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Two Essays on Short Sellers

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Two Essays on Short Sellers

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ABSTRACT

This dissertation examines the role of short sellers in our capital markets. The first chapter examines the impact constraints on short sellers have on the liquidity. Prior literature has struggled to cleanly identify whether constraining short sellers is harmful or not for liquidity. We exploit a plausibly exogenous shock to shorting supply that occurs on dividend record dates to test the relation between shorting constraints and market quality. This shock arises due to a combination of equity settlement rules and the tax treatment of the payments in lieu of dividends that stock lenders receive when they loan their shares. We find a temporary degradation in liquidity on dividend record dates in the form of larger effective spreads. Our evidence that liquidity deteriorates on dividend record days, especially in stocks that likely have less slack in lending supply, suggest shorting supply constraints affect the cost of transacting faced by all traders. The second chapter investigates whether sell side equity analysts use the trading activity of short sellers in their information set. By taking advantage of a lagged disclosure of short interest I can identify the relationship between analysts' actions and the trading of short sellers more directly than prior literature. I find that analysts exhibit an increased propensity to downgrade their recommendations for a stock after a disclosed increase in short selling. I also find a significantly positive relationship between changes in short interest and the likelihood of a downward EPS revision. Overall, these results suggest that market participants extract information from short-sellers' trading activity.

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INTRODUCTION

Short sellers serve an important role in our capital markets acting as informational intermediaries and they leave a noticeable footprint on today's equity markets. Boehmer, Jones, and Zhang (2008*b*) and Diether, Lee, and Werner (2009*b*) report that short sales account for one-quarter to one-third of all trading; Rapach, Ringgenberg, and Zhou (2016) show that average firm-level short interest has steadily increased over the past four decades to an average of about five percent of shares outstanding during the 2003-2014 period. The first chapter of this dissertation examines whether constraining short sellers' ability to transact will have an impact on liquidity costs. Theory offers two channels through which short selling frictions may affect liquidity. The first is competition. Increased competition drives down market maker rents and should facilitate liquidity improvements (Grossman and Miller, 1985). The second channel is adverse selection. Here, the effects are ambiguous. On the one hand, shorting constraints may remove informed traders from the market, reduce adverse selection, and improve liquidity. On the other hand, if the frictions are restrictions that disproportionately sideline the least sophisticated short sellers, adverse selection may increase (Diamond and Verrecchia, 1987). We exploit a plausibly exogenous shock to shorting supply and find a temporary degradation in liquidity. This finding suggests shorting supply constraints affect the cost of transacting faced by all traders.

The second chapter of this dissertation examines whether sell-side equity analysts use the trading activity of short sellers in their information set. Causal inferences on this relationship have been elusive in the literature as the actions of short sellers and analysts are typically endogenously determined. I take advantage of the lagged disclosure of short interest to isolate the response of analysts to the trade disclosure of short sellers. This allows for stronger causal inferences. I find that analysts do change their behavior after the disclosure of short interest. This finding suggests that analysts do extract information from the trading activity of short sellers.

CHAPTER I
CAUSAL EFFECTS OF SHORT-SELLING SUPPLY ON MARKET QUALITY

The first chapter is co-authored with Dr. Eric Kelley.

Abstract

We exploit a plausibly exogenous shock to shorting supply that occurs on dividend record dates to test the relation between shorting constraints and market quality. This shock arises due to a combination of equity settlement rules and the tax treatment of the payments in lieu of dividends that stock lenders receive when they loan their shares. Using ordinary dividend events between 2004 and 2016, we find a temporary degradation in liquidity on dividend record dates in the form of larger effective spreads. We find notably stronger effects for stocks with characteristics associated with less slack in equity lending supply, and for those paying larger dividends.

1. Introduction

By sheer volume alone, short sellers leave a noticeable footprint on today's equity markets. Boehmer, Jones, and Zhang (2008*b*) and Diether, Lee, and Werner (2009*b*) report that short sales account for one-quarter to one-third of all trading; Rapach, Ringgenberg, and Zhou (2016) show that average firm-level short interest has steadily increased over the past four decades to an average of about five percent of shares outstanding during the 2003-2014 period. Nevertheless, short selling remains costly and is in some cases impossible. Some impediments arise in equity lending markets, while others are imposed by regulators.[1] How do frictions to short selling affect liquidity and other dimensions of market quality? Answering this question with precise causal inferences is essential for regulators who shape policy as well as the growing number of asset managers who participate in equity lending.

Theory offers two channels through which short selling frictions may affect liquidity. The first is competition. When unconstrained, short sellers may act as liquidity providers in a manner similar to the traditional market-making sector, absorbing transient buying imbalances as they arise. Increased

[1] Kolasinski, Reed, and Finggenberg (2013) empirically study search frictions in the U.S. equity lending market. See Jones, 2012, for a historical discussion of various restrictions to short selling in the U.S.

competition drives down market maker rents and should facilitate liquidity improvements (Grossman and Miller, 1985). Thus, the competition channel predicts short-selling frictions will deteriorate liquidity. The second channel is adverse selection. Here, the effects are ambiguous. On the one hand, shorting constraints may remove informed traders from the market, reduce adverse selection, and improve liquidity. On the other hand, if the frictions are restrictions that disproportionately sideline the least sophisticated short sellers, adverse selection may increase (Diamond and Verrecchia, 1987).

An ideal experiment would test these theories by exogenously limiting short sales in a set of stocks and comparing market quality outcomes with those from similar control stocks. In this spirit, prominent work studies short-selling bans implemented around the 2008 financial crisis period. Boehmer, Jones, and Zhang (2013) and Beber and Pagano (2013) show that liquidity deteriorates, and volatility increases for banned stocks during the crisis. While these papers exploit well-defined and highly publicized shocks to the ability to short, causal interpretations are elusive. The short sale bans were applied to financial firms as endogenous outcomes of crises, events marked by changes to information asymmetry, stress on firms involved in market making, and overall policy uncertainty. Thus, skeptics are unconvinced the correlations documented by these authors imply causation. Outcomes of the SEC's RegSHO Pilot, which removed the uptick rule for a random set of firms in 2005, bolster this doubt. Alexander and Peterson (2008) and Diether, Lee, and Werner (2009*b*) associate the elimination of the ostensible barrier to shorting with slightly *wider* spreads for NYSE stocks. Boehmer, Jones, and Zhang (2008*a*) report similar increases in spreads in conjunction with the permanent repeal of the uptick rule in 2007.[2]

Kaplan, Moskowitz, and Sensoy (2013) create their own laboratory and mitigate endogeneity concerns. They work alongside a large money manager to increase lending supply for a random set of hard-to-borrow stocks while leaving unchanged the supply for a set of control firms. The resulting supply shock was substantial. For the average stock in the first (second) phase of the experiment, the money manager made available for lending shares totaling 18% (36%) of short interest. But interestingly, these authors find

[2] A number of papers associate shorting *activity* with market quality. Examples include Bris, Goetzmann, and Zhu (2007) and Boehmer and Wu (2012).

no significant stock price effect, nor do they detect any change in spreads, volatility, or skewness. Thus, the shock to lending supply had no bearing on a host of market quality measures. Viewing the literature as a whole, causal interpretations of the effect of shorting constraints on market quality are tenuous.

In this paper, we exploit a nuance in the United States tax code that, combined with equity settlement rules, offers a plausibly exogenous and transient shock to shorting supply. This shock occurs each time a firm pays a dividend – four times per year for most dividend-payers, so we need not rely on periods of market stress such as the financial crisis to study shorting constraints. Moreover, the shock falls on the dividend payment *date of record*, which occurs after the cum dividend and ex dividend dates. While the cum and ex dates attract dividend capture trading (Henry and Koski, 2017), the record date represents a day void of expected new information. Thus, dividend record dates facilitate study of the causal effects of lending supply. We detail the mechanics driving this shock in Section 1 below, and we provide evidence that lending fees indeed spike on dividend record dates in Section 3.a.

Our main results reveal a deterioration in liquidity around the supply shock. Using each firm-dividend event as its own control, we find that percentage and dollar effective spreads, which are common measures of liquidity, are about five percent greater on dividend record days than on nearby control days. This difference is highly statistically significant. The economic magnitude of this effect is roughly constant across the 2004-2007 and 2009-2016 subperiods as well. We find the late period result particularly interesting as the equity lending market has become larger, more transparent, and generally more accessible in recent years. For example, the Risk Management Association reports that in 2009 the value of lendable U.S. Equities, for fifteen large financial institutions, was $2.011 trillion, whereas, in 2017 that same value has grown to $7.394 trillion. We provide additional evidence the liquidity deterioration occurs through both a competition channel and an adverse selection channel. We also find very limited evidence that volatility and intraday variance ratio, a common measure of informational efficiency, increase marginally.

While our main liquidity result emerges primarily in stocks smaller than the median NYSE-listed firm (about $2 billion market capitalization), we find economically and statistically significant results in some samples of larger stocks as well. These stark size patterns are not surprising since equity lending supply is usually slack (D'Avolio, 2002; Kolasinski, Reed, and Ringgenberg, 2013) and the constraint likely does not bind in large cap stocks. When we apply even more stringent sample filters by only considering stocks that were hard-to-borrow around prior dividend events, the deterioration in market quality is generally larger. We also find modestly stronger results for high dividend paying stocks and in a much smaller sample of special dividends, cases which represent relatively greater tax-based disincentives to lend shares over the record day.

Our contribution rests squarely on the ability to identify meaningful exogenous variation in lendable supply. The existing body of work has understandably struggled to establish causality, and we highlight three important findings that offer credibility to our test design. First, our methodology assumes a temporary change in equity lending market conditions. Using daily equity lending fee data, we show fees indeed spike on dividend record dates and immediately fall back to pre-dividend levels (Figure 1). Second, this change to equity lending conditions manifests in a spike in equity delivery failures as well (Figure 2). Prior studies link failures to high loan fees (Evans, Geczy, Musto, and Reed, 2008), and Thornock (2013) shows failures increase around record dates in his 2005-2007 sample period. We show that Thornock's result also holds during the 2009-2016 period. Third, we utilize differential tax rates as the ultimate driver of the lending supply shock. Since this friction was non-existent prior to the 2003 U.S. tax code changes, we compare liquidity on dividend record dates during eighteen-month intervals before and after the change. Using each dividend-paying firm as its own control, we show that liquidity deteriorates on record dates *exclusively* in the post-tax change period.

Our overall message complements that of Blocher, Reed, and Van Wesep (2013), who also use dividend events as an exogenous shock to shorting supply. While our focus is on liquidity and other market quality metrics, they study price levels. Miller (1977) predicts that constraints in shorting supply combine with differences in opinion to drive overpricing. Blocher, Reed, and Van Wesep (2013) offer strong support of this theory. For stocks with binding constraints, they find abnormally positive returns leading up to the ex-dividend day and reversals thereafter. Together, our results suggest that constraints to shorting supply distort two related dimensions of financial markets: price levels and liquidity.

2. Institutional Background and Related Literature

A trader engaging in a short sale must locate and borrow shares from some other investor who already owns them. This equity loan is usually arranged through intermediaries, and it is priced according to the supply and demand of lendable shares in a fragmented and opaque market. Empirical researchers beginning with D'Avolio (2002) offer a peak into this market and document loan supply typically exceeds demand (i.e., supply is slack and therefore does not affect lending fees). However, in some cases, the supply constraint binds, lending fees are high (the stock is "on special"), and short selling is costly if not impossible. Moreover, lender market power emerges from search costs, which leads to higher and more disperse fees across lenders (Duffie, Garleneau, and Pedersen, 2002; Kolasinski, Reed, and Ringgenberg, 2013).

Binding constraints could arise when few owners have lending programs, which may be the case with very small stocks or stocks with low institutional ownership. Alternatively, regulators may impose temporary or permanent bans on short selling, which is tantamount to constraining supply to zero. As a result, some empirical studies use low institutional ownership as a proxy for constraints (e.g., Nagel, 2005; Asquith, Parteek, and Ritter, 2005); others study bans (e.g., Boehmer, Jones, and Zhang, 2013; Beber and Pagano, 2013).[3] The fact that short-selling constraints as well as proxies such as institutional ownership often arise endogenously presents a challenge to this research. Specifically, while prior work has developed a robust understanding of relationships between shorting constraints or activity and many other economic outcomes, strict causal inferences remain treacherous.

We are not the first to exploit the dividend payment mechanism as a shock to lending supply. Thornock (2013) recognizes this experimental setting and provides a thorough discussion of the underlying mechanics. He also offers numerous industry references suggesting lenders withdraw shares around dividend record dates. The setup contains three critical elements. First, in our sample period, equity transactions on day T settled on day $T+3$. As a result, the dividend record date, which establishes ownership

[3] Other examples include Kolasinski, Reed, and Thornock (2012) and Marsh and Payne (2012).

of the dividend, is typically three trading days after the dividend cum day, the final day one could purchase the share and have rights to receive the dividend. Second, the equity lending market has same-day settlement, so one who sells a stock short on a cum dividend day (*T*) needs not borrow the share until the record date (*T + 3*) to settle the short sale transaction.

