAYS_SINCE, which is the number of trading days between an earnings announcement and the most recent short-interest announcement prior to it. For earnings announcements that take place after the (placebo) end-of-month short-interest announcement date in the preamendment period, DAYS_SINCE is defined using the placebo REP_DATE. Later, we introduce interaction terms with HIGH_DAYS_SINCE, a dummy variable that equals 1 when the firm's DAYS_SINCE is above the sample median, and 0 otherwise. All other variables are defined as in Table 2. The explanatory variables include all interaction terms between POST, *e*, and HIGH_DAYS_SINCE. In columns 2–4, we control for NUM_EST, IO, FORECAST_ERROR, EARNINGS_PERSIST, EARNINGS_VOL, and NUM_ANN and include industry and time (year, month-of-year, and day-of-week) fixed effects (FE). In column 4, we also include stock FE. All regressions include a constant term, whose coefficient is suppressed for reporting purposes. We present ordinary least squares estimates with standard errors (in parentheses) double-clustered by stock and earnings-announcement day. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	CAR[0,1]			
Variables	1	2	3	4
POST × *e*	−0.0007	−0.0003	−0.0002	−0.0003
	(0.0009)	(0.0010)	(0.0010)	(0.0010)
POST × *e* × HIGH_DAYS_SINCE	−0.0033**	−0.0044***	−0.0041***	−0.0031**
	(0.0014)	(0.0015)	(0.0015)	(0.0015)
POST × HIGH_DAYS_SINCE	0.0012	0.0010	0.0004	0.0012
	(0.0010)	(0.0010)	(0.0010)	(0.0010)
e × HIGH_DAYS_SINCE	0.0022**	0.0036***	0.0039***	0.0036***
	(0.0011)	(0.0012)	(0.0012)	(0.0012)
HIGH_DAYS_SINCE	0.0011	0.0013	0.0009	0.0004
	(0.0008)	(0.0008)	(0.0008)	(0.0008)
POST	0.0099***	0.0104***	0.0106***	0.0105***
	(0.0014)	(0.0015)	(0.0015)	(0.0014)
e	0.0019*	0.0010	0.0012*	0.0008
	(0.0011)	(0.0007)	(0.0007)	(0.0007)
NUM_EST		−0.0051***	−0.0051***	−0.0016***
		(0.0003)	(0.0003)	(0.0005)
IO		0.0000***	0.0000***	0.0000*
		(0.0000)	(0.0000)	(0.0000)
FORECAST_ERROR		0.1568***	0.1568***	0.0976***
		(0.0131)	(0.0131)	(0.0132)
EARNINGS_PERSIST		0.0031***	0.0032***	0.0033***
		(0.0006)	(0.0006)	(0.0008)
EARNINGS_VOL		0.0005**	0.0005***	0.0017***
		(0.0002)	(0.0002)	(0.0003)
NUM_ANN			−0.0015***	−0.0017***
			(0.0003)	(0.0003)
No. of obs.	71,976	59,020	59,020	59,020
R^2	0.105	0.121	0.121	0.063
Controls	No	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Stock FE	No	No	No	Yes

in equation (1); however, instead, we use TURNOVER, VOLATILITY, and SPREAD as the dependent variables.

In Table 4, we start by using TURNOVER as the dependent variable. The coefficient on $e \times$ POST is negative (−0.0011) and statistically significant, implying that in the postamendment period, there is on average a 7.2% reduction in turnover around earnings announcements that occur after the end-of-month REP_DATE. Similarly, in column 2, we use VOLATILITY as the dependent variable and find that the coefficient on $e \times$ POST is negative (−0.0209) and statistically significant, suggesting that volatility around earnings announcements after the end-of-month REP_DATE is significantly lower (approximately 6.8%, on average) than in the preamendment period. Together, these results are in congruence with the pricing

TABLE 4
Other Short-Term Effects around Earnings Announcements

Table 4 presents the regression results for the measures of other effects around earning announcements. The dependent variables are as follows: In column 1, TURNOVER is the average daily volume over the [0,1] days around the earnings announcement divided by the stock's shares outstanding; in column 2, VOLATILITY is the difference between the highest and lowest share prices over the [0,1] days around the earnings announcement, normalized by an average of the two; and in column 3, SPREAD is the daily average bid–ask spread over the [–4,–2] days before the earnings announcement. The explanatory variables include the following: POST is a dummy variable that equals 1 for the firm's earnings-announcement dates after Sept. 7, 2007, and 0 otherwise; *e* is a dummy variable that equals 1 when the firm's earnings announcement occurs after the end-of-month REP_DATE and before the mid-month REP_DATE of the following month, and 0 otherwise; and POST × *e* is an interaction term between POST and *e*. All regressions include the control variables NUM_EST, IO, FORECAST_ERROR, EARNINGS_PERSIST, EARNINGS_VOL, and NUM_ANN and industry, stock, and time (year, month-of-year, and day-of-week) fixed effects (FE). Control variables are defined in the Appendix. We include a constant term in all regression specifications but suppress it for reporting purposes. We present ordinary least squares estimates with standard errors (in parentheses) double-clustered by stock and earnings-announcement day. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	TURNOVER	VOLATILITY	SPREAD
Variables	1	2	3
POST × *e*	−0.0011***	−0.0209***	−0.0126**
	(0.0003)	(0.0066)	(0.0057)
POST	0.0010*	0.0077*	0.1061***
	(0.0005)	(0.0045)	(0.0097)
e	0.0009***	0.0149***	0.0079*
	(0.0002)	(0.0048)	(0.0044)
No. of obs.	59,934	59,425	59,904
R^2	0.082	0.022	0.132
Controls	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes

results presented in Table 2; that is, in the postamendment period, earnings announcements occurring after the end-of-month short-interest announcements are less of a surprise to the market, and thus the lower price reactions are complemented by lower trading activity (turnover) and lower volatility.

We also expect the regulatory amendments to affect information asymmetry and liquidity. The revelation of short sellers' private information through increased public disclosure of short interest may reduce market makers' risks arising from asymmetric information and therefore lower the bid–ask spread (Glosten and Milgrom (1985)). We measure bid–ask spreads prior to the earnings announcements because earnings announcements are prescheduled announcements; thus, market makers can anticipate an increase in informed trading activity before the earnings announcements. The results show that the coefficient on $e \times$ POST is negative (-0.0126) and statistically significant, indicating that in the postamendment period, there is on average a 7% reduction in the pre-earnings-announcement bid–ask spread. Intuitively, these results indicate that more frequent disclosure of short interest expedites the incorporation of short sellers' private information into the public domain. The market learns about their private information, and this reduces asymmetric information between investors prior to firms' earnings announcements. These results complement the findings in Table 2.