The third element is driven by taxes. When a stock is on loan over the dividend record date, the borrower reimburses the lender the amount of the dividend since the buyer in the short transaction would be the legal shareholder of record. This "payment in lieu" of the dividend is taxable at the lender's marginal tax rate, which is possibly as high as 35%. Had the lender instead held the stock and received the dividend outright, the dividend could have been taxed at a qualified rate of 15% according to the Jobs and Growth Tax Relief Reconciliation Act of 2003. Thus, any tax-sensitive lender would have a disincentive to lend shares over a dividend record date, resulting in a potential dramatic contraction in lending supply. At the same time, there is no new expected information about a particular stock on its dividend record date, and any arbitrage dividend capture trading likely occurs on the cum-day and ex-day.[4]

A numerical example illustrates the tax effect. Consider an investor in the 35% marginal tax bracket who owns a 10,000 share position in a firm with a price of $31.87 and per share dividend payment of $0.13 (the median values from the early sample reported in Table 1 Panel A below). After the 2003 tax change, this dividend could qualify for a tax rate of 15%. The investor receiving the dividend outright would therefore owe $195 in taxes. If the investor had instead lent the shares, he would owe $455 in taxes on the same amount received as a payment in lieu of dividend. The tax differential of $260 represents an extra eight basis points of the position value. A loan fee adjustment that accounts for this differential would be an annualized increase of (8 bps x 252 days) = 20.5%. For this reason, lenders (or more likely their agents)

[4] Researchers at least as early as Pettit (1972) study informational effects on dividend announcement days. Prominent examples of cum-day and ex-day pricing studies are Elton and Gruber (1970), Lakonishok and Vermaelen (1986), and Michaely (1991). Henry and Koski (2017) analyze dividend capture strategies.

may increase their fees by a large margin or simply withhold shares from lending around dividend record dates. Either outcome reflects an exogenous and transient contraction in lending supply.[5]

Thornock (2013) examines dividend events from 2005 through 2007 and documents a number of interesting results. Using data from nine large equity lenders, he finds that lending fees spike and shares on loan fall around dividend record dates. These patterns are particularly strong for tax sensitive lenders, which are exactly those who have a disincentive to lend over record dates. He also finds the dispersion of loan fees across lenders and the likelihood of equity fails to deliver both increase, which are consistent with short sellers facing greater search costs (see also Kolasinski, Reed, and Ringgenberg, 2013) and loan fees (Evans, Geczy, Musto, and Reed, 2008).

3. Data

III.a. Sample

We gather dividend information, prices, market values, and returns from CRSP for all dividends with ex-days between January 1, 2004 and December 31, 2016. We restrict our sample to ordinary quarterly taxable cash dividends (CRSP distcd = 1232) of $0.01 or greater that are paid by ordinary common shares listed on the NYSE, NASDAQ, or AMEX exchanges. We also exclude events with stock prices below $5 or above $1,000 per share on the cum dividend date. The former mitigates concerns related to highly illiquid stocks and price discreteness; the latter avoids distortions in dollar spreads amongst very highly priced stocks. In addition, we drop events with other distributions on the same day as the dividend and those with stock splits during the 30 trading days prior to and following the ex-day. Finally, we require that the dividend record date be exactly two trading days after the ex-day.

We obtain equity loan fees and scores reflecting the cost to borrow from the Markit Securities Finance database, which provides daily stock-level measures for buy-side clients from July 2006 forward. Markit aggregates data from custodians and equity lending agents to cover a large fraction of the U.S.

[5] Lenders may also choose to recall their shares on loan. Such a recall event, combined with the inability of short sellers to easily locate alternative shares to borrow, may result in the forced covering of existing positions. We discuss the implications of recalls and forced covers in Section 3.d below.

market. Since one symptom of borrowing frictions is an abnormally high number of delivery failures (e.g., Evans, Geczy, Musto, and Reed, 2008), we supplement the loan fee data with equity fails-to-deliver statistics downloaded from the SEC's Freedom of Information Act website.

We obtain short interest (*SI*) data from Compustat. The U.S. stock exchanges report short interest in shares once per month (as of the 15^{th}) through August 2007 and twice per month (as of the 15^{th} and 30^{th}) thereafter. For consistency, we limit our analysis to the mid-month reports for the entire series. We normalize short interest by dividing the number of shares held short by shares outstanding from CRSP. Since some stocks are rarely if ever shorted under any conditions, we drop events where average short interest over the prior six months is below 0.1% of shares outstanding. Finally, we gather quarterly institutional ownership (*IO*) from Thomson Financial's 13F database, which we also scale by shares outstanding.

We consider two sample periods in our primary analysis. The first, which we label the "early period", is 2004 to 2007. Since the tax law underlying our experimental design was enacted in 2003, beginning our analysis in 2004 offers equity lenders and their agents ample time to make necessary adjustments to lending practices around dividends.[6] This sample contains 15,933 dividend events from 1,596 unique firms. We end the early period in 2007 because regulators imposed shorting bans, removed Rule 203 "locate" exemptions, and tightened close-out requirements on delivery failures (Rule 204T), all during the financial crisis period in 2008. Excluding 2008 from our analysis also alleviates general concern related to illiquidity and volatility in the crisis that might confound our inferences.

The second period, which we label the "late period", spans 2009 through 2016, and it contains 31,059 dividends paid by 1,757 unique firms. Changes to short selling rules enacted during the financial crisis are in full force during the late period. In addition, the late period coincides with a gradual increase in transparency in the equity lending market. For example, data providers such as Markit that offer short sellers near real-time information on the conditions of the lending market became more prevalent around

[6] In subsequent analysis, we utilize the events between July 1, 2001 and December 31, 2002 to compare market quality on record dates before and after the tax change.

this time. With the evolution of the securities lending market, the extent to which supply constraints bind and ultimately affect market quality may be reduced in the later period of our experiment.

III.b. Liquidity and other market quality measures

For each trading day in the thirty days leading up to ex-day through the thirty days after the record day, we calculate liquidity and other market quality measures from TAQ data. We use the monthly TAQ files from 2001 through 2010 and the daily files from 2011 through 2016, following Holden and Jacobsen's (2014) data filters and computational procedures.[7] Our key measure of liquidity is the percent effective spread,

$$ES\% = \frac{2|p_t - m_t|}{m_t}, \tag{1}$$

which is twice the absolute difference between the transaction price (p_t) and the prevailing quote midpoint (m_t) at the time of the trade *t*, all scaled by the quote midpoint. We also consider the dollar effective spread (*ES$*), which is simply the numerator of (1). We average spreads for each stock-day, using trade values as weights.

As is standard in the literature, we decompose effective spread into the realized spread (*RS*) and price impact (*PI*). We calculate realized spread as

$$RS\% = \frac{2BuySell(p_t - m_{t+k})}{m_t}, \tag{2}$$

where the *BuySell* indicator variable equals +1 (-1) for buyer-initiated (seller-initiated) trades signed according to the Lee and Ready (1991) algorithm. The difference between the transaction and some future

[7] Specifically, we require "normal" quote conditions (A, B, H, O, R, W), and we drop quotes that are cancelled or withdrawn, ask and bid =0 or missing, markets are locked or crossed markets, or bid-ask spread >$5. We delete any abnormal trades. If the NBBO has two quotes in same millisecond, we use the one that is last in sequence.

quote midpoint m_{t+k} represents the component of the spread that reverses and is a proxy for compensation for market making. We compute price impact as

$$PI\% = \frac{2BuySell(m_{t+k} - m_t)}{m_t}. \tag{3}$$

Because it measures the permanent price change associated with a trade, price impact also captures a dimension of liquidity. We consider both 30 seconds and 5 minutes as alternate values for k. Like *ES*, we compute both dollar and percentage measures of *RS* and *PI*, and we average each at the stock-day level using trade values as weights. We winsorize all spread variables each calendar day in our sample using the 5^{th} and 95^{th} percentiles.

We compute two additional market quality measures from TAQ data. Our estimate for volatility (*Range*) is the difference between the maximum and minimum intraday transaction prices, scaled by the day's volume-weighted average price. This intraday trading range estimate is also used by Boehmer, Jones, and Zhang (2013). If short sellers benefit market quality through liquidity provision, their presence may dampen volatility. Thus, an exogenous shorting constraint should be associated with greater volatility. Alternatively, short sellers who trade too aggressively or act as noise traders may increase volatility.

Our final market quality measure is based on intraday variance ratios. If stock prices follow a random walk, return variance should scale linearly with the return horizon. Thus, the variance of 30-minute returns should be twice the variance of 15-minute returns. We compute the variance ratio $VR(n,m)$ the ratio of the n-period return variance the m-period return variance, both divided by the length of the period. Deviations from unity reflect non-zero return autocovariances, so we use the absolute value of $1 - VR(n,m)$ in our analysis. If short sellers facilitate information incorporation, as opposed to creating noise, we expect the variance ratio to depart from one on dividend record dates.

III.c. Summary statistics

We present summary statistics in Table 1. We report the mean and standard deviation, along with the 5^{th}, 25^{th}, 50^{th}, 75^{th}, and 95^{th} percentiles separately for the early (Panel A) and late (Panel B) period samples. In the early period, dividends in our sample resemble those studied in the literature, with a median value of \$0.14 and annualized yield of (0.43 x 4 =) 1.72%. The median market capitalization for dividend paying firms is \$1.6 billion, and the interquartile range is about \$5 billion. The sample is comprised of fairly liquid stocks, with median effective dollar (percentage) spread of just over \$0.03 per share (0.10%) and cum-day price of \$32.01. Our spread values are comparable to those from Henry and Koski (2017), who report a median percentage spread of 0.15% in their study of NYSE-listed dividend payers in the 1999-2007 period.

Proxies for shorting constraints and activity suggest slack in the equity lending market. The median and interquartile range for fractional institutional ownership are 0.65 and 0.39; these statistics for short interest as a fraction of shares outstanding are 0.025 and 0.034. Moreover, the 10^{th} percentile of institutional ownership is 0.17, while the 90^{th} percentile of short interest is 0.08, so a rough proxy for shorting supply typically far exceeds shorting demand. The lending fee distributions (based on data from July 2006 forward) reinforce the view that lending supply is mostly slack. The fifteenth through seventy-fifth percentiles are roughly the same, and a meaningful spike occurs in the vicinity of the 90^{th} percentile. Thus, the vast majority of stocks are easy to borrow. This picture resembles the mostly flat lending supply curve traced by Kolasinski, Reed, and Ringgenberg (2013) using data from an alternative source.

Statistics for the late period are similar. Not surprisingly, market capitalization, institutional ownership, and short interest all increase moderately to median values of \$2 billion, 0.68, and 0.03, respectively. Median effective spreads are about the same, though the mean value rises from \$0.047 to \$0.080, reflecting wider dispersion in the right tale of the distribution in the later sample. This is consistent with Chordia, Roll, and Subrahmanyam (2011) who show that spreads bottomed out just prior to the financial crisis. Dividends increase slightly to a median value and annualized yield of \$0.17 and 2.16%, respectively. The distribution of equity lending fees resembles that from the early period as most stocks in the cross section remain inexpensive to borrow.

Table 2 presents correlations for the same set of variables, again with the early period in Panel A and the late period in Panel B. Noteworthy are the correlations between lending fee and the market capitalization and institutional ownership variables. Both are negative, consistent with the common use of either market capitalization or institutional ownership as (inverse) proxies for short selling constraints. Our main (inverse) market quality variables are negatively correlated with market capitalization and institutional ownership and positively correlated with lending fees.

4. Empirical Results

IV.a. Preliminary analysis of the equity lending market

We design our tests on the premise that the supply of shortable shares is temporarily constrained around dividend record dates; therefore, we conduct a preliminary analysis of the equity lending market. Figure 1 plots lending fees from Markit in event time around dividend record dates (labeled in blue). For perspective, the cum day is labeled in black and "day 0" represents the ex-day. Panel A shows that for the early period (beginning here in July 2006 with daily Markit data), lending fees spike exactly on the record day. Panel B shows little has changed in more recent years. The latter result is perhaps surprising given increased transparency and integration in the lending market. The figures suggest short sellers desiring to initiate positions on the record date will be more constrained in their ability to fulfill the locate requirement.

Abnormally high delivery failures occur when short sellers cannot locate and borrow shares for settlement (three days after they transact). In fact, Evans, Geczy, Musto, and Reed (2008) show that certain market makers actually *choose* delivery failure when lending fees are high. Thornock (2013) documents a record day spike in delivery failures using his sample of dividends from 2005 to 2007. We corroborate Thornock's findings over our longer time period. We plot in Figure 2 the fraction of firms with outstanding fails exceeding 10,000 shares in event time around the record date.[8] Panel A shows that for the early period, the fraction of firms with fails slowly rises through the cum day, spikes on the record date, and then falls sharply. About fourteen percent of firms have reported fails on the record date in contrast with about ten percent on the following day and ten to twelve percent on the days leading up to the cum day.