3. Long-Term Price Reactions to Earnings Announcements

We examine whether long-term price reactions after earnings announcements are also mitigated once there is more frequent disclosure of short interest. The results so far indicate that market participants learn from short-interest

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TABLE 5

Long-Term Price Reactions to Earnings Announcements

Table 5 presents the regression results for the long-term price reactions to earnings announcements. The dependent variable, CAR[2,61], is the absolute value of 60-day cumulative abnormal returns in the [2,61] days after the earnings announcement, defined as the difference between the buy-and-hold returns of the stock and beta multiplied by the buy-and-hold return of the market. The explanatory variables are as follows: POST is a dummy variable that equals 1 for the firm's earnings-announcement dates after Sept. 7, 2007, and 0 otherwise; *e* is a dummy variable that equals 1 when the firm's earnings announcement occurs after the end-of-month REP_DATE and before the mid-month REP_DATE of the following month, and 0 otherwise; POST × *e* is an interaction term between POST and *e*. In columns 2–4, we control for NUM_EST, IO, FORECAST_ERROR, EARNINGS_PERSIST, EARNINGS_VOL, and NUM_ANN (which are defined in the Appendix) and include industry and time (year, month-of-year, and day-of-week) fixed effects (FE). In column 4, we also include stock FE. All regression specifications include a constant term, whose coefficient is suppressed for reporting purposes. We present ordinary least squares estimates with standard errors (in parentheses) double-clustered by stock and earnings-announcement day. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	CAR[2,61]			
Variables	1	2	3	4
POST × *e*	−0.0066**	−0.0083***	−0.0080***	−0.0075***
	(0.0026)	(0.0026)	(0.0026)	(0.0027)
POST	0.0309***	0.0269***	0.0270***	0.0269***
	(0.0039)	(0.0043)	(0.0043)	(0.0041)
e	0.0016	0.0042**	0.0050***	0.0053***
	(0.0019)	(0.0017)	(0.0019)	(0.0019)
NUM_EST		−0.0166***	−0.0166***	−0.0058***
		(0.0009)	(0.0009)	(0.0020)
IO		−0.0003***	−0.0003***	−0.0005***
		(0.0000)	(0.0000)	(0.0001)
FORECAST_ERROR		0.8893***	0.8892***	0.5217***
		(0.0606)	(0.0606)	(0.0631)
EARNINGS_PERSIST		0.0071***	0.0072***	0.0085***
		(0.0021)	(0.0021)	(0.0030)
EARNINGS_VOL		0.0057***	0.0057***	0.0035**
		(0.0008)	(0.0008)	(0.0015)
NUM_ANN			−0.0017	−0.0061***
			(0.0010)	(0.0013)
No. of obs.	74,733	56,609	56,609	56,609
R^2	0.024	0.073	0.073	0.028
Controls	No	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes

announcements about upcoming earnings announcements, and therefore price informativeness increases. As we discuss in Section II, the increase in price informativeness is expected to affect not only the immediate price reactions to earnings announcements but also the price reactions thereafter. In Table 5, we estimate equation (1) using the [2,61] days after earnings announcements as the measure of CARs. Across all specifications, the coefficient estimates on $e \times$ POST are negative and statistically significant, ranging between −66 bps and −83 bps. These estimates indicate an average of a 7%–9% reduction in long-term price reactions to earnings announcements after the end-of-month REP_DATE in the postamendment period.[10]

4. Robustness

In this section, we conduct a number of robustness tests. First, we assess whether the timing of a firm's earnings announcements affects our results. We reconduct the analysis using a subsample of firms that have a propensity to

[10]Table IA.3 in the Supplementary Material reproduces Tables 2 and 5 using size- and BM-adjusted returns in the dependent variable. The findings are remarkably similar.

release their earnings announcements in the same time frame relative to the end-of-month REP_DATE, in both the pre- and postamendment periods. Specifically, we select the firms that release their earnings news in the same time frame (i.e., either in the first or the second 2 weeks, defined with respect to short-interest announcement dates) more than 50% of the time, in both the pre- and postamendment periods. The subsample constructed in this way contains approximately 65% of the firms included in the original sample. The results are reported in Panel A of Table 6. We observe that the coefficient on $e \times$ POST across all specifications is negative, statistically significant, and of comparable magnitude to the results presented previously. This robustness check highlights that the timing of earnings announcements does not drive our results.

Recently, Heitz, Narayanamoorthy, and Zekhnini (2019) show that, with the Sarbanes–Oxley Act, the number of 8-K filings increased significantly since 2004, and the earnings-announcement premium has vanished as a result. To ensure that the results we document are not stemming from 8-K filings, we download all 8-K filings from the Electronic Data Gathering, Analysis, and Retrieval (EDGAR) database for each firm during our sample period and conduct robustness tests. We define #8K_FILINGS as the total number of 8-K filings (per firm) reported between two consecutive REP_DATEs prior to the earnings announcement plus the number of filings since the last REP_DATE before the earnings announcement. For observations that are after the (placebo) end-of-month short-interest-announcement date in the preamendment period, #8K_FILINGS is defined using the placebo REP_DATE. The results reported in Panel B of Table 6 show the robustness of our findings to 8-K filings.[11]

In July 2007, the SEC removed the uptick rule for the remaining NYSE, American Stock Exchange (AMEX), and NASDAQ stocks that had not been included in the original Regulation SHO pilot implemented in May 2005. This could be viewed as relaxing short-sale constraints, thereby improving price efficiency. If the removal of the uptick rule were to explain our results, one would need to be able to explain why efficiency was worse after the (placebo) end-of-month short-interest announcements in the preamendment period. It is not clear why this would be the case. Nonetheless, we reproduce our main results excluding the periods from Jan. 2003 to May 2005 as well as the stocks that had a change in the uptick-test rule in July 2007. Therefore, the new sample uses only stocks that did not experience a change in the uptick rule during our sample period. Panel C of Table 6 shows that the results remain similar.

In Panel D of Table 6, we define CAR[0,1] and VOLATILITY using high-frequency returns. In columns 1 and 3, respectively, CAR[0,1] is the absolute value of the sum of 1- and 30-minute returns; in columns 2 and 4, VOLATILITY is the realized variance (in basis points) of these high-frequency returns over the [0,1] days around the earnings announcement. Our findings carry over when we use high-frequency returns around earnings-announcement dates. Finally, we test whether the results are robust to the inclusion of aggregate short interest in the

[11] As an alternative approach to control for information released through 8-K filings, we also control for the total abnormal returns realized after 8-K filings released prior to the earnings-announcement date. Our results remain unaffected (Table IA.4 in the Supplementary Material).

regressions (Table IA.4 in the Supplementary Material) and to the use of alternative sample periods (Table IA.5 in the Supplementary Material). We find in both analyses that the findings are robust.

5. Alternative Measures of Price Efficiency

In this section, we test whether our main results carry over when we use alternative measures of price efficiency that do not depend on earnings

TABLE 6

Robustness Tests

Table 6 presents the robustness-test results for the main regressions on price efficiency. In Panel A, we present the results for a subsample of firms that tend to announce their earnings in the same time window relative to the short-interest announcement (either $e=0$ or $e=1$ at each REP_DATE) in both the pre- and postamendment periods. In Panel B, we present the robustness of regression results to the number of 8-K filings that firms have released prior to their earnings announcements. #8K_FILINGS is the total number of 8-K filings reported between two consecutive REP_DATEs prior to the earnings announcement plus the number of filings since the last REP_DATE before the earnings-announcement date. For observations that are after the (placebo) end-of-month short-interest-announcement date in the preamendment period, #8K_FILINGS is defined using the placebo REP_DATE. In Panel C, we present the robustness of regression results to Regulation SHO regulations. Regulation SHO is a regulation implemented on Jan. 3, 2005, that removed the uptick rule for a pilot group of stocks. On July 6, 2007, the U.S. Securities and Exchange Commission (SEC) implemented the rule for the remaining stocks that had not been included in the original Regulation SHO pilot. We reproduce our results excluding the periods from Jan. 2003 to May 2005 as well as the stocks that experienced a change in the uptick-test rule in July 2007. In Panels A–C, the dependent variables are CAR[0,1], TURNOVER, SPREAD, VOLATILITY, and CAR[2,61], respectively. CAR[0,1] and CAR[2,61] are defined as the difference between the buy-and-hold returns of the stock and beta multiplied by the buy-and-hold return of the market. In Panel D, we define CAR[0,1] and VOLATILITY using high-frequency returns. In columns 1 and 3, CAR[0,1] is the absolute value of the sum of 1- and 30-minute returns, respectively; in columns 2 and 4, VOLATILITY is the realized variance (in basis points) of these high-frequency returns over the [0,1] days around the earnings announcement. The explanatory variables are POST, e, and POST × e, and the control variables are NUM_EST, IO, FORECAST_ERROR, EARNINGS_PERSIST, EARNINGS_VOL, and NUM_ANN. All variables are defined in the Appendix. Regressions include a constant term, which is suppressed for reporting purposes. We present ordinary least squares estimates with standard errors (in parentheses) double-clustered by stock and earnings-announcement day. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	CAR[0,1]	TURNOVER	SPREAD	VOLATILITY	CAR[2,61]
Variables	1	2	3	4	5
Panel A. Timing of Earnings News					
POST × e	−0.0027***	−0.0009***	−0.0136**	−0.0314***	−0.0082***
	(0.0009)	(0.0003)	(0.0069)	(0.0081)	(0.0030)
POST	0.0135***	0.0014**	0.0928***	0.0026	0.0286***
	(0.0016)	(0.0007)	(0.0115)	(0.0140)	(0.0051)
e	0.0033***	0.0008***	0.0097*	0.0219***	0.0033
	(0.0007)	(0.0003)	(0.0057)	(0.0062)	(0.0023)
No. of obs.	39,171	39,734	39,710	39,362	37,519
R^2	0.064	0.086	0.144	0.024	0.033
Controls	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Panel B. Number of 8-K Filings					
POST × e	−0.0018**	−0.0008***	−0.0135**	−0.0192***	−0.0077***
	(0.0007)	(0.0003)	(0.0057)	(0.0066)	(0.0027)
POST	0.0117***	0.0007	0.1067***	−0.0090	0.0270***
	(0.0014)	(0.0005)	(0.0097)	(0.0115)	(0.0041)
e	0.0023***	0.0005**	0.0071	0.0124**	0.0056***
	(0.0005)	(0.0002)	(0.0045)	(0.0048)	(0.0019)
#8K_FILINGS	−0.0015***	−0.0013***	−0.0043***	−0.0085***	−0.0013*
	(0.0002)	(0.0001)	(0.0016)	(0.0019)	(0.0007)
No. of obs.	59,020	59,934	59,904	59,425	56,609
R^2	0.063	0.086	0.132	0.022	0.028
Controls	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes	Yes	Yes

(continued on next page)

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TABLE 6 (continued)
Robustness Tests

Variables	CAR[0,1] 1	TURNOVER 2	SPREAD 3	VOLATILITY 4	CAR[2,61] 5
Panel C. Regulation SHO					
POST × *e*	−0.0025** (0.0011)	−0.0009** (0.0004)	−0.0120 (0.0088)	−0.0179* (0.0099)	−0.0057** (0.0026)
POST	0.0119*** (0.0018)	0.0017** (0.0007)	0.0854*** (0.0145)	−0.0139 (0.0154)	0.0190*** (0.0055)
e	0.0026*** (0.0009)	0.0011*** (0.0003)	−0.0002 (0.0073)	0.0118 (0.0082)	0.0027 (0.0028)
No. of obs.	27,793	28,265	28,265	27,963	26,486
R^2	0.057	0.076	0.066	0.022	0.024
Controls	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes	Yes	Yes

Panel D. CAR and VOLATILITY Using High-Frequency Returns

Variables	CAR[0,1] Using 1-Min Returns 1	VOLATILITY Using 1-Min Returns 2	CAR[0,1] Using 30-Min Returns 3	VOLATILITY Using 30-Min Returns 4
POST × *e*	−0.0025*** (0.0008)	−0.0133** (0.0064)	−0.0016** (0.0007)	−0.0861** (0.0387)
POST	0.0126** (0.0056)	0.1616** (0.0813)	0.0109** (0.0053)	0.3473** (0.1606)
e	0.0018*** (0.0007)	0.0164** (0.0083)	0.0020*** (0.0005)	0.0911* (0.0480)
No. of obs.	52,874	52,874	52,874	52,874
R^2	0.039	0.043	0.042	0.045
Controls	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes	Yes

announcements. Our first approach is to follow Hou and Moskowitz (2005) and estimate price delay, the delay with which stock prices respond to market information. The greater the price delay is, the more the stock's return variation can be captured by lagged market returns, indicating less price efficiency.

We adopt a variant of Hou and Moskowitz's (2005) price-delay measures because they estimate each stock's price-delay measures only once per year using the time series of 1 year of lagged stock returns. We estimate Hou and Moskowitz's measures of price delay by pooling daily stock returns between two consecutive REP_DATEs (including the placebo ones) for each stock pertaining to each POST and e. Our first price-delay measure is DELAY1, which considers the impact of lagged market returns predicting future stock returns. The second measure, DELAY1_NEG, is similar to the first one, but it differs from it by using only negative lagged market returns for the estimation. The third measure, DELAY3, distinguishes between shorter and longer lags of market returns and accounts for the precision of estimates on the coefficient of lagged market returns. The Appendix provides the details regarding the calculation of these variables. We reestimate our regression equation using DELAY1, DELAY1_NEG, and DELAY3 as our measures of information efficiency. Panel A of Table 7 shows that the results are consistent with previous findings.

TABLE 7
Alternative Measures of Price Efficiency

Table 7 presents the regression results using alternative measures of price efficiency. Panel A presents results with the price delay measure of Hou and Moskowitz (2005); Panel B presents results with high-frequency measures. In Panel A, the dependent variables are DELAY1, DELAY1_NEG, and DELAY3. DELAY1, DELAY1_NEG, and DELAY3 are estimated for each stock pertaining to each of 4 cases, POST = 0 and e=0, POST = 0 and e=1, POST = 1 and e=0, and POST = 1 and e=1, by pooling daily stock returns between two consecutive REP_DATEs (including the placebo ones). Further details of the variable definitions are in the Appendix. Columns 2, 4, and 6 include stock fixed effects (FE), and standard errors (in parentheses) are clustered by stock. In Panel B, we use high-frequency measures of price efficiency, measured as the average between the current REP_DATE and the following REP_DATE. The dependent variable in columns 1 and 2 is VAR_RATIO, and it is PE in columns 3 and 4. The explanatory variables are POST, e, and POST × e, and the control variables are idiosyncratic volatility (IVOL), the stock's market capitalization (SIZE), book-to-market ratio (BM), past cumulative monthly returns (PAST_RETURNS), and illiquidity (ILLIQ). Definitions of variables are provided in the Appendix. Regressions include time (year, month-of-year, and day-of-week) FE; in columns 2 and 4, we also include the control variables of industry and stock FE. Standard errors (in parentheses) are clustered by stock and short-interest-announcement days. All regressions include a constant term (unreported). *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Price Delay

	DELAY1		DELAY1_NEG		DELAY3	
Variables	1	2	3	4	5	6
POST × e	−0.0184***	−0.0209***	−0.0343***	−0.0372***	−0.1187***	−0.0947***
	(0.0049)	(0.0044)	(0.0051)	(0.0047)	(0.0360)	(0.0357)
POST	0.0064	0.0035	0.0038	0.0021	0.0094	0.0055
	(0.0047)	(0.0043)	(0.0045)	(0.0042)	(0.0265)	(0.0268)
e	0.0207***	0.0247***	0.0574***	0.0617***	0.0790***	0.0866***
	(0.0034)	(0.0031)	(0.0035)	(0.0033)	(0.0260)	(0.0261)
No. of obs.	21,033	21,033	21,033	21,033	18,187	18,187
R^2	0.019	0.021	0.015	0.026	0.003	0.002
Stock FE	No	Yes	No	Yes	No	Yes