Panel B reveals a similar picture for the late sample, with the fraction of firms with fails spiking to near nine percent on the record day relative to four to six percent on days surrounding the event. In the late period, the incidence of failure is notably lower on all days for at least two reasons. First, exemptions to the "locate requirement" in Reg 203 were lifted in 2008. A more stringent locate requirement further curtailed the common practice of "naked shorting" and resulted in fewer delivery failures. Second, Reg 204T, enacted in 2008 and made permanent in 2009, required market makers to close out any delivery failures by the subsequent day, which resulted in fewer outstanding fails at any given time.

IV.b. Main Analysis

We now turn to our main analysis. The literature documents a number of variables that explain liquidity and other market quality measures in the cross section. Well-known determinants are firm market capitalization, trading volume, listing exchange, volatility, and stock price (see, *e.g.*, the matching procedure of Boehmer, Jones, and Zhang, 2013). Rather than controlling for each in a regression that imposes a specific functional form, we take advantage of the fact that each of these variables is quite persistent for a given firm over the days surrounding a dividend payment and estimate a simple fixed effects regression.

Our principal specification is a pooled regression with event day dummies and firm-dividend fixed effects:

[8] Prior to September 16, 2008, only securities with a fails balance exceeding 10,000 were reported, while all securities were reported thereafter. To achieve meaningful comparisons with the early period, we use the reported data in the late period to compute the fraction of securities with fails exceeding 10,000 shares.

$$Y_{it} = \alpha + \sum_{\Delta=-5}^{5} \beta^{T+\Delta} D_{it}^{T+\Delta} + \text{event fixed effects} + e_{it}. \tag{4}$$

where the dependent variable Y_{it} refers to a market quality metric. We note each of our dependent variables is an *inverse* measure such that smaller values represent greater market quality. The model uses daily stock-level observations for days –30 through +30 relative to the ex-day. The event day dummies D_{it} estimate separately the effects of each day in the $[T - 5, T + 5]$ window around the ex-day, and the intercept captures event days in a reasonable benchmark period [–30, –6] and [+6, +30]. Our primary interest is the record day dummy D^{T+2}, to which we refer in the tables as *RecDay* for readability. If shorting supply constraints harm market quality, the estimated coefficient on *RecDay* will be positive. By including firm-dividend fixed effects, a firm becomes its own control around each dividend. That is, the effect of any firm-level determinant that is fixed over the (– 30, +30)-day window around a specific dividend is captured by these dummies. This specification is more flexible and arguably more reasonable than a *firm* fixed effect specification which would hold fixed any unobserved firm effect over the course of the entire sample. Nevertheless, we re-estimate our models using firm effects and our results are similar to those we report here.

We intend to exploit a binding supply constraint on the dividend record date. However, prior research shows that lending supply is slack for most stocks on any given day. For example, D'Avolio (2002) finds in his sample that less than ten percent of stocks are on special, and Asquith, Parteek, and Ritter (2005) show that about 95% of publicly traded stocks have greater institutional ownership (a proxy for lending supply) in excess of short interest. Using more recent data covering twelve equity lenders, Kolasinski, Reed, and Ringgenberg (2013) estimate the lending supply curve is mostly flat up until quantity exceeds about the 75^{th} percentile of shares outstanding at which point stocks become special. As a result, any tests using the full sample of dividend events likely lack sufficient power to detect a meaningful effect.

For most of our analysis that follows, we restrict the sample to dividend events in which the supply constraint more likely binds on the record date (*e.g.*, the upward sloping region of the lending supply curve).[9] D'Avolio (2002) and many others show that smaller stocks are more difficult to borrow, and the negative correlation between loan fees and market capitalization in Table 2 serves as consistent evidence. Therefore, we choose a simple size cut and focus on stocks with market capitalization below that of the median NYSE firm. With some variation across years, this median value is in the neighborhood of $2 billion. Accordingly, stocks in our "small group" fall into the traditional small-cap universe, while stocks in our "large group" are traditionally considered mid- and large-caps. Even within small capitalization stocks, there may be considerable variation to the extent supply constraints bind. Thus, we view our choice to focus on this sample as a conservative one that biases our tests against detecting a significant record day effect. In later analysis, we apply more stringent cuts on the sample and demonstrate economically stronger results.

Table 3 presents our main results for liquidity effects. In Column (1), we report that for the early period, percentage effective spread increases by 1.21 basis points on the record date. The point estimate is highly statistically significant, and it marks about a five percent increase relative to the benchmark period, which is represented by the intercept. Importantly, our specification facilitates a comparison of the record day effect to that on the two adjacent days, which we label $RecDay_{-1}$ and $RecDay_{+1}$ for convenience. The incremental effect for the record day is over twice the magnitude of that on the following day, and the estimate on the prior day ($RecDay_{-1}$) is actually negative and once again much smaller in magnitude. Thus, the spike in effective spread is largely isolated to the record date. The results for dollar spreads, shown in Column (3), are similar. There is a $0.0029 increase on the record day compared to the benchmark period spread of $0.061. To put these numbers in perspective, the economic magnitude of the dollar spread increase is in the same ballpark as typical exchange access fees, which are currently capped 0.30 cents (30 mils) per share.[10]

[9] We do not, however, intend to identify firms where the constraint already binds *prior to* the dividend. In unreported tests, we use Markit data (when available) to drop any firm that is hard to borrow (DCBS > 1) on any day in the four weeks leading up to the dividend ex-date. This filtering produces results nearly identical to those reported in Table 3.
[10] The findings of Battalio, Corwin, and Jennings (2016) suggest access fees are sufficiently large to influence order routing choices.

We estimate the models using the large firm subsample, and we report the results in the even-numbered columns for comparison. As expected, since the supply constraint is less likely to bind in these stocks, the record day point estimates are much smaller than they are for the small firm subsample. For example, the record day dummy for large firms, shown in Column (2), is 0.22 basis points, roughly one-fifth of the estimate for small firms in Column (1).

We also note a significant ex-day effect in Column (1) that is about half the magnitude of the record day effect. While we find this result interesting, we refrain from making strong interpretations as traders respond to ex-day price drops and close out positions associated with dividend capture strategies, which may create a number of confounding effects. Our results are also robust to using firm fixed effects or replacing the individual event dummies with only an ex-day dummy and a record day dummy (not reported).[11]

The right-hand side of Table 3 displays results from the late period, 2009-2016. The main result that spreads increase on the record day is present in this sample period as well, though completely confined to the small stock subsample (Columns (5) and (7)). Magnitudes are roughly the same in the late and early samples at 1.58 and 1.21 basis points, respectively. In addition, the spike is more precisely isolated to the record day in the late sample, as the point estimates on adjacent days ($RecDay_{-1}$ and $RecDay_{+1}$) are uniformly negative for both percentage and dollar spreads. Moreover, the ex-day effect is once again about half the record day effect for percent spread and statistically indistinguishable from zero for dollar spread. We view the robustness of our results across sub-periods as important since the equity lending market has become generally more transparent and accessible over time.

One interpretation of our results is that shorting supply constraints temporarily remove liquidity providers from the market, and the increased spreads reflect decreased competition. Indeed, Comerton-

[11] D'Avolio (2002) shows that similar to small capitalization stocks, those with low institutional ownership are more likely to be difficult to borrow. We base our sample cuts on institutional ownership rather than market capitalization and find similar results to those reported in Table 3. For firms with low institutional ownership, spreads increase by a statistically significant 1.68 bps (1.65 bps) on records days for the early (late) period, which is about an order of magnitude greater than the effect for firms with high institutional ownership.

Forde, Jones, and Putnins (2016) document the importance of liquidity-providing short sellers for market quality. A second, and non-mutually exclusive, interpretation is based on adverse selection. Diamond and Verrecchia (1987) argue that an increase in shorting costs (a *restriction* as opposed to an outright prohibition) will disproportionately remove less sophisticated short sellers. As a result, adverse selection risk increases. Kolasinski, Reed, and Thornock (2013) provide consistent evidence from the financial crisis.

We decompose effective spread into realized spread, the component that reverses over a given horizon, and price impact, the permanent component. The former is a proxy for the compensation for liquidity provision, and the latter proxies for adverse selection. We recognize, however, that both are notoriously noisy measures because the calculation requires choosing a future and somewhat arbitrary time increment over which any "temporary" component of spreads should reverse. We compute two versions each of realized spread and price impact, using future time increments of thirty seconds and five minutes. We repeat our main tests, using percent and dollar realized spread as the dependent variable and report the results in Table 4. We compare the effects on realized spread with those on price impact, the component of spreads that reflects the "permanent" price change.

The results provide some support for each interpretation. Focusing first on the early period, the point estimates in Columns (1) and (2) reveal a statistically significant 0.80 basis point increase in 5-minute realized spread on the dividend record date for small stocks compared to a 0.13 basis point increase for large stocks. Increases in price impact are roughly half this magnitude and only statistically significant for small stocks. Collectively, these findings are consistent with the liquidity provision hypothesis. The results are robust across percent and dollar realized spread as well as for each time horizon for measuring realized spreads (not reported). The late period results reported in Panel B are more consistent with an adverse selection story. There, we see a positive but insignificant record-day effect on 5-minute realized spreads for even the smaller stocks (t-statistic = 1.25). In contrast, price impact increases by 1.13 basis points for small stocks and is statistically unaffected in large stocks. When we repeat our analysis using 30-second rather than 5-minute realized spreads and price impacts, our results are qualitatively and quantitatively similar.

We next analyze dividend record date effects on volatility and intraday variance ratios. We report the results in Table 5. In contrast to the liquidity results documented in Table 4, the record day supply shocks appear largely innocuous to these additional dimensions of market quality. Focusing on the small capitalization results, the record day effect on volatility reported in Column (1) is negative and marginally statistically insignificant (*t*-value = -1.70) in the early period. However, the immediately adjacent days exhibit similar negative effects. The record day effect in the late period, shown in Column (5), is negative and insignificant. The variance ratio tests in Columns (5) and (7) reveal point estimates that are positive but insignificant in both the early and late periods. As in the case for volatility, incremental record day effects are indistinguishable from those on adjacent days.

The mostly insignificant record day effects on volatility and variance ratios are inconsistent with Boehmer, Jones, and Zhang (2013), who document large *increases* in volatility for financial stocks subject to the 2008 short sale ban. They also differ from Boehmer and Wu's (2012) finding that shorting *activity* is related to more efficient intraday prices and Saffi and Sigurdsson's (2011) result associating lending supply with longer-run variance ratios. Our findings in Table 5 are more in line with Kaplan, Moskowitz, and Sensoy (2013), who show that exogenous shocks to lending supply have no bearing on volatility. In the next section, we apply more stringent sample cuts to place these findings under more scrutiny.

IV.c. Subsample Analysis

Our tests up to this point suffer from a lack of power for at least two reasons. First, because loan supply is typically slack even in small capitalization stocks, supply shifts around dividend record dates may not impose a binding constraint. Second, lenders' tax-based incentives may not be economically sufficient to warrant a shift in supply. The relatively low power of our tests increases our confidence in the strength of the liquidity results from Tables 3 and 4, but it raises questions about our lack of results for the market quality measures examined in Table 5. In this section, we apply more stringent sample cuts to study a set of stocks we believe to be most susceptible to meaningful supply effects. Of course, the resulting loss of observations in the sampling process could reduce power, so the ultimate outcome is an empirical question.

Markit provides its clients each day a Daily Cost to Borrow Score (*DCBS*) that ranges from 1 to 10. For stocks that are easy to borrow, *DCBS* equals one. Any value exceeding one indicates non-zero borrowing costs. *DCBS* on a typical day is one for about 93% of stocks in our small capitalization sample. To identify a subset of stocks for which the supply constraint will likely bind on the record date, we use a stock's *DCBS* from its *prior* dividend record date and only retain those with prior *DCBS* > 1. For this subsample, we include all stocks regardless of size since *DCBS* directly measures borrowing cost. In the late period, there are 1,221 dividends paid by firms with prior *DCBS* > 1, of which 985 are in the original small capitalization sample. Since daily Markit data is only available from July 2006 forward, imposing this *DCBS* filter in the early period is too restrictive to leave a meaningful early period sample for this analysis.

We repeat our main analysis for effective spread, volatility, and the variance ratio using all stocks with high prior *DCBS* and report the results in Table 6. The record day coefficient for effective spread is 1.72 basis points, which is statistically significant and larger in magnitude than the coefficient reported in Table 4.[12] In this subsample, the record day coefficient for variance ratio, presented in Column (3), is also positive and statistically significant. This result is noteworthy since the tests using the less restrictive sample (*e.g*., all small capitalization stocks in Table 5) were unable to detect a significant effect. The coefficient explaining volatility in Column (2) is also positive, but insignificant.

Since the income tax differential applies to payments in lieu of dividends, lenders have a greater tax-based incentive to reduce the lending supply of stocks with higher dividend yields, all else equal. For out next test, we therefore split the small capitalization sample into high and low dividend yield stocks using the median dividend yield as the breakpoint between groups. Table 7 presents these results, which are somewhat mixed. For the early period, the record day effect for high dividend yield stocks is about 50% larger than that for low dividend yield stocks (1.49 basis points vs. 0.99 basis points), and this difference is

[12] For comparability with the small capitalization sample results presented in Table 3, we repeat this analysis using only small capitalization firms. The record day coefficient explaining effective spread is 2.15 basis points with a *t*-statistic of 2.15.

statistically significant. For the late period, the record day effect is 1.44 basis points for high dividend yield stocks and 1.76 basis points for low dividend yield stocks. Turing to other measures of market quality, results for both the high and low dividend yield stocks generally resemble those in Table 6 as we detect little effect on volatility or the variance ratio.

The stocks categorized as “high yield” in the previous tests still pay somewhat modest dividends. The summary statistics reported in Table 1 indicate a median dividend yield of about two percent annualized in each sample. We conduct an additional out of sample test based on particularly large dividends with a sample of special dividends (CRSP distcd = 1272). The special dividends that fall under this distribution code are taxed the same as ordinary dividends. This sample is substantially smaller than our main sample; we find 150 special dividends in the early period and 213 special dividends in the late period for small capitalization firms that meet the same filters as before. But these dividends are larger than the (annualized) ordinary dividends as well. The median yield for the early (late) sample is 2.05% (3.28%).

We estimate our main specification using special dividends and report the findings in Table 8. The economic magnitudes of the record day effects on effective spreads dwarf those we observed for even the most interesting subsamples of ordinary dividends. For the early period, the record day coefficient presented is 2.17 basis points, statistically significant and larger than the same coefficient in the sample high dividend yield stocks reported in Table 7. The record day coefficient for the late period is even larger at 2.78 basis points, but it is not statistically significant (*t*-statistic = 1.39), likely driven in part by the lack of power of the much smaller sample of special dividends. We also report a positive record day effect on volatility, though it is only statistically significant in the late period. As with many of our other tests, we find no record day effect on variance ratios.

In sum, the results in the section reinforce our main liquidity results from Table 3. Record day effects for effective spreads are uniformly positive and statistically significant. When we focus on situations in which lending supply constraints most likely bind, the record day effects are even stronger, again consistent with our conclusion that short sellers serve as important liquidity suppliers. Results for the other market quality measures we examine – volatility and variance ratios – are generally consistent in sign with our predictions, but their lack of robust, statistical significance prevent us from making any strong statements.

IV.d. Loan Recalls and Short Squeeze Effects

Tax-based disincentives to lend shares on a dividend record day may affect short sellers through two distinct, yet non-mutually exclusive channels. First, some traders who desire new short positions may remain sidelined because they cannot easily locate borrowable shares. This supply constraints mechanism coheres with our main narrative. Second, loan recalls force short sellers to cover open positions by purchasing shares in the open market if they (or their brokers) are unable to find alternative shares to borrow. This latter scenario, which resembles a short squeeze, presents interpretational difficulties. Insofar as abnormal buying pressure induced by covers affects liquidity directly, perhaps through changes in inventory risk or the shifting of potential shorts to the other side of the market as aggressive buyers, the spread increases we document are not clearly attributable to lending supply constraints per se.

Because neither loan recalls nor short covers appear in any of our datasets, we cannot easily disentangle these mechanisms. Instead, we conduct two alternative tests, both of which suggest a short squeeze is not the sole driver of our findings. Our first test analyzes stock price patterns around dividend record dates. A short squeeze on the record day implies a positive record-day return followed by a reversal as the buying pressure subsides. We therefore estimate Equation (4) as above, except we use market-adjusted stock return as the dependent variable. Panel A of Table 9 reports coefficient estimates for each relevant day. Across time and stock-size subsamples, we find no evidence of a significantly positive abnormal record-day return; three of the four coefficients are insignificant, and one of them significantly negative. We report consistently negative abnormal returns on the day following the record day. But these coefficients more reasonably reflect the (sometimes delayed) reversal of a positive abnormal return leading up to the ex-day, as documented in Blocher, Reed, and Van Wessep (2013).

Since a short squeeze mechanism is associated with positive buying pressure, our second test separates all dividend events according to the sign of the record day return. If our results from Table 3 stem from recall-induced buying pressure that somehow also harms liquidity, we expect the spread findings to concentrate in events with positive record day returns. Panel B and C of Table 9 reveals this is not the case. While Panel B shows a positive record day spread effect for events with positive record day returns, the most striking results are in Panel C. Specifically, columns 1 and 3 reveal a statistically significant increase in effective spread even on negative-return record days. Comparing Panel C with Panel B, we see the record day effect for the late period (column 3) is stronger for negative return days than for positive return days, while the opposite is true for the early period.

IV.e. Falsification Tests Around the 2003 Tax Change

Our identification strategy relies critically on equity lenders paying different tax rates on qualified dividends and payments in lieu of dividends. Prior to the Jobs and Growth Tax Relief Reconciliation Act of 2003, no tax differentials existed as dividends were taxed as ordinary income. As a result of this legislation, the tax rate on qualified dividends became 15%, while payments in lieu were still taxed at the lender's marginal tax rate on ordinary income. Thus, if tax induced constraints on lending supply drive our central results, we should find no effect prior to 2003. To conduct this falsification test, we gather all dividends according to our original sampling procedure with ex-days between July 1, 2001 and December 31, 2002. We choose this start date because the tick size on U.S. exchanges was reduced from sixteenths to decimals in early 2001, and Bessembinder (2003) confirms changes in various market quality measures. We therefore focus on 4,455 dividends paid by 1,060 unique firms during the 18-month period between decimalization and the dividend tax change where market structure was roughly constant.

Summary statistics reported in Table 10 reveal that dividends and dividend-paying firms during this period are comparable to those in our early period, summarized in Table 1. The median dividend in the pre-tax change sample is the same as that in the early period, the median price is $29.99 compared to $32.01, and median market capitalization is marginally smaller. Most notable is the fact that spreads are wider in the pre-tax change period, consistent with a gradual downward trend in spreads during the early 2000s.

We conduct our test by pooling the pre-tax change sample with dividend events having ex-days between January 1, 2004 through June 30, 2005 (the first 18 months of the early period). This sampling gives roughly equal weight to events before and after the tax change while allowing the full year of 2003 for the policy change to take effect.[13] We then estimate a version of Equation (4) with the following modifications. First, we include the dummy variable *Post* that equals one after 2003 and zero otherwise, and we interact this variable with the dummies corresponding to various days in event time. Second, we replace the firm-dividend fixed effects with a firm fixed effect. Including this variable implies each *firm* serves as its own control. Thus, we account for any unobservable firm-level determinant of spreads. The interpretation of the *Post* x event day dummy variable interactions is the incremental effect of a particular event day after the tax change.

The main coefficient of interest is that on the *Post* dummy interacted with the record day dummy. We hypothesize a positive coefficient on this variable and an insignificant coefficient on the record day dummy. An insignificant direct effect on the record day implies spreads are no different on the record day prior to the tax change. Table 11 reports the results. The coefficients of interest for effective spreads are indeed positive and significant. The incremental record-day effect during the post-tax change period is 1.48 basis points for small firms and 0.45 basis points for large firms. These values are quite close to the point estimates we report for the early period in Table 3, and the direct record date effect in this specification is negative but indistinguishable from zero. Together these findings, are consistent with the record date effect on spreads existing exclusively after the 2003 tax change. Consistent with the much weaker and often insignificant results for volatility and variance ratios that we present in our previous tables, we generally do not find meaningful incremental effects for these variables around the tax change.

5. Conclusion

Short selling constraints often emerge as endogenous outcomes of either market-wide or idiosyncratic conditions. For example, regulators in several countries imposed shorting bans around the 2008 Financial Crisis period. More generally, individual stocks may be "on special" when changes in investor beliefs result in heightened demand to borrow stocks to sell short. As a result, identifying a

[13] President George W. Bush signed the bill into law on May 28, 2003.

causal effect of short-selling constraints on economic outcomes like liquidity has proven treacherous. The dearth of clean causal inferences impedes policy makers concerned with market quality and the allocative role of prices in the real economy.

Our identification strategy sidesteps these concerns. The plausibly exogenous variation in lending supply on a dividend record date, an otherwise informationless event, facilitates a unique test of the relation between shorting supply constraints and market quality. Our evidence that liquidity deteriorates on dividend record days, especially in stocks that likely have less slack in lending supply, suggest shorting supply constraints affect the cost of transacting faced by all traders. This message is important as a growing body of research argues liquidity influences expected returns (Amihud and Mendelsen, 1986; Acharya and Pedersen, 2005). Authors studying crisis periods argue that policies prohibiting or limiting short selling decrease liquidity. Our results suggest such a decrease would occur in normal economic times as well and bolster the conclusions that the ability to short-sell improves the functioning of financial markets.

That our findings are mostly confined to smaller stocks does not imply our conclusions lack generalizability. Rather, it reflects the well-established fact that in U.S. equity markets, lending supply is typically slack and dividend record date supply shocks do not bind in large firms. In this vein, any naturally occurring shock to lending supply aside from an outright ban is unlikely to bind for large firms. As a practical matter, this recognition points to the tradeoff between studying bans and natural experiments such as ours. Researchers must weigh the benefits of exploiting bans, *e.g.*, the ability to examine even the largest firms, against methodological concerns that bans are often endogenous.

So which potential short sellers are sidelined? While our datasets are not amendable to addressing this question, we offer some conjectures to guide future research. First, the shock to lending supply unlikely affects high frequency traders, at least directly. HFT are largely immune from transitory increases in borrowing costs—they close out net positions before the end of a trading day and need not actually borrow shares. Second, we doubt traditional market makers are sidelined either. Some lenders (e.g., tax exempt institutions) are unaffected by the taxation of substitute dividends. If market makers have better access to the equity lending market than other traders do, they may still be able to find shares and borrow cheaply through dividend record dates. Large institutions and hedge funds likely have access as well. More reasonably, we conjecture that sidelined investors are smaller institutions and retail investors, whose brokers are unwilling to search for borrowable shares. Our findings suggest these traders contribute to a well-functioning market for liquidity.

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Appendix

Table 1.1 Summary Statistics

This table provides summary statistics for dividend events in our sample. Panel A presents results from the early period, 2004-2007. Panel B presents results from the late period, 2009-2016. The variable Market Value is calculated by multiplying the prior year end share price and the number of shares outstanding. The variable Effective Spread Pre Dividend is the average effective spread in days (-30, -6) relative to the ex-dividend day. The variable Variance Ratio is average variance ratio in days (-30, -6) relative to the ex-dividend day. The variable Volatility is average range in days (-30, -6) relative to the ex-dividend day. The variable Share Price is the price (in dollars) as of close of trading on the cum-dividend day. The variable Institutional Ownership is the percentage of shares outstanding that are held by institutions (based on 13-f filings). The variable Short Interest is the average proportion of shares outstanding that are short in the prior six-months. The variable Dividend Amount is the dollar amount of the dividend. The variable Dividend Yield is calculated as the Dividend Amount divided by the Share Price. The Indicative Fee variable is the average lending fee in days (-30, -6) relative to the ex-dividend day, measured in basis points. The statistics we report are the mean, standard deviation (SD), and the 5th, 25th, 50th, 75th, and 95th percentile. We winsorize spread variables, Indicative Fee, Range, and Variance Ratio at the 5% level by trading day.