Panel B. Variance Ratio and Pricing Error

	VAR_RATIO		PE	
Variables	1	2	3	4
POST × e	−0.0236***	−0.0206***	−0.0039***	−0.0041***
	(0.0009)	(0.0010)	(0.0003)	(0.0003)
POST	0.0418***	0.0456***	0.0007	0.0061***
	(0.0023)	(0.0027)	(0.0007)	(0.0007)
e	0.0199***	0.0173***	0.0045***	0.0033***
	(0.0016)	(0.0017)	(0.0005)	(0.0005)
No. of obs.	533,604	419,321	451,621	357,784
R^2	0.016	0.076	0.050	0.242
Controls	No	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes
Industry FE	No	Yes	No	Yes
Stock FE	No	Yes	No	Yes

The coefficients on $e \times$ POST are significantly negative regardless of the delay measure used, which indicates improvements in price efficiency.

Furthermore, we calculate high-frequency measures of price efficiency based on intraday trades and quotes from Trade and Quote (TAQ).[12] Our first high-frequency measure of price efficiency is based on articles (e.g., Boehmer and Kelley (2009)) that use variance ratios to test whether prices follow a random walk. A random walk implies that the ratio of longer-term to shorter-term return variances, scaled by unit of time, should be equal to 1. We construct our measure of the variance ratio, defined as VAR_RATIO$=$ $|1-(\text{VAR(30MIN)}/30\text{VAR(1MIN)})|$, where VAR(30MIN) is the variance of

[12]For further details regarding the processing of TAQ data and the construction of the high-frequency measures of price efficiency, please refer to the Supplementary Material.

30-minute intraday returns, and VAR(1MIN) is the variance of 1-minute intraday returns. According to this measure, a smaller VAR_RATIO indicates that stock prices are more informationally efficient. Panel B of Table 7 reports the results using VAR_RATIO as the measure of price efficiency. Column 1 shows the results with no control variables; column 2 includes control variables that might be associated with high-frequency measures of price efficiency. We find that the coefficient on $e \times$ POST in both specifications is significantly negative.

Our second high-frequency measure of price efficiency is based on calculating pricing errors (e.g., Boehmer and Kelley (2009), Boehmer and Wu (2013), and Hasbrouck (1993)). We decompose log intraday transaction prices from TAQ into an efficient-price, random-walk component (m_t) and a stationary component, the pricing error (s_t). We then construct the scaled pricing error, $\text{PE} = (\sigma(s)/\sigma(p))$, where $\sigma(s)$ is the standard deviation of the pricing error, which is assumed to follow a 0-mean, covariance-stationary process, and $\sigma(p)$ is the standard deviation of intraday transaction prices, used to control for cross-sectional differences in price volatility. According to this measure, a small PE indicates that stock prices are more informationally efficient. Columns 3 and 4 of Panel B in Table 7 show the results using PE as the measure of price efficiency. Consistent with previous findings, we find that the coefficients on $e \times$ POST are significantly negative.

6. Summary

The findings in Section III show that the new reporting regime improves price efficiency. This finding is contrary to the view that higher public-disclosure requirements would be harmful to efficiency because they hurt the production of information, but it is consistent with articles emphasizing the benefits that can come with publicizing private information. Overall, our findings are consistent with those of Ljungqvist and Qian (2016), who document that some boutique hedge funds occasionally share their information with the public. While Ljungqvist and Qian analyze voluntary information sharing, we study mandatory public disclosures of positions, which are different from voluntary information sharing in a number of ways. Voluntary information sharing is occasional, reflects only an individual investor's opinion, and can be costly to access because investors have to search through each arbitrageur's website. Mandatory disclosures organized by exchanges, conversely, are regular and frequent, reflect the overall view of a given stock, and are easier to locate by the investing public. Importantly, Ljungqvist and Qian examine whether the market reacts to voluntary disclosures; we instead focus on the broader efficiency implications of mandatory public disclosures.

B. Short Sellers' Holding Periods, Reward-to-Risk Ratios, and Activity

Short sellers may face important horizon risks, such as the risk that a mispricing can take too long to correct, and thus potential profits are eroded by accumulating transaction costs, or the risk that the mispricing worsens in the short run because of noisy trading activity (Barberis and Thaler (2003)). As argued by the seminal articles of Dow and Gorton (1994) and Abreu and Brunnermeier (2002), horizon risk can discourage arbitrage activity. If short sellers' information is more quickly incorporated into prices with the new disclosure regime, then we would expect a decline in the holding periods of short sellers and an increase in their rewards. Furthermore, if limits to arbitrage arising from horizon risk are mitigated with the new disclosure rules, then we also expect to see an increase in the amount of short selling. In this section, we examine these hypotheses.

We start by measuring the holding periods of short sellers' positions using data from Information Handling Services (IHS) Markit. IHS Markit reports the weighted-average number of (calendar) days that transactions have been open. We use data from July 3, 2006, onward, the date on which IHS Markit commenced reporting data at a daily frequency. We take the average of all loans for a stock between 2 consecutive short-interest-announcement days and run the following regression:

$$(3) \quad \text{LOAN\_LENGTH}_{i,t+1} = \alpha_i + \theta_0 e_{i,t} + \theta_1 \text{POST}_{i,t} + \theta_2 [e \times \text{POST}]_{i,t} + \lambda X_{i,t} + \varepsilon_{i,t},$$

where LOAN_LENGTH is the average loan tenure for a stock after a short-interest announcement and prior to the next short-interest announcement (including both actual and placebo announcements). In an extended specification, we also include control variables for stock characteristics that might be related to short sellers' holding periods (e.g., stock's market capitalization, BM ratio, idiosyncratic volatility, past cumulative monthly returns, and illiquidity) as well as stock fixed effects. If the regulatory amendments hasten the speed with which information is impounded into prices, then the holding periods of short sellers' positions would be reduced in the postamendment period. Specifically, we would observe $\theta_0 > 0$ and $\theta_2 < 0$. This is precisely what we find. In Table 8, we find that e is 9.6, whereas $e \times$ POST is −9.8, and both are statistically significant. Similar to the main results on price efficiency, there is a complete sign-flipping pattern in the estimates, providing strong support for the hypothesis. Short sellers have a holding period of (approximately) 80 calendar days for a typical stock; thus, the estimates correspond to an approximate 9%–12% change in short sellers' holding periods.

We next analyze the impact of the regulatory amendments on the reward-to-risk ratios of short sellers' positions. If short sellers' information is impounded into prices more readily with the regulatory amendments, then short sellers would be able to earn returns to their information more reliably. We test this prediction using the IHS Markit database because it allows us to observe short positions on both actual and placebo report dates; short interest from Compustat is what is disclosed to the public, so this allows us to observe short interest only on actual report dates.