Panel A: 2004 - 2007

	Mean	SD	P5	P10	P25	P50	P75	P90	P95
Dollar Effective Spread (cents)	4.664	3.711	1.669	1.858	2.343	3.257	5.404	9.870	13.070
Share Price ($)	38.51	48.21	12.28	15.44	22.26	32.01	45.92	63.24	76.43
Institutional Ownership (%)	58.94	26.88	0.00	17.19	40.90	64.83	80.38	89.71	93.80
Short Interest (%)	3.771	4.092	0.445	0.723	1.328	2.520	4.763	8.148	10.936
Dividend Amount ($)	0.173	0.151	0.030	0.040	0.075	0.135	0.230	0.340	0.420
Dividend Yield (%)	0.005	0.004	0.001	0.002	0.003	0.004	0.007	0.010	0.012
Indicative Fee (bps)	62.98	138.28	37.50	37.50	37.50	37.50	50.00	50.00	62.50
N (Dividends)	15,933								
N (Firms)	1,596								

Panel B: 2009 - 2016

	Mean	SD	P5	P10	P25	P50	P75	P90	P95
Dollar Effective Spread (cents)	8.004	14.109	1.148	1.266	1.721	3.212	7.380	17.013	33.509
Share Price ($)	41.42	42.78	9.28	11.97	18.53	31.96	52.17	77.90	99.05
Institutional Ownership (%)	62.79	23.87	11.31	27.54	50.19	68.09	80.16	88.95	93.20
Short Interest (%)	4.220	4.318	0.719	1.032	1.668	2.811	5.149	9.077	12.719
Dividend Amount ($)	0.226	0.246	0.038	0.050	0.090	0.170	0.290	0.455	0.580
Dividend Yield (%)	0.006	0.005	0.001	0.002	0.003	0.005	0.008	0.011	0.014
Indicative Fee (bps)	61.50	247.66	25.00	37.50	37.50	37.50	37.50	50.00	75.00
N (Dividends)	31,059								
N (Firms)	1,757								

Table 1.2 Correlation Matrix

This table provides correlations for key variables in our analysis. Panel A presents results from the early period, 2004-2007. Panel B presents results from the late period, 2009-2016. The variable Dividend Amount is the dollar amount of the dividend. The variable Share Price is the price (in dollars) as of close of trading on the cum-dividend day. The variable Ln(MV) is the natural log of Market Value. The variable Dividend Yield is calculated as the Dividend Amount divided by the Share Price. The variable IO is Institutional Ownership and is the percentage of shares outstanding that are held by institutions (based on 13-f filings). The variable Short Interest is the average proportion of shares outstanding that are short in the prior six-months. The variable Range is average range in days (-30, -6) relative to the ex-dividend day. The variable Variance Ratio is average variance ratio in days (-30, -6) relative to the ex-dividend day. The variables Dollar ES and Percent ES are the average effective spread in days (-30, -6) relative to the ex-dividend day, measured in dollars and percent, respectively. The Ln(Fee) variable is the natural log of Indicative Fee.

Panel A: 2004 - 2007

	Dividend Amount	*Price*	*Ln(MV)*	*Dividend Yield*	*IO*	*Short Interest*	*Volatility*	*Variance Ratio*	*Dollar ES*	*Percent ES*	*Ln(Fee)*
Dividend Amount	1.000										
Price	0.424	1.000									
Ln(MV)	0.307	0.227	1.000								
Dividend Yield	0.576	-0.141	-0.055	1.000							
IO	-0.030	0.078	0.325	-0.211	1.000						
Short Interest	-0.081	-0.045	-0.127	-0.008	-0.050	1.000					
Volatility	-0.270	-0.099	-0.336	-0.088	-0.057	0.288	1.000				
Variance Ratio	-0.025	-0.049	-0.172	0.070	-0.107	-0.046	-0.088	1.000			
Dollar ES	0.055	0.143	-0.507	-0.034	-0.365	-0.180	0.195	0.084	1.000		
Percent ES	-0.175	-0.153	-0.699	0.100	-0.442	-0.174	0.315	0.160	0.814	1.000	
Ln(Fee)	0.007	-0.065	-0.202	0.159	-0.189	0.366	0.205	0.018	0.127	0.239	1.000

Panel B: 2009 - 2016

	Dividend Amount	*Price*	*Ln(MV)*	*Dividend Yield*	*IO*	*Short Interest*	*Volatility*	*Variance Ratio*	*Dollar ES*	*Percent ES*	*Ln(Fee)*
Dividend Amount	1.000										
Price	0.472	1.000									
Ln(MV)	0.324	0.391	1.000								
Dividend Yield	0.498	-0.176	-0.105	1.000							
IO	-0.001	0.101	0.270	-0.155	1.000						
Short Interest	-0.034	-0.068	-0.094	0.036	0.055	1.000					
Volatility	-0.215	-0.217	-0.375	0.034	-0.060	0.193	1.000				
Variance Ratio	-0.035	-0.020	-0.354	0.013	-0.203	-0.126	-0.002	1.000			
Dollar ES	0.060	0.126	-0.093	-0.039	-0.164	-0.097	0.005	0.183	1.000		
Percent ES	-0.087	-0.111	-0.257	0.037	-0.244	-0.119	0.106	0.175	0.897	1.000	
Ln(Fee)	0.011	-0.068	-0.188	0.143	-0.240	0.363	0.096	0.076	0.060	0.112	1.000

Table 1.3 Effective Spreads around the Dividend Dates

This table presents the results of a pooled regression with event day dummies for the period (T-5, T+5) around the ex-dividend day, seen in Equation (4). The dummy variable for the cum-dividend day (T-1) is CumDay. The dummy variable for the ex-dividend day is ExDay. The dummy variable for the record-dividend day (T+2), which is our main variable of interest, is RecDay. We only report the coefficients for the CumDay through the RecDay+1. The dependent variable in columns 1 and 3 is the Percent Effective (ES%), measured in basis points, and the dependent variable in columns 2 and 4 is the Dollar Effective Spread (ES$), measured in cents. We cut the sample based on the market value of the firm relative to the NYSE decile breakpoints. If the firm has a Market Value at or below the 5th decile we consider them as Low MV, otherwise they are High MV. Columns 1 and 2 present results from our "early" sample (2004-2007) and columns 3 and 4 present results from our "later" sample (2009-2016). We winsorize all spread variables each calendar day at the 5% level. We include firm-dividend fixed effects in the regression. The t-statistics are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

	2004-2007				**2009-2016**			
Dependent Variable	***ES%* (bps)**		***ES$* (cents)**		***ES%* (bps)**		***ES$* (cents)**	
Sample Cut	**Low MV**	**High MV**	**Low MV**	**High MV**	**Low MV**	**High MV**	**Low MV**	**High MV**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CumDay	-0.647***	-0.284***	-0.150***	-0.101***	-2.153***	-0.481***	-0.471***	-0.203***
	(-5.43)	(-5.53)	(-4.92)	(-5.24)	(-9.01)	(-3.45)	(-8.37)	(-4.75)
ExDay	0.602***	0.734***	0.176***	0.236***	0.764***	-0.052	0.067	-0.025
	(5.05)	(14.33)	(5.77)	(12.25)	(3.20)	(-0.38)	(1.20)	(-0.59)
$RecDay_{-1}$	-0.383***	-0.129**	-0.078**	-0.062***	-0.461*	-0.328**	-0.098*	-0.154***
	(-3.21)	(-2.51)	(-2.56)	(-3.21)	(-1.93)	(-2.35)	(-1.74)	(-3.61)
RecDay	1.210***	0.224***	0.288***	0.062***	1.575***	-0.072	0.356***	-0.031
	(10.14)	(4.38)	(9.44)	(3.20)	(6.59)	(-0.51)	(6.32)	(-0.72)
$RecDay_{+1}$	0.538***	-0.052	0.156***	-0.032	-0.328	-0.115	-0.126**	-0.143***
	(4.51)	(-1.01)	(5.11)	(-1.64)	(-1.37)	(-0.83)	(-2.24)	(-3.35)
Constant	24.481***	7.143***	6.116***	2.926***	38.987***	17.862***	9.208***	6.446***
	(1,493.61)	(1,014.51)	(1,458.66)	(1,106.56)	(1,187.81)	(931.78)	(1,191.76)	(1,101.33)
Dividend FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.680	0.433	0.687	0.502	0.661	0.882	0.736	0.881
N (Dividends)	8426	7507	8426	7507	15672	15387	15672	15387
N (Dividend Days)	530,836	472,941	530,836	472,941	987,331	969,380	987,331	969,380

Table 1.4 Realized Spreads and Price Impact around the Dividend Dates

This table presents the results of a pooled regression with event day dummies for the period (T-5, T+5) around the ex-dividend day, seen in Equation (4). The dummy variable for the cum-dividend day (T-1) is CumDay. The dummy variable for the ex-dividend day is ExDay. The dummy variable for the record-dividend day (T+2), which is our main variable of interest, is RecDay. We only report the coefficients for the Cum Day through the RecDay+1. The dependent variable in columns 1, 2, 5 and 6 is the Percent Realized Spread (RS%), measured in basis points. The dependent variable in columns 3, 4, 7, and 8 is the Percent Price Impact (PI%), measured in basis points. We cut the sample based on the market value of the firm relative to the NYSE decile breakpoints. If the firm has a Market Value at or below the 5th decile we consider them as Low MV, otherwise they are High MV. The left side of the table presents results from our "early" sample (2004-2007) and the right side of the table presents results from our "later" sample (2009-2016). We winsorize all spread variables each calendar day at the 5% level. We include firm-dividend fixed effects in the regression. The t-statistics are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

	2004-2007				**2009-2016**			
Dependent Variable	***RS%* (bps)**		***PI% (bps)***		***RS%* (bps)**		***PI% (bps)***	
Sample Cut	**Low MV**	**High MV**	**Low MV**	**High MV**	**Low MV**	**High MV**	**Low MV**	**High MV**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CumDay	-0.56***	-0.17***	-0.09	-0.07	-0.59***	-0.17*	-1.50***	-0.41***
	(-4.31)	(-3.07)	(-0.77)	(-1.34)	(-4.02)	(-1.90)	(-7.22)	(-3.66)
ExDay	0.02	0.53***	0.49***	0.10*	-0.14	-0.03	0.70***	-0.13
	(0.16)	(9.49)	(4.24)	(1.94)	(-0.96)	(-0.29)	(3.35)	(-1.20)
$RecDay_{-1}$	-0.33**	-0.06	-0.06	-0.08	-0.09	-0.02	-0.49**	-0.35***
	(-2.51)	(-1.05)	(-0.53)	(-1.47)	(-0.58)	(-0.26)	(-2.36)	(-3.16)
RecDay	0.80***	0.13**	0.41***	0.08	0.18	0.08	1.13***	-0.12
	(6.13)	(2.30)	(3.54)	(1.53)	(1.25)	(0.88)	(5.43)	(-1.05)
$RecDay_{+1}$	0.41***	0.05	0.07	-0.11**	-0.19	-0.07	-0.11	-0.10
	(3.16)	(0.98)	(0.61)	(-2.19)	(-1.31)	(-0.82)	(-0.53)	(-0.92)
Constant	11.11***	3.25***	12.37***	3.87***	14.59***	5.990***	22.19***	10.41***
	(622.63)	(425.90)	(786.66)	(549.47)	(720.53)	(486.21)	(777.72)	(681.31)
Dividend FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.362	0.157	0.276	0.207	0.481	0.713	0.331	0.750
N (Dividends)	15,933	31,059	15,933	31,059	15,933	31,059	15,933	31,059
N (Dividend Days)	530,830	472,939	530,825	472,935	987,163	969,379	987,159	969,375

Table 1.5 Volatility and Price Efficiency

This table presents the results of a pooled regression with event day dummies for the period (T-5, T+5) around the ex-dividend day, seen in Equation (4). The dummy variable for the cum-dividend day (T-1) is CumDay. The dummy variable for the ex-dividend day is ExDay. The dummy variable for the record-dividend day (T+2), which is our main variable of interest, is RecDay. We only report the coefficients for the Cum Day through the RecDay+1. The dependent variable in columns 1, 2, 5 and 6 is Volatility (measured in percent). The dependent variable in columns 3, 4, 7, and 8 is the Variance Ratio (multiplied my 100). We cut the sample based on the market value of the firm relative to the NYSE decile breakpoints. If the firm has a Market Value at or below the 5th decile we consider them as Low MV, otherwise they are High MV. The left side of the table presents results from our "early" sample (2004-2007) and the right side of the table presents results from our "later" sample (2009-2016). We winsorize dependent variables each calendar day at the 5% level. We include firm-dividend fixed effects in the regression. The t-statistics are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

	2004-2007				**2009-2016**			
Dependent Variable	***Volatility***		***Variance Ratio***		***Volatility***		***Varince Ratio***	
Sample Cut	**Low MV**	**High MV**	**Low MV**	**High MV**	**Low MV**	**High MV**	**Low MV**	**High MV**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CumDay	-0.018	-0.008	0.044	-0.126	-0.014	-0.037***	0.036	-0.143
	(-1.05)	(-0.66)	(0.32)	(-0.88)	(-1.11)	(-3.98)	(0.35)	(-1.44)
ExDay	0.170***	0.057***	0.101	0.210	0.122***	0.045***	0.261**	0.095
	(9.99)	(4.64)	(0.72)	(1.46)	(9.78)	(4.87)	(2.54)	(0.96)
$RecDay_{-1}$	-0.050***	-0.017	0.163	-0.137	-0.011	-0.043***	0.089	0.114
	(-2.95)	(-1.37)	(1.17)	(-0.96)	(-0.91)	(-4.58)	(0.86)	(1.15)
RecDay	-0.029*	-0.031**	0.213	0.439***	-0.013	-0.068***	0.064	-0.029
	(-1.70)	(-2.53)	(1.53)	(3.06)	(-1.05)	(-7.29)	(0.63)	(-0.29)
$RecDay_{+1}$	-0.027	-0.064***	0.179	0.301**	0.009	-0.042***	-0.125	0.067
	(-1.60)	(-5.22)	(1.29)	(2.10)	(0.71)	(-4.46)	(-1.22)	(0.67)
Constant	2.757***	2.060***	19.232***	18.363***	3.126***	2.301***	20.337***	17.564***
	(1,179.82)	(1,216.42)	(1,006.77)	(930.70)	(1,826.34)	(1,797.82)	(1,439.78)	(1,293.07)
Dividend FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.276	0.339	0.0302	0.0176	0.4	0.455	0.0724	0.0165
N (Dividends)	8,426	7,507	8,416	7,507	15618	15329	15461	15194
N (Dividend Days)	530,825	472,940	530,177	472,930	983,918	965,712	974,020	957,224