On each REP_DATE (including the placebo one), for each stock, we first calculate the change in short interest (in IHS Markit) from the previous REP_DATE. Short interest is the daily total short positions in a given stock divided by the stock's shares outstanding. Based on changes in short interest, we form 10 portfolios and hold these portfolios until the next REP_DATE (approximately 15 calendar days). We then pool the daily portfolio returns pertaining to each into 4 cases, POST = 0 and $e = 0$, POST = 0 and $e = 1$, POST = 1 and $e = 0$, and POST = 1 and $e = 1$, and run the 4-factor model for each POST and e using the time series of portfolio returns. Table 9 reports the reward-to-risk ratios (4-factor alpha divided by its standard error) for each of the 4 cases. The results indicate that with the

TABLE 8
Short Sellers' Holding Periods

Table 8 presents the regression results of the impact of the regulatory amendments on short sellers' holding periods. The table presents the regression results where the dependent variable, LOAN_LENGTH, is the average loan tenure (in calendar days) for short-sale positions after the current REP_DATE and before the next REP_DATE. LOAN_LENGTH is calculated using the daily Information Handling Services (IHS) Markit data available from July 2006. The explanatory variables are as follows: POST is a dummy variable that equals 1 for observations in the postamendment period, and 0 otherwise; *e* is a dummy variable that equals 1 when LOAN_LENGTH is calculated after the end-of-month REP_DATE and before the mid-month REP_DATE of the following month, and 0 otherwise; and POST × *e* is an interaction term between POST and *e*. In column 2, we include the following control variables: idiosyncratic volatility (IVOL), the stock's market capitalization (SIZE), book-to-market ratio (BM), past cumulative monthly returns (PAST_RETURNS), illiquidity (ILLIQ), and stock fixed effects (FE). Further details regarding the definition of control variables can be found in the Appendix. All regressions include time (year, month-of-year, and day-of-week) FE. We also include a constant term in all regression specifications but suppress it for reporting purposes. We present ordinary least squares estimates with standard errors (in parentheses) double-clustered by stock and short-interest-announcement days. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	LOAN_LENGTH	
Variables	1	2
POST × *e*	−9.8280***	−9.0411***
	(2.9838)	(3.2552)
POST	3.2377***	2.1909**
	(0.7576)	(0.8690)
e	9.6129***	8.7815***
	(2.9862)	(3.2575)
SIZE		−0.0012***
		(0.0002)
IVOL		−0.5024***
		(0.0843)
ILLIQ		−0.0275
		(0.0358)
BM		4.6395***
		(0.6222)
PAST_RETURNS		1.0262
		(0.7028)
No. of obs.	382,612	306,198
R^2	0.028	0.039
Controls	No	Yes
Time FE	Yes	Yes
Stock FE	No	Yes

new reporting regime, short sellers earn higher reward-to-risk ratios in the days following the short-interest announcement. Long–short portfolios formed after the end-of-month REP_DATE in the postamendment period (POST = 1 and $e = 1$) have a reward-to-risk ratio of 2.5, whereas portfolios formed after the placebo end-of-month REP_DATE in the preamendment period (POST = 0 and $e = 1$) have a reward-to-risk ratio of 1.54. Consistent with the hypothesis, this difference is mostly driven by stocks that are heavily shorted.[13]

Finally, in addition to examining short sellers' holdings periods and reward-to-risk ratios, we ask whether the amount of short selling is also affected after the rule amendments. We expect that after the regulatory amendments, as a result of declines in horizon risk, short sellers might be more willing to take positions. To examine this, we run the regression in equation (3) using ΔSHORT as the dependent variable. It is defined as the change in short interest (reported by IHS Markit) scaled by the stock's shares outstanding, and it is calculated after REP_DATE and before the next REP_DATE. If short sellers are more active after the regulatory

[13]Table IA.7 in the Supplementary Material reports the reward-to-risk ratios (along with alphas and standard errors) for all decile portfolios. Our conclusions hold throughout.

TABLE 9

Reward-to-Risk Ratios of Short Sellers' Positions

Table 9 presents the impact of the regulatory amendments on the reward-to-risk ratios of short-sellers' position changes, using the Information Handling Services (his) Markit data starting from July 2006. Starting from July 2006, IHS Markit reports the daily total short positions taken by the universe of market participants that it covers. On each REP_DATE (including the placebo one), for each stock, we first calculate the change in short interest (in IHS Markit) from the previous REP_DATE. Short interest is the daily total short positions in a given stock divided by the stock's shares outstanding. Based on changes in short interest, we form 10 portfolios and hold these portfolios until the next REP_DATE (approximately 15 calendar days). We then pool the daily portfolio returns pertaining to each of 4 cases, POST = 0 and $e=0$, POST = 0 and $e=1$, POST = 1 and $e=0$, and POST = 1 and $e=1$, and run the 4-factor model for each POST and e using the time series of portfolio returns. From this procedure, we estimate the 4-factor alphas along with the standard errors (Newey–West standard errors with 5 lags, reported in parentheses). The table reports the reward-to-risk ratios, defined as the 4-factor alpha divided by its standard error, for each of the 4 cases. The bottom-decile portfolio (P1) has a ΔSHORT below the 10th percentile, and the top-decile portfolio (P10) has a ΔSHORT above the 90th percentile; P1 – P10 is the spread between the two portfolios.

	e = 0		e = 1	
POST = 0	P1	1.1857	P1	1.2400
	P10	−1.9921	P10	−1.6000
	P1 – P10	1.8453	P1 – P10	1.5370
POST = 1	P1	1.2361	P1	1.2051
	P10	−2.0381	P10	−2.4476
	P1 – P10	2.0897	P1 – P10	2.4894

amendments, this would result in $\theta_0 < 0$ and $\theta_2 > 0$. Table 10 shows results that are consistent with these predictions across all specifications. There is a significant increase in the amount of short selling after the regulatory amendments. Overall, these results provide evidence that higher public disclosure of short interest has important implications. The regulatory amendments reduce short sellers' holding periods, assist short sellers in obtaining better rewards, and increase short-selling activity.

C. Does the Market React to Short-Interest Announcements?

In Section III, we show that increasing the frequency of short-interest disclosure improves price informativeness. If this result is driven by the mechanism that with greater disclosure, the wider investing public learns about short sellers' private information more promptly, then it should be that short-interest announcements reveal new information to which the market reacts.

Although short interest tends to be persistent, existing articles in the literature show that changes in short interest contain important information about future company news and subsequent stock returns. A number of studies provide insights on the nature of the information that a change in short interest holds. For instance, there is a significant increase in short interest for stocks that newly enter anomaly portfolios (Hanson and Sunderam (2013), Daniel, Klos, and Rottke (2017)). Moreover, short interest seems to increase substantially prior to the release of negative earnings announcements (Boehmer et al. (2020), Christophe et al. (2004)).