Table 1.6 Market Quality around the Dividend Dates- Cuts on DCBS

This table presents the results of a pooled regression with event day dummies for the period (T-5, T+5) around the ex-dividend day, as seen in Equation (4). The dummy variable for the cum-dividend day (T-1) is CumDay. The dummy variable for the ex-dividend day is ExDay. The dummy variable for the record-dividend day (T+2), which is our main variable of interest, is RecDay. We only report the coefficients for the Cum Day through the RecDay+1. The sample used in this table is that of firms that have a daily cost to borrow score (DCBS) greater than one six months prior to the dividend event. The table presents results from our "later" sample (2009-2016). The dependent variables are ES%, which is the Percent Effective Spread measured in basis points, Volatility measured in percent, and Variance Ratio multiplied by 100. We winsorize all dependent variables each calendar day at the 5% level. We include firm-dividend fixed effects in the regression. The t-statistics are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable	***ES%***	***Volatility***	***Variance Ratio***
	(1)	(2)	(3)
CumDay	-1.630*	-0.007	0.342
	(-1.95)	(-0.17)	(0.87)
ExDay	2.353***	0.165***	0.809**
	(2.81)	(4.27)	(2.06)
$RecDay_{-1}$	-0.227	0.059	-0.861**
	(-0.27)	(1.53)	(-2.19)
RecDay	1.722**	0.016	0.842**
	(2.06)	(0.41)	(2.15)
$RecDay_{+1}$	-0.638	0.081**	0.124
	(-0.76)	(2.11)	(0.31)
Constant	44.017***	3.177***	20.074***
	(383.19)	(598.22)	(372.71)
Dividend FE	Yes	Yes	Yes
Adjusted R^2	0.745	0.475	0.052
N (Dividends)	1221	1214	1202
N (Dividend Days)	76,952	76,503	75,696

Table 1.7 Market Quality around the Dividend Dates- Cuts on Dividend Yield

This table presents the results of a pooled regression with event day dummies for the period (T-5, T+5) around the ex-dividend day, as seen in Equation (4). The dummy variable for the cum-dividend day (T-1) is CumDay. The dummy variable for the ex-dividend day is ExDay. The dummy variable for the record-dividend day (T+2), which is our main variable of interest, is RecDay. We only report the coefficients for the Cum Day through the RecDay+1. We define high and low dividend yield as above and below, respectively, the median value of Dividend Yield. The dependent variables are ES%, which is the Percent Effective Spread measured in basis points, Volatility measured in percent, and Variance Ratio multiplied by 100. Panel A presents results from our "early" sample (2004-2007) and Panel B presents results from our "later" sample (2009-2016). We winsorize all dependent variables each calendar day at the 5% level. We include firm-dividend fixed effects in the regression. The t-statistics are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Panel A: Early Period

Dependent Variable	***ES%***		***Volatility***		***Variance Ratio***	
Sample Cut	**High Yield**	**Low Yield**	**High Yield**	**Low Yield**	**High Yield**	**Low Yield**
	(1)	(2)	(3)	(4)	(5)	(6)
CumDay	-0.842***	-0.493***	-0.019	-0.022	0.091	0.012
	(-4.34)	(-3.32)	(-0.90)	(-1.21)	(0.41)	(0.07)
ExDay	0.761***	0.477***	0.197***	0.093***	0.017	0.184
	(3.92)	(3.21)	(9.28)	(5.15)	(0.08)	(0.94)
$RecDay_{-1}$	-0.637***	-0.182	-0.028	-0.057***	0.084	0.239
	(-3.28)	(-1.22)	(-1.30)	(-3.13)	(0.38)	(1.22)
RecDay	1.492***	0.986***	-0.008	-0.041**	0.375*	0.058
	(7.69)	(6.64)	(-0.39)	(-2.28)	(1.69)	(0.30)
$RecDay_{+1}$	0.825***	0.310**	-0.003	-0.043**	0.373*	0.021
	(4.25)	(2.08)	(-0.12)	(-2.35)	(1.68)	(0.11)
Constant	27.560***	22.039***	2.611***	2.710***	19.619***	19.408***
	(1,034.08)	(1,079.34)	(896.12)	(1,093.04)	(644.79)	(720.43)
Dividend FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.674	0.672	0.332	0.291	0.0323	0.0288
N (Dividends)	3727	4699	3727	4699	3721	4694
N (Dividend Days)	234,800	296,036	234,793	296,032	234,431	295,746

Panel B: Late Period

Dependent Variable	*ES%*		*Volatility*		*Variance Ratio*	
Sample Cut	**High Yield**	**Low Yield**	**High Yield**	**Low Yield**	**High Yield**	**Low Yield**
	(1)	(2)	(3)	(4)	(5)	(6)
CumDay	-2.594***	-1.563***	-0.010	-0.021	-0.127	0.247
	(-7.81)	(-4.61)	(-0.69)	(-1.35)	(-0.88)	(1.49)
ExDay	1.762***	-0.574*	0.162***	0.031**	0.317**	0.246
	(5.31)	(-1.69)	(11.66)	(2.04)	(2.20)	(1.48)
$RecDay_{-1}$	-0.566*	-0.320	0.003	-0.021	-0.128	0.3450**
	(-1.71)	(-0.94)	(0.20)	(-1.38)	(-0.89)	(2.08)
RecDay	1.441***	1.755***	0.004	-0.019	0.210	-0.104
	(4.34)	(5.18)	(0.30)	(-1.25)	(1.46)	(-0.62)
$RecDay_{+1}$	-0.501	-0.096	0.009	0.011	-0.055	-0.283*
	(-1.51)	(-0.28)	(0.64)	(0.70)	(-0.38)	(-1.70)
Constant	42.673***	34.051***	3.029***	3.001***	20.705***	20.599***
	(935.86)	(731.65)	(1,583.21)	(1,425.00)	(1,045.55)	(903.84)
Dividend FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.657	0.660	0.474	0.387	0.0702	0.0747
N (Dividends)	8973	6699	8938	6680	8851	6609
N (Dividend Days)	565,296	422,035	563,089	420,829	557,641	416,379

Table 1.8 Market Quality around the Dividend Dates for Special Dividends

This table presents the results of a pooled regression with event day dummies for the period (T-5, T+5) around the ex-dividend day, as seen in Equation (4). The dummy variable for the cum-dividend day (T-1) is CumDay. The dummy variable for the ex-dividend day is ExDay. The dummy variable for the record-dividend day (T+2), which is our main variable of interest, is RecDay. We only report the coefficients for the Cum Day through the RecDay+1. In this table we examine special dividends only, dividends that have a distribution code of 1272. The sample used in this table is that of firms that have a daily cost to borrow score (DCBS) greater than one six months prior to the dividend event, and we cut the sample based on the market value of the firm relative to the NYSE decile breakpoints. We only keep observations if the firm has a Market Value at or below the 5th decile. The table presents results from our "later" sample (2009-2016). The dependent variables are ES%, which is the Percent Effective Spread measured in basis points, Volatility measured in percent, and Variance Ratio multiplied by 100. We winsorize all dependent variables each calendar day at the 5% level. We include firm-dividend fixed effects in the regression. The t-statistics are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

	2004-2007			**2009-2016**		
Dependent Variable	***ES%***	***Volatility***	***Variance Ratio***	***ES%***	***Volatility***	***Variance Ratio***
	(1)	(2)	(3)	(4)	(5)	(6)
CumDay	-1.412	-0.216	0.42	-6.808***	0.142	1.527
	(-1.22)	(-0.84)	(0.32)	(-3.40)	(1.15)	(1.23)
ExDay	5.266***	1.204***	-0.159	5.829***	1.157***	0.752
	(4.55)	(4.70)	(-0.12)	(2.91)	(9.37)	(0.60)
$RecDay_{-1}$	0.467	0.474*	0.791	-2.048	0.470***	-0.414
	(0.40)	(1.85)	(0.61)	(-1.02)	(3.81)	(-0.33)
RecDay	2.169*	0.324	-0.591	2.775	0.327***	-0.064
	(1.87)	(1.26)	(-0.45)	(1.39)	(2.64)	(-0.05)
$RecDay_{+1}$	1.46	0.106	-1.035	1.104	0.149	-0.503
	(1.26)	(0.41)	(-0.80)	(0.55)	(1.20)	(-0.40)
Constant	32.180***	3.017***	19.976***	50.610***	3.261***	20.764***
	(202.35)	(85.72)	(112.04)	(183.91)	(192.13)	(120.97)
Dividend FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.687	0.139	0.0314	0.723	0.282	0.03
N (Dividends)	150	150	119	213	213	126
N (Dividend Days)	9,450	9,450	7,484	13,419	13,413	7,936

Table 1.9 Stock Returns and Effective Spreads around the Dividend Dates

This table presents the results of a pooled regression with event day dummies for the period (T-5, T+5) around the ex-dividend day, as seen in Equation (4). The dummy variable for the cum-dividend day (T-1) is CumDay. The dummy variable for the ex-dividend day is ExDay. The dummy variable for the record-dividend day (T+2), which is our main variable of interest, is RecDay. We only report the coefficients for the Cum Day through the RecDay+1. We cut the sample based on the market value of the firm relative to the NYSE decile breakpoints. If the firm has a Market Value at or below the 5th decile we consider them as Low MV, otherwise they are High MV. In all panels, Columns 1 and 2 present results from our "early" sample (2004-2007) and columns 3 and 4 present results from our "later" sample (2009-2016). In Panel A, the dependent variable is market-adjusted stock returns in percent. We compute market adjusted returns we subtract the CRSP value-weighted index retrun from the individual stock return. In Panel B and C the dependent variable is the Percent Effective Spread (ES%), measured in basis points. In Panel B we examine dividend events that have a positive market-adjusted return on the record day. In Panel C, we examine dividend events that have a non-positive market-adjusted return on the record day. We winsorize all dependent variables each calendar day at the 5% level. We include firm-dividend fixed effects in the regression. The t-statistics are reported in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01

Panel A: Abnormal Returns

	2004-2007		**2009-2016**	
Dependent Variable	***Market-Adj Rets (%)***			
Sample Cut	**Low MV**	**High MV**	**Low MV**	**High MV**
	(1)	(2)	(3)	(4)
CumDay	0.0562***	-0.0143	0.0170	-0.0294***
	(3.57)	(-1.09)	(1.42)	(-3.02)
ExDay	0.1494***	0.0424***	0.0978***	0.0439***
	(9.49)	(3.22)	(8.18)	(4.51)
$RecDay_{-1}$	0.0433***	0.0128	-0.0207*	-0.0182*
	(2.75)	(0.97)	(-1.73)	(-1.87)
RecDay	0.0189	-0.0033	-0.0304**	-0.0117
	(1.20)	(-0.25)	(-2.54)	(-1.20)
$RecDay_{+1}$	-0.0416***	-0.0001	-0.0182	-0.0209**
	(-2.64)	(-0.01)	(-1.53)	(-2.14)
Constant	-0.0186***	-0.0053***	0.0033**	0.0093***
	(-8.56)	(-2.92)	(2.01)	(6.92)
Dividend FE	Yes	Yes	Yes	Yes
Adjusted R^2	-0.00225	-0.000448	-0.00295	-0.00185
N (Dividends)	8387	7474	15727	15447
N (Dividend Days)	528,395	470,874	990,830	973,170

Panel B: Positive Return Record Days

	2004-2007		2009-2016	
Dependent Variable	*ES% (bps)*			
Sample Cut	**Low MV**	**High MV**	**Low MV**	**High MV**
	(1)	(2)	(3)	(4)
CumDay	-0.642***	-0.209***	-2.075***	-0.456**
	(-3.76)	(-2.89)	(-6.11)	(-2.26)
ExDay	0.392**	0.700***	0.815**	0.045
	(2.30)	(9.68)	(2.40)	(0.22)
$RecDay_{-1}$	-0.465***	-0.075	-0.592*	-0.091
	(-2.72)	(-1.03)	(-1.74)	(-0.45)
RecDay	1.627***	0.293***	1.242***	0.039
	(9.52)	(4.06)	(3.66)	(0.19)
$RecDay_{+1}$	0.582***	-0.063	-0.337	0.036
	(3.40)	(-0.87)	(-0.99)	(0.18)
Constant	24.514***	7.156***	38.515***	18.213***
	(1,044.54)	(720.27)	(825.76)	(656.88)
Dividend FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.681	0.452	0.657	0.882
N (Dividends)	4154	3648	7542	7602
N (Dividend Days)	261,711	229,802	475,142	478,926

Panel C: Negative Return Record Days

	2004-2007		2009-2016	
Dependent Variable	*ES% (bps)*			
Sample Cut	**Low MV**	**High MV**	**Low MV**	**High MV**
	(1)	(2)	(3)	(4)
CumDay	-0.650***	-0.337***	-2.252***	-0.502***
	(-3.90)	(-4.64)	(-6.71)	(-2.62)
ExDay	0.806***	0.767***	0.701**	-0.150
	(4.83)	(10.56)	(2.09)	(-0.78)
$RecDay_{-1}$	-0.300*	-0.180**	-0.317	-0.559***
	(-1.80)	(-2.49)	(-0.94)	(-2.91)
RecDay	0.810***	0.155**	1.876***	-0.185
	(4.86)	(2.14)	(5.59)	(-0.96)
$RecDay_{+1}$	0.492***	-0.041	-0.271	-0.265
	(2.95)	(-0.57)	(-0.81)	(-1.38)
Constant	24.448***	7.131***	39.476***	17.466***
	(1,067.94)	(715.31)	(856.39)	(662.76)
Dividend FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.679	0.416	0.665	0.883
N (Dividends)	4273	3863	8187	7847
N (Dividend Days)	269,212	243,351	515,780	494,360

Table 1.10 Summary Statistics- Ex-Days around Tax Change

This table provides summary statistics for our sample prior to the dividend tax change in 2003. We include all dividend with an ex-day in the 18-month period prior to 2003. The variable Market Value is measured in millions of dollars and is calculated by multiplying the prior year end share price and the number of shares outstanding. The variables Dollar Effective Spread Pre Dividend and Percent Effective Spread Pre Dividend are calculated as the average dollar and percent, respectively, effective spread in days (-30, -6) relative to the ex-dividend day. The variable Variance Ratio is average variance ratio in days (-30, -6) relative to the ex-dividend day. The variable Volatiltiy is average range in days (-30, -6) relative to the ex-dividend day. The variable Share Price is the price (in dollars) as of close of trading on the cum-dividend day. The variable Institutional Ownership is the percentage of shares outstanding that are held by institutions (based on 13-f filings). The variable Short Interest is the average proportion of shares outstanding that are short in the prior six-months. The variable Dividend Amount is the dollar amount of the dividend being paid. The variable Dividend Yield is calculated as the Dividend Amount divided by the Share Price. The statistics we report are the mean, standard deviation (SD), and the 5th, 25th, 50th, 75th, and 95th percentile. We winsorize spread variables, Range, and Variance Ratio at the 5% level by trading day.

	Mean	SD	P5	P10	P25	P50	P75	P90	P95
Market Value ($ 1,000,000)	8943	29101	163	231	488	1475	5427	17406	36318
Dollar Effective Spread (cents)	6.296	4.669	2.027	2.343	3.161	4.754	7.493	13.307	16.894
Percent Effective Spread (bps)	24.075	21.283	5.268	6.285	9.186	15.862	31.435	56.274	69.954
Variance Ratio	0.192	0.037	0.139	0.148	0.166	0.188	0.212	0.240	0.260
Volatility	0.026	0.010	0.013	0.015	0.019	0.024	0.031	0.039	0.044
Share Price ($)	35.31	37.09	12.16	15.01	21.44	29.99	42.67	57.70	68.70
Institutional Ownership (%)	57.93	23.86	12.58	21.80	42.07	61.79	76.71	86.18	90.99
Short Interest (%)	2.390	3.015	0.238	0.397	0.840	1.526	2.784	5.078	7.250
Dividend Amount ($)	0.167	0.139	0.030	0.040	0.070	0.135	0.225	0.327	0.415
Dividend Yield (%)	0.005	0.004	0.001	0.002	0.003	0.005	0.007	0.010	0.012
N (Dividends)	4,455								
N (Firms)	1,060								

Table 1.11 Falsification Test – Pre and Post 2003 Tax Change

This table presents the results of a pooled regression with event day dummies for the period (T-5, T+5) around the ex-dividend day, as seen in Equation (4). The sample is all ordinary dividends in the 18 months ending December 2002 or the 18 months beginning January 2004. The dummy variable for the cum-dividend day (T-1) is CumDay. The dummy variable for the ex-dividend day is ExDay. The dummy variable for the record-dividend day (T+2) is RecDay. We also interact these variables with a Post variable that equals 1 if the Ex-dividend date is after the 2003 tax change. We only report the coefficients for the CumDay through the RecDay+1. The dependent variable in columns 1 and 2 is the Percent Effective Spread (ES%). The dependent variable in column 3 and 4 is Volatility, measured in percent. The dependent variable in column 5 and 6 is the Variance Ratio, multiplied by 100. We cut the sample based on the market value of the firm relative to the NYSE decile breakpoints. If the firm has a Market Value at or below the 5th decile we consider them as Low MV, otherwise they are High MV. We winsorize all dependent variables each calendar day at the 5% level. We include firm fixed effects in the regression. The t-statistics are reported in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01

Dependent Variable	**ES% (bps)**		**Volatility**		**Variance Ratio**	
Sample Cut	**Low MV**	**High MV**	**Low MV**	**High MV**	**Low MV**	**High MV**
	(1)	(2)	(3)	(4)	(5)	(6)
CumDay	-0.991**	-0.465***	-0.108***	-0.210***	0.161	-0.018
	(-2.38)	(-3.13)	(-3.14)	(-9.48)	(0.51)	(-0.07)
ExDay	1.171***	2.285***	0.049	-0.090***	0.180	-0.211
	(2.81)	(15.37)	(1.42)	(-4.06)	(0.56)	(-0.82)
$RecDay_{-1}$	-0.249	-0.258*	-0.037	-0.161***	0.211	0.028
	(-0.60)	(-1.73)	(-1.08)	(-7.29)	(0.66)	(0.11)
RecDay	-0.575	-0.210	0.000	-0.118***	0.134	0.093
	(-1.38)	(-1.41)	(0.01)	(-5.33)	(0.42)	(0.36)
$RecDay_{+1}$	-0.161	-0.464***	0.010	-0.136***	-0.117	0.002
	(-0.39)	(-3.12)	(0.28)	(-6.14)	(-0.37)	(0.01)
Post	-23.143***	-8.230***	-0.582***	-1.086***	0.391***	0.172***
	(-266.40)	(-268.39)	(-91.36)	(-203.93)	(5.89)	(3.22)
*$RecDay_{-1}$ * Post*	0.150	0.114	0.012	0.119***	-0.027	-0.087
	(0.28)	(0.55)	(0.31)	(3.44)	(-0.07)	(-0.24)
*RecDay * Post*	1.479***	0.453**	-0.017	0.092***	-0.019	0.514
	(2.78)	(2.20)	(-0.42)	(2.66)	(-0.05)	(1.43)
*$RecDay_{+1}$ * Post*	0.290	0.272	-0.035	0.094***	0.182	0.366
	(0.54)	(1.32)	(-0.89)	(2.73)	(0.45)	(1.02)
Constant	50.498***	16.282***	3.083***	2.952***	19.819***	18.642***
	(787.68)	(762.88)	(580.75)	(922.73)	(403.82)	(501.60)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.571	0.441	0.206	0.273	0.0339	0.017
N (Dividends)	4872	5383	5383	5249	4864	5383
N (Dividend Days)	306,935	339,129	339,125	330,663	306,401	339,117

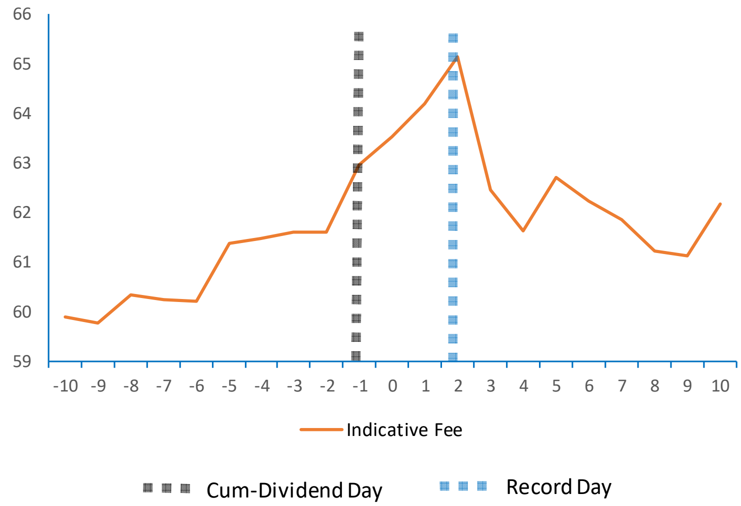
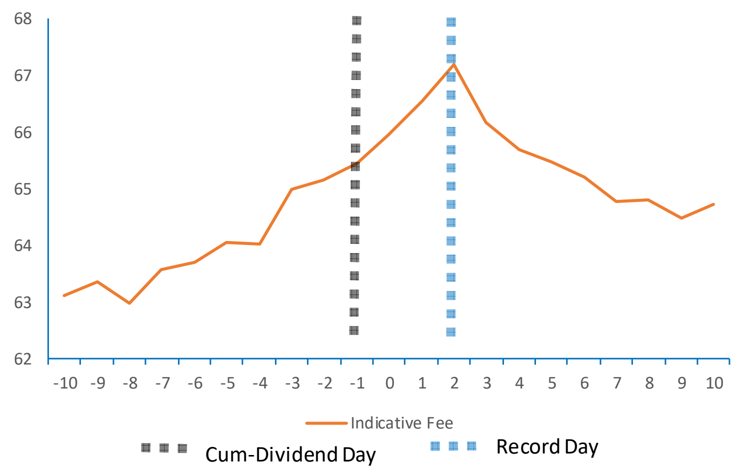


Figure 1.1 Indicative Fee around Dividend Dates

Panel A: Early Period (2004 - 2007)



Panel B: Late Period (2009 - 2016)

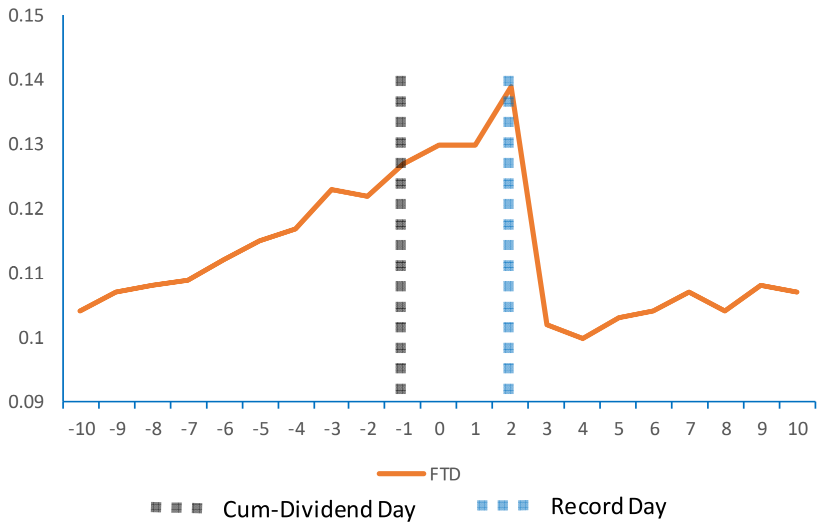
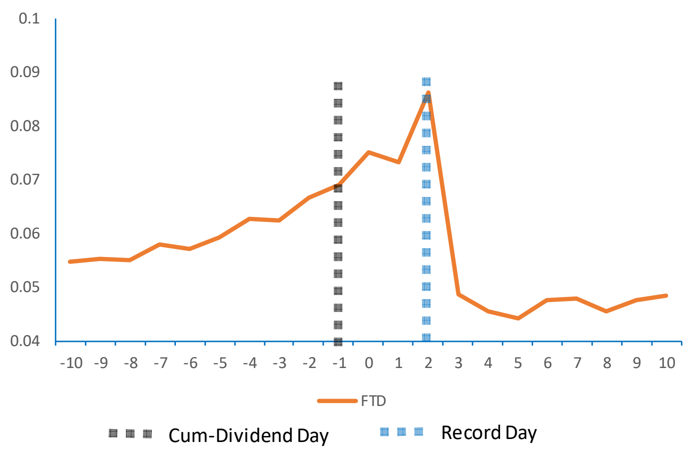


Figure 1.2 Fails to Deliver around Dividend Dates

Panel A: Early Period (2004 - 2007)



Panel B: Late Period (2009 - 2016)

CHAPTER II
DO ANALYSTS LEARN FROM THE TRADING OF INFORMED INVESTORS? EVIDENCE FROM SHORT SELLERS.

Abstract

I examine whether sell-side equity analysts use the trading activity of short sellers in their information set. Taking advantage of the lagged disclosure of short interest, I find that analysts exhibit an increased propensity to downgrade their recommendations for a stock after a disclosed increase in short selling. I also find a significantly positive relationship between changes in short interest and the likelihood of a downward EPS revision. This relationship is driven by an increased propensity to revise down when short interest increases. Overall, these results suggest that market participants extract information from short-sellers' trading activity.