Therefore, we examine market reactions to changes in short interest between two successive short-interest announcements. We use short interest from Compustat because this is precisely the short interest that is disseminated to the public. If the investing public is already gathering information on short-selling activity from alternative sources (e.g., through access to proprietary data sets or informal contacts with brokers), then short-interest announcements by exchanges would

TABLE 10
Amount of Short Selling

Table 10 presents the impact of the regulatory amendments on the amount of short selling. The dependent variable is ΔSHORT_MARKIT, which is the percentage change in the total short positions reported by Information Handling Services (IHS) Markit between two consecutive REP_DATEs (including the placebo REP_DATE), scaled by the stock's shares outstanding. ΔSHORT_MARKIT is calculated using the daily IHS Markit data available from July 2006. The explanatory variables are as follows: POST is a dummy variable that equals 1 for observations in the postamendment period, and 0 otherwise; *e* is a dummy variable that equals 1 when ΔSHORT_MARKIT is calculated after the end-of-month REP_DATE and before the mid-month REP_DATE of the following month, and 0 otherwise; and POST × *e* is an interaction term between POST and *e*. In column 2, we include the following control variables: idiosyncratic volatility (IVOL), the stock's market capitalization (SIZE), book-to-market ratio (BM), past cumulative monthly returns (PAST_RETURNS), illiquidity (ILLIQ), and stock fixed effects (FE). Further details regarding the definition of control variables can be found in the Appendix. All regressions include time (year, month-of-year, and day-of-week) FE. We also include a constant term in all regression specifications but suppress it for reporting purposes. We present ordinary least squares estimates with standard errors (in parentheses) double-clustered by stock and short-interest-announcement days; *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	ΔSHORT_MARKIT	
Variables	1	2
POST × *e*	0.1582***	0.1632***
	(0.0427)	(0.0482)
POST	−0.1205	−0.1271
	(0.0734)	(0.0826)
e	−0.1244***	−0.1282***
	(0.0394)	(0.0448)
SIZE		−0.0000***
		(0.0000)
IVOL		−0.0006
		(0.0007)
ILLIQ		0.0006***
		(0.0002)
BM		−0.0193***
		(0.0054)
PAST_RETURNS		0.0035
		(0.0137)
No. of obs.	345,458	261,958
R^2	0.008	0.009
Controls	No	Yes
Time FE	Yes	Yes
Stock FE	No	Yes

not matter, and thus we would not find significant price reactions to changes in short interest.[14]

In Panel A of Table IA.8 in the Supplementary Material, we find a significant negative relationship between changes in short interest and the average 2-day announcement returns.[15] A strategy that buys the stocks in the bottom portfolio and sells the stocks in the top portfolio earns an average daily 4-factor alpha of 15 bps. There is a monotonic pattern across portfolios, with the statistical significance being the strongest for the top- and bottom-decile portfolios, as one would

[14]Previously, Senchack and Starks (1993) have studied market reactions to short-interest announcements from 1980 to 1986. We reconduct this analysis during our sample period because market reactions to short-interest announcements might be different in more recent periods, for instance, as a result of the availability of more information on short-selling activity. Also, we can overcome the data limitations; whereas Senchack and Starks (1993) were able to hand-collect data on short interest for only a group of stocks, we can observe this for all Compustat firms.

[15]For compatibility with panel regressions where each observation is equally weighted, the portfolio results that we report use equal-weighted returns. Our findings are similar with value-weighted returns.

expect. Because short interest conveys pessimistic information, the price reactions (in absolute terms) are the largest for the top-decile portfolio.

Furthermore, we test whether market reactions to short-interest announcements are different in the pre- and postamendment periods. Although the average price reaction is significant during our sample period, it might be that this is mostly driven by the preamendment period if alternative ways to acquire information on short sales have become more widely available in the postamendment period. We find that this is not the case. Panel B of Table IA.8 in the Supplementary Material shows that price adjustments are, if anything, larger (approximately doubled) in the postamendment period, suggesting that short interest is arguably more informative in the postamendment period. These findings reveal that short-interest announcements matter despite the availability of possible alternative channels (perhaps as a result of alternative channels being costly or not providing complete information).

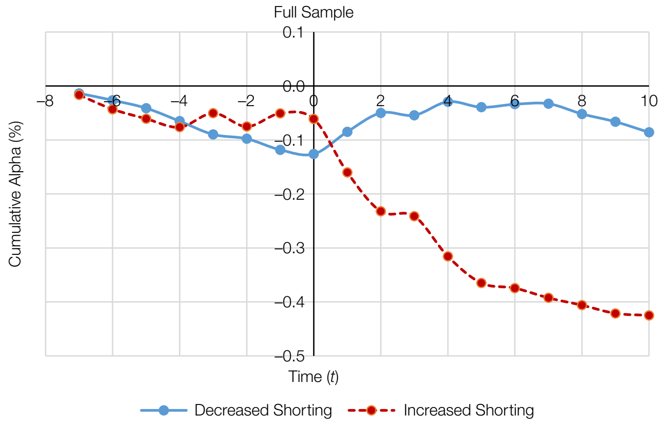
Next, we assess whether the market may view mid-month and end-of-month short-interest announcements differently. Because end-of-month announcements are made only in the postamendment period (and there are significant differences in reactions between the pre- and postamendment periods), for a more meaningful analysis, we compare the differences in market reactions to mid-month and end-of-month short-interest announcements in the postamendment period. The results show no indication of differential reactions to mid-month versus end-of-month announcements (Panel C of Table IA.8 in the Supplementary Material).

Figure 2 plots the cumulative 4-factor alphas for the top- and bottom-decile portfolios starting from 7 trading days prior to the short-interest announcements.

FIGURE 2

Market Reactions to Short-Interest Announcements in the Full Sample

Figure 2 presents the price reactions to short-interest announcements. On each announcement date, we form 10 portfolios based on ΔSHORT, which is the change in short interest between two successive short-interest announcements, scaled by the stock's shares outstanding. The bottom-decile ("Decreased Shorting") portfolio has a ΔSHORT below the 10th percentile, and the top-decile ("Increased Shorting") portfolio has a ΔSHORT above the 90th percentile. In this figure, we show the cumulative 4-factor alphas (in %), starting from 7 trading days prior to the short-interest announcements and continuing until 10 trading days after the short-interest announcements. Short interest is publicly disclosed after 4:00PM at $t = 0$.



We don't observe any noticeable pattern in alphas before the short-interest announcements, suggesting that there is no significant front running. In Table IA.9 of the Supplementary Material, we conduct formal tests to confirm this. Table IA.10 in the Supplementary Material extends the analysis to the measures of trading activity prior to the announcements. In Panels A–F, we use turnover, volatility, bid–ask spread, short interest, number of loans, and loan concentration, respectively. The results are mostly insignificant, although there are some weak effects in turnover and volatility. Taken together with Table IA.9 in the Supplementary Material, there seems to be some degree of trading activity prior to the announcements; however, it isn't significant enough to generate an important price impact.[16]

Finally, we check whether there is an overreaction to short-interest announcements, which may occur if investors believe that short interest is more informative than it actually is or if abusive short sellers use public announcements to manipulate other market participants' beliefs. The prior literature has documented limited evidence for manipulation, and the evidence that has been found has been concentrated around seasoned equity offerings (Henry and Koski (2010)). If investors overreact to short-interest announcements, we would expect to find return reversals. In Figure IA.1 in the Supplementary Material, we show the cumulative 4 alphas over the next 60 trading days after the announcement date for the top-decile portfolio. We conduct subsample tests repeating this analysis i) for small stocks, which might be more susceptible to manipulation because they don't have enough liquidity, and ii) for growth stocks, which tend to have high short-selling activity. We do not find any reversals in any of the samples that we study.

D. Cross-Sectional Evidence

In this section, we analyze whether there are cross-sectional differences in the impact of the new disclosure regime on price efficiency. We start by examining the role of arbitrage risk. We measure arbitrage risk in a number of ways. First, we follow Engelberg, Reed, and Ringgenberg (2018) and calculate FEE_RISK, which is defined as the standard deviation in a stock's loan fees in a given month. In addition, following Stambaugh, Yu, and Yuan (2015), we use idiosyncratic volatility (IVOL) as a measure of risky arbitrage. High idiosyncratic volatility can cause adverse price movements and therefore lead to early liquidation risks. In a similar vein, we introduce a measure of noise-trading activity, RETAIL_TRADING, which equals 1 when the stock's institutional ownership is low and the stock has a high trading activity, and 0 otherwise. If the new disclosure regime helps short sellers overcome the limits to arbitrage, then we expect the effects to be pronounced for stocks with higher arbitrage risk.

Next, we test whether the main results depend on whether the earnings announcement was a negative or positive surprise. If more frequent disclosure of short interest helps investors promptly learn about short sellers' private information (which contains negative information), we would expect the results to be pronounced for stocks with negative information. To test this idea, we define

[16]As for the levels of short interest, the top portfolio has higher short interest than the bottom portfolio, consistent with the fact that the top-decile (bottom-decile) portfolio includes stocks with recent large increases (decreases) in short interest.

NEG_NEW, which is a dummy variable that equals 1 if the firm's earnings surprise is negative, and 0 otherwise.

For each of these variables, we introduce triple differences, and we include all lower-level interaction terms in the empirical specification. The results are reported in Table 11. The main variables of interest are the coefficients on the triple-interaction terms. The findings are quite useful in that we consistently find that the results are pronounced for stocks with higher arbitrage risk. Through columns 1–3, we find that the estimates are nearly doubled for stocks with higher arbitrage risk, providing strong support for the mechanism. In the final column, we observe that whereas $e \times$ POST is -0.0014 and statistically significant at the 10% level, $e \times$ POST $\times$ NEG_NEW is -0.0021 and statistically significant at the 5% level. This shows that greater disclosure of short interest particularly helps with

TABLE 11
Cross-Sectional Differences

Table 11 presents the cross-sectional differences in the regression results reported in Table 2. In column 1, we introduce interaction terms with HIGH_FEE_RISK, a dummy variable that equals 1 when the firm's FEE_RISK is above the sample median, and 0 otherwise; in column 2, we introduce interaction terms with HIGH_IVOL, a dummy variable that equals 1 when the firm's idiosyncratic volatility is above the sample median, and 0 otherwise; in column 3, we introduce interaction terms with HIGH_RETAIL_TRADING, a dummy variable that equals 1 when the firm's IO is below the sample median and its TRADING_ACTIVITY is above the sample median, and 0 otherwise; and in column 4, we introduce interaction terms with NEG_NEW, which equals 1 if the firm's earnings surprise is negative, and 0 otherwise. Variable definitions are provided in the Appendix. The dependent variable is CAR[0,1], which is the absolute value of 2-day cumulative abnormal returns in the [0,1] days around the earnings announcement, defined as the difference between the buy-and-hold returns of the stock and beta multiplied by the buy-and-hold return of the market. POST is a dummy variable that equals 1 for the firm's earnings-announcement dates after Sept. 7, 2007, and 0 otherwise, and *e* is a dummy variable that equals 1 when the firm's earnings announcement occurs after the end-of-month REP_DATE and before the mid-month REP_DATE of the following month, and 0 otherwise. The explanatory variables include all interaction terms between POST, *e*, and CHAR, which refers to the stock characteristics explained previously. All regressions include the control variables NUM_EST, IO, FORECAST_ERROR, EARNINGS_PERSIST, EARNINGS_VOL, and NUM_ANN and industry and time (year, month-of-year, and day-of-week) fixed effects (FE). We also include a constant term in all regression specifications but suppress it for reporting purposes. We present ordinary least squares estimates with standard errors (in parentheses) double-clustered by stock and earnings-announcement day; *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	HIGH_FEE_RISK	HIGH_IVOL	HIGH_RETAIL_ TRADING	NEG_NEW
Variables	1	2	3	4
POST × *e*	−0.0015*	−0.0012*	−0.0018*	−0.0014*
	(0.0008)	(0.0007)	(0.0010)	(0.0008)
POST × *e* × CHAR	−0.0010**	−0.0007*	−0.0014**	−0.0021**
	(0.0005)	(0.0004)	(0.0007)	(0.0010)
e × CHAR	0.0007*	0.0001	0.0013*	0.0014**
	(0.0004)	(0.0016)	(0.0007)	(0.0007)
POST × CHAR	0.0016	−0.0045**	−0.0026	0.0022**
	(0.0020)	(0.0018)	(0.0021)	(0.0011)
CHAR	0.0029**	0.0175***	0.0100***	0.0012*
	(0.0013)	(0.0014)	(0.0015)	(0.0007)
POST	0.0083***	0.0127***	0.0115***	0.0116***
	(0.0022)	(0.0020)	(0.0019)	(0.0019)
e	0.0026***	0.0030***	0.0029***	0.0031***
	(0.0009)	(0.0007)	(0.0007)	(0.0006)
No. of obs.	42,294	56,255	59,019	59,020
R^2	0.135	0.143	0.125	0.122
Controls	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes

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the diffusion of negative information, which tends to travel slowly (Hong, Lim, and Stein (2000)).[17]

IV. Conclusion

In this article, we investigate the effect that greater disclosure of arbitrage activity and informed trading has on price efficiency. To answer this question, we study the shorting market and exploit SEC-approved amendments to exchange rules, which increased the frequency of public disclosure of short positions. Greater public disclosure can potentially have both costs and benefits; thus, the impact it has on price efficiency, a priori, is not immediately obvious. On the one hand, greater disclosure may hurt the production of information if it reduces the ability of arbitrageurs to profit from their information. On the other hand, disclosure can be beneficial because it can help arbitrageurs overcome the limits to arbitrage arising from horizon risk.

We estimate the changes to price efficiency with more frequent reporting of short interest using an identification strategy that relies on placebo dates, dates when short interest would have been publicly reported had broker-dealers been required to report short-interest positions at the end of the month in the preamendment period. Our findings indicate that the new reporting regime has an important impact on a stock's informational environment. Information encapsulated within short interest is more quickly incorporated into prices, thereby increasing price informativeness. In extended analyses, we find that greater short-interest disclosure also reduces short sellers' holding periods and increases the amount of short selling.

Our work has implications for regulatory policy regarding short-selling public disclosure and, more broadly, the public disclosure of private information. Whereas in the EU, regulations requiring the immediate disclosure of short positions have discouraged short selling and hampered price efficiency, we find that bimonthly disclosure of short positions in the United States can ameliorate the negative consequences associated with greater publicity. Regulatory policies should consider both the potential costs and the potential benefits of greater public-disclosure requirements imposed on arbitrageurs and informed trading. Public-disclosure requirements should aim to maximize the benefits by providing enough time and flexibility to traders to execute their trades and build their positions. If requirements are designed in this way, the potential costs associated with distorting incentives to produce private information can be mitigated, and public-disclosure requirements can therefore foster price efficiency.

Appendix. Definition of Variables

#8K_FILINGS: Total number of 8-K filings per firm during the period between a short-interest announcement and the previous one plus the number of 8-K filings that were reported after the most recent short-interest announcement prior to the

[17]Although the effects are small, the market reacts positively to decreases in short interest (Table IA.8 in the Supplementary Material). Consistent with this, in column 4 of Table 11, we observe that there are also small efficiency gains for stocks that do not have negative earnings surprises.

earnings-announcement date. For observations with POST = 0 and $e=1$, it is defined using the placebo short-interest-announcement date. *Source:* SEC EDGAR, Compustat.

BM: Book equity in June of calendar year t divided by market equity in December of the previous calendar year $t-1$. *Source:* CRSP, Compustat.