1. Introduction

Sell-side equity analysts play an important role in our financial markets as they serve as informational intermediaries and aid in investment decision making. In addition, markets are known to respond to the recommendation changes and earnings estimates of analysts. Prior research questions their objectivity, suggesting they are reluctant to incorporate negative information and overly optimistic in their stock recommendations (Lin and McNichols 1998; Barber, Lehavy, and Trueman 2007) and earnings forecasts (De Bondt and Thaler 1990; Michaely and Womack 1999; Hong and Kubik 2003). In this paper, I examine the actions of analysts after the disclosure of a specific piece of negative information- a spike in short interest. By examining the interactions of analysts and short sellers I provide evidence on the willingness of analysts to incorporate negative information into their decisions. Taking advantage of the gap between when short sellers positions are disclosed from when they are initiated, I isolate the response of analysts to the disclosure itself. More generally, I offer insight into analysts' information set, also known as the "black box."[14]

[14] See Bradshaw (2011) for an explanation of the "black box" and the current state of the literature surrounding the information valued and possessed by analysts

Analysts and short sellers face different incentives. Analysts may over-recommend stocks to obtain additional business or trading commissions from their customers (Lin and McNichols 1998; Barber et al. 2007; Carleton, Chen, and Steiner 1998) or to gain access to private information via direct management contact (Lim 2001). Therefore, analysts may choose to ignore negative information (Scherbina 2007) as benefits outweigh the cost of being less accurate. Alternatively, they may be systematically optimistic and underreact to negative information and/or overreact to positive information (Easterwood and Nutt 1999). Short sellers' incentives, in contrast, are clearer because they place their own capital at stake. Short-sellers' motivation to make profit drives them to incorporate whatever information they discover into their trading, whether it be positive or negative. In addition, a robust empirical literature shows that short sellers correctly predict future returns (see, for example, Boehmer, Jones, and Zhang 2008). The differing incentives between analysts and short sellers create an interesting interaction. Although many studies have examined analysts and short sellers, prior studies have bene unable to isolate a direction of causality on analysts' willingness to garner information from short sellers' trading activity.[15]

Whether analysts mimic the actions of short sellers is ultimately an empirical question. On one hand, analysts might ignore short sellers if they are overconfident and/or they succumb to incentives to disregard negative information. On the other hand, analysts might mimic short sellers if they believe that short sellers are more informed (Drake, Rees, and Swanson 2011) and they are willing to incorporate the information provided by short sellers. In addition, competition among analysts can reduce bias (Hong and Kacperczyk 2010), analyst's accuracy leads to higher

[15] Short sellers have information above and beyond that of financial analysts (Drake, Rees, and Swanson 2011). Also, short sellers are informed and trade ahead of both analysts' downgrades (Christophe, Ferri, and Hsieh 2010) and earnings announcements (Christophe, Ferri, and Angel 2004).

reputations (Jackson 2005), and analysts have incentives associated with compensation and promotions to develop strong reputations (Leone and Wu 2002; Hong and Kubik 2003).

Given that analysts' information set is unobservable, researchers struggle to draw causal inferences about their decision-making. Although the research in this area is limited, Brown, Call, Clement and Sharp (2016) offers a glimpse. In their survey of buy-side analysts, they find that analysts surveyed highly value 10-K and 10-Q reports when determining stock recommendations. In addition, analysts utilize management access, calls and visits with sell-side analysts, and knowledge of other investors' opinions or holdings. It is the knowledge of other investors (short sellers) opinions or holdings that I examine. In addition to the lack of observable information, studies that examine analysts' actions face the criticism of confounding information and events. In the context of other studies, short sellers' and analysts' decisions might be endogenously determined.

I mitigate endogeneity concerns by exploiting the lagged disclosure of short interest, which occurs between seven and fourteen days after the short interest effective date.[16] Since short interest is stale by up to fourteen days on the date it is disclosed, confounding information or events are likely outdated. My approach resembles that in Kecskes et al. (2013), who document that short sellers provide predictive information to creditors in the bond market on and around the disclosure dates of short interest.

I find that analysts exhibit an increased propensity to downgrade their recommendations for a stock after a disclosed increase in short interest. In contrast, their propensity to upgrade is

[16] Nasdaq stocks report short interest after trading hours on the 7th business day after the effective short interest date. NYSE and AMEX stock prior to June 30, 2008 reported short interest after trading hours on the 4th business day after the effective short interest date. All short interest reporting dates on and after June 30, 2008 FINRA took over the consolidation of all short interest and NYSE and AMEX stocks report short interest after trading hours on the 7th business day after the effective short interest date.

unaffected by the disclosure of either increases or decreases in short selling. The unwillingness for analysts to incorporate positive information may come as a surprise as we know there can be good news in short interest disclosures (Boehmer et al. 2010). However, a plausible explanation for the asymmetry stems from the clarity of the signal sent by short sellers. An increase in short interest is a very clear indication that short sellers believe a stock is overvalued. However, a decrease in short interest is ambiguous as short sellers can cover for reasons that are exogenous to the value of the stock. For example, capital constraints might force short sellers out of their positions.[17] Given the ambiguity of short interest decreases, analysts can more easily interpret the information content of increases in short interest than decreases and therefore they may not respond to decreases. In addition, the asymmetry could, in part, be due to the fact the analyst's recommendations tend to be overly optimistic and they are less willing to upgrade their recommendation regardless of new information.[18]

I also find a significantly positive relationship between changes in short interest and the likelihood of a downward EPS revision. An increased propensity to revise down when short interest increases drives this relationship.[19] These findings reinforce the interpretation that analysts respond to increases in short interest.

An alternative explanation is that analysts respond to the same information as short sellers, but with a lag. In a falsification test, I use a placebo date that is near, but before, dissemination and find no significant relationship between downgrades and short interest spikes. Taken together,

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[17] Other reasons include short sellers taking profits because price is now at or below 'correct' value, realizing they were incorrect in their assessment, or being squeezed out of their positions

[18] See also Ali, Klein, and Rosenfeld 1992; Dugar and Nathan 1995; Michaely and Womack 1999; Hong and Kubik 2003

[19] This increase is in addition to the unconditional higher probability of a downward revision than an upward revision that is documented in the literature on analysts "walk-downs." This literature shows that analysts optimistic forecast bias decreases as we approach the earnings announcement date (Richardson, Teoh, and Wysocki 2004).

these results indicate that sell-side equity analysts respond to short interest spikes themselves, as opposed to the underlying information short sellers collect. They appear to incorporate the knowledge of other investors opinions or holdings into their recommendation decisions and EPS revisions. Also, the asymmetry in their willingness to respond to short interest increases and decreases suggests that analysts have limited ability in processing noisy signals sent by short sellers.

This paper contributes to multiple strands of research. The first is work on the biases in analysts' recommendations and EPS forecasts. Prior research has reached differing conclusions about the existence and source of this bias. This paper provides evidence that analysts are willing to incorporate negative information. In addition, I document an asymmetry in analysts' actions around short interest increases and decreases, which may be driven by how noisy short interest decreases are relative to increases. Second, this paper adds to the literature on identifying inputs into analyst's outputs (i.e. penetrating the "black box"). This is the first paper to show that analysts "black box" contains the trading activity of short sellers. Finally, this paper is related to the analysts herding literature. There is evidence that analysts are willing to follow the signals sent by other analysts when they issue recommendations (Welch 2000; Jegadeesh and Kim 2009) and when they issue earnings forecasts (Trueman 1994), i.e. analysts exhibit herding behavior. Studies of herding typically analyze whether market participants learn from and mimic the actions of others within their group. This paper relates in the sense that analysts are following a signal conveyed by another group of market participants.

This paper also adds to our understanding of how short sellers influence our financial markets, which is of interest to both investors and regulators. If market participants change their behavior based on the trading of short sellers, in a manner that benefits price discovery, then imposing constraints on short sellers could be harmful to market efficiency. If regulators are unarmed with this information, then changes to the current regulatory landscape may generate unintended consequences. The findings in this paper indicate that analysts act as a conduit through which information from short sellers can flow to the market. Analyst appear to have skill in processing the information content of short interest increases which is consistent with the role they serve in the market as informational intermediaries. Also, the findings in this paper suggest that since short sellers are an important source of information, more frequent trade disclosure of short sellers could be beneficial for our equity markets.

2. Review / Hypothesis Development

II.a. Related Literature

Prior research has found that analysts are overly optimistic when they issue recommendations (Lin and McNichols 1998; Barber et al. 2007) and future earnings estimates (De Bondt and Thaler 1990; Michaely and Womack 1999; Hong and Kubik 2003). The literature offers two broad explanations. First, analysts may have acquired negative information throughout their research and analysis but choose to ignore it (Scherbina 2008). Reasons for doing so include investment banking relationship business (Dugar and Nathan 1995; Lin and McNichols 1998), increasing trading commissions (Irvine 2004), and fostering relationships with management (Francis and Philbrick 1993; Chen and Matsumoto 2006). Second, analysts may be systematically optimistic by underreacting to negative information and/or overreacting to positive information (Easterwood and Nutt 1999). This paper seeks to add to this discussion by studying times when a specific type of negative information is disclosed and examining analysts' actions surrounding the disclosure.

Penetrating analysts' "black box" is a much more recent focus in the literature. Using proprietary data, Soltes (2014) provides evidence that private interactions with management are an important source of information for analysts. Confirming that result, Brown, Call, Clement, and Sharp (2015) conduct surveys of sell-side analysts and document that private access to management is "very useful" for most of sell-side analysts. In addition, Brown et al. (2015) show that most analysts find industry knowledge, earnings conference calls, and management's earnings guidance as "very useful" for determining earnings forecasts and stock recommendations. Although, they don't directly test the interactions of short sellers and analysts, Drake et al. (2011), analyze short sellers and analysts and provide weak evidence that analysts actions appear to correctly incorporate information that is contained in variables that are known to predict future returns.

This paper is also related to short sellers' impact on the financial markets. Short sellers theoretically incorporate negative information into stock prices (Diamond and Verrecchia 1987). Empirically evidence shows that short sellers trading predicts future returns (Boehmer, Jones, and Zhang 2008). Consistent with their ability to predict returns, short sellers' trades predict numerous corporate events as well as analysts' actions (Christophe et al. 2010). Examining short selling prior to earnings announcements, Christophe et al. (2004) find that short sellers can predict negative earnings surprises. Their information appears to go beyond just that of earnings announcements, as they have been shown to predict earnings restatements (Desai, Krishnamurthy, and Venkataraman 2006; Efendi and Swanson 2009), bond rating downgrades (Henry, Kisgen, and Wu 2013), and the discovery and severity of corporate financial misconduct (Karpoff and Lou 2010).

More directly this paper is related to the literature that studies the interaction of short sellers and financial analysts. Drake et al. (2011) document that while short selling is associated in the correct direction with eleven different fundamental variables that predict returns, analysts fail to correctly incorporate the same fundamental information. Therefore, they conclude that short sellers are superior in their use of information and investors can increase returns by using short sellers trading activity and trading against analysts' recommendations. The ability of short sellers to predict value relevant events extends to that of analysts' actions. Christophe et al. (2010) document that short sellers can reliable predict analysts' downgrades by showing that there is abnormal short selling in the three days prior to the public announcement of an analyst downgrade. Pownall and Simko (2005) document that short interest spikes are associated with negative returns around the disclosure of short sellers trading activity. Using an exogenous change in short-sale constraints (RegSHO), Choi (2018) find that lower short selling constraints are positively associated with analysts' rounding of forecasts.[20] Also using RegSHO, Ke, Lo, Sheng, and Zhang (2018) find that lowering short selling constraints improves analyst earnings forecast quality.

II.b. Hypothesis Development

When an analyst issues a recommendation, they are making a statement about where they expect future price to be relative to current price. This is very much in line with what trading signals from short sellers represent. When a short seller initiates a position, he/she profits if the current price falls in the future. Analysts may choose to fully incorporate the information in an unbiased manner. We have evidence that competition among analysts can reduce bias (Hong and Kacperczyk 2010), analyst's accuracy leads higher reputations (Jackson 2005), and analysts have

[20] RegSHO was adopted in 2005 by the SEC. This suspended a rule known as the "uptick" rule for a random group of stocks in the Russell 3000 Index referred to as the "pilot" stocks. The "uptick" rule was considered a short selling constraint as it required short sellers to only transact after an uptick in price. Therefore, the suspension of the "uptick" rule for the "pilot" stocks was considered an exogenous decrease in short selling constraints.

incentives, associated with their career, to develop strong reputations (Leone and Wu 2002; Hong and Kubik 2003). Alternatively, when provided with negative information analysts may choose to ignore that information (Scherbina 2008), or they may interpret that information in a bias manner (Easterwood and Nutt 1999). Given that short sellers' trading activity is known to contain information about future price and earnings, above and beyond that of analysts, I posit that analysts value the information that is contained in the trading