CAR[0,1]: Calculated two ways: i) absolute value of the difference between the buy-and-hold returns of the stock over $[0,1]$ and beta multiplied by the buy-and-hold return of the market over $[0,1]$; ii) absolute value of the difference between buy-and-hold returns of the stock over $[0,1]$ and that of a size- and BM-matched portfolio over $[0,1]$. The beta used in the first method is estimated by regressing daily stock returns on daily market returns using a $[t-300, t-46]$ window, where t is the date of the earnings announcement. *Source:* CRSP, Fama and French (1993).

CAR[2,61]: Calculated two ways: i) absolute value of the difference between buy-and-hold returns of the stock over $[2,61]$ and beta multiplied by the buy-and-hold return of the market over $[2,61]$; ii) absolute value of the difference between buy-and-hold returns of the stock over $[2,61]$ and that of a size- and BM-matched portfolio over $[2,61]$. The beta used in the first method is estimated by regressing daily stock returns on daily market returns using a $[t-300, t-46]$ window, where t is the date of the earnings announcement. *Source:* CRSP, Fama and French (1993).

DAYS_SINCE: Number of trading days between an earnings-announcement date and the last short-interest announcement prior to it. For observations with POST = 0 and $e=1$, it is calculated as days since the last placebo short-interest announcement. *Source:* IBES, Compustat.

DELAY1: For a given POST and e, using daily stock data between consecutive REP_DATEs, we first run the following regression for each stock:

$$r_{j,t} \quad = \quad \alpha + \beta R_{m,t} + \sum_{n=1}^{4} \delta^{(-n)} R_{m,t-n} + \varepsilon_{j,t},$$

where $r_{j,t}$ is the stock's return in week t, and $R_{m,t}$ is the return on the CRSP value-weighted market index in week t. We then calculate DELAY1 between REP_DATEs as follows:

$$\text{DELAY1} \quad = \quad 1 - \frac{R^2_{\delta^{(-n)}=0, \forall n \in [1,4]}}{R^2},$$

where $R^2_{\delta^{(-n)}=0, \forall n \in [1,4]}$ is the R^2 from the previous regression where all the coefficients on $\delta^{(-n)}$ are restricted to 0 and is divided by the R^2 from the previous regression with no restrictions. *Source*: CRSP.

DELAY1_NEG: Calculated using the same method as DELAY1, except we only use negative market returns in the estimation (positive market returns are set to equal 0). *Source:* CRSP.

DELAY3: Coefficient estimates are first calculated using the regression from DELAY1. Next, we calculate DELAY3 between REP_DATEs as follows:

$$\text{DELAY3} \quad = \quad \frac{\sum_{n=1}^{4} n\delta^{(-n)} / \text{se}\left(\delta^{(-n)}\right)}{\beta / \text{se}(\beta) + \sum_{n=1}^{4} \delta^{(-n)} / \text{se}\left(\delta^{(-n)}\right)},$$

where se(.) is the standard error of the coefficient estimate. *Source:* CRSP.

e: Dummy variable that equals 1 for observations after the end-of-month REP_DATE and before the mid-month REP_DATE of the following month, and 0 otherwise. *Source:* Compustat.

EARNINGS_PERSIST: First-order autocorrelation coefficient of quarterly EPS during the past 4 years. *Source:* IBES, Compustat.

EARNINGS_VOL: Standard deviation of quarterly EPS in the past 4 years. *Source:* IBES, Compustat.

FORECAST_ERROR: Absolute value of the difference between the announced earnings and the consensus EPS forecast normalized by the firm's stock price at the end of the corresponding quarter. The consensus EPS forecast is calculated as in Hirshleifer et al. (2009). *Source:* IBES, Compustat, CRSP.

FEE_RISK: Standard deviation of loan fees (for a stock) in the previous month. *Source:* IHS Markit.

HIGH_DAYS_SINCE: Dummy variable that equals 1 when the firm's DAYS_SINCE is above the sample median, and 0 otherwise.

HIGH_FEE_RISK: Dummy variable that equals 1 when the firm's FEE_RISK is above the sample median, and 0 otherwise.

HIGH_RETAIL_TRADING: Dummy variable that equals 1 when the firm's IO is below the sample median and its TRADING_ACTIVITY is above the sample median, and 0 otherwise.

ILLIQ: Average ratio of the absolute value of daily returns to the stock daily volume in the past 6 months, as in Amihud (2002). *Source:* CRSP.

IO: Fraction of all shares outstanding held by institutional investors for a given stock at the end of the quarter (in %). *Source:* Thomson Reuters.

IVOL: Standard deviation of idiosyncratic monthly returns over the past 2-year window (in %), where idiosyncratic monthly returns are the residuals in a regression of a stock's monthly return on the three Fama and French (1993) factors. *Source:* CRSP, Fama and French (1993).

LOAN_LENGTH: Average loan tenure for short-sale positions after each REP_DATE and before the next REP_DATE. *Source:* IHS Markit.

NEG_NEW: Dummy variable that equals 1 if the firm's earnings surprise is negative, and 0 otherwise. *Source:* Compustat, IBES.

NUM_ANN: Total number of earnings announcements by other firms on the day of a firm's own earnings announcement. *Source:* IBES.

NUM_EST: Natural logarithm of 1 plus the number of analysts giving EPS forecasts for the given firm in that quarter. *Source:* IBES.

PAST_RETURNS: Cumulative monthly returns over the past 6 months. *Source:* CRSP.

PE: Calculated for each stock on each trading day as follows:

$$\text{PE} = \frac{\sigma(s)}{\sigma(p)}$$

where $\sigma(s)$ is the standard deviation of the pricing error, which is assumed to follow a 0-mean, covariance-stationary process, and $\sigma(p)$ is the standard deviation of intraday transaction prices. We then calculate the average PE between REP_DATEs. *Source:* TAQ.

POST: Dummy variable that equals 1 for observations in the postamendment period (i.e., after Sept. 7, 2007), and 0 otherwise. *Source:* Compustat.

REP_DATE: Mid-month and end-of-month short-interest announcement dates, including the placebo REP_DATEs in the preamendment period. *Source:* Compustat.

ΔSHORT: Change in short interest between two successive short-interest announcement dates, scaled by the stock's shares outstanding (in %). In the preamendment period, it captures monthly changes; in the postamendment period, it is bimonthly changes. *Source:* CRSP, Compustat.

ΔSHORT_MARKIT: Change in short interest (in %) based on the universe of market participants covered by IHS Markit. It is calculated as the difference between two consecutive REP_DATEs (including the placebo REP_DATEs), scaled by the stock's shares outstanding. *Source:* IHS Markit, CRSP.

SIZE: Market capitalization of a stock, measured by price in month t multiplied by shares outstanding in month t ($millions). *Source:* CRSP.

SPREAD: Daily (%) average bid–ask spread over the [–4,–2] window before the earnings announcement. *Source:* CRSP.

TRADING_ACTIVITY: A stock's average turnover (volume divided by shares outstanding) in the previous month. *Source:* CRSP.

TURNOVER: Average daily trading volume in the [0,1] days around the earnings announcement divided by shares outstanding. *Source:* CRSP.

VAR_RATIO: Calculated for each stock on each trading day as follows:

$$\text{VAR\_RATIO} = \left|1 - \frac{\text{VAR(30MIN)}}{30 \times \text{VAR(1MIN)}}\right|,$$

where VAR(30MIN) is the variance of 30-minute returns, and VAR(1MIN) is the variance of 1-minute returns. We then calculate the average VAR_RATIO between REP_DATEs. *Source:* TAQ.

VOLATILITY: Difference between the highest and the lowest share prices over the [0,1] days around the earnings announcement, normalized by the average of the two. *Source:* CRSP.

Supplementary Material

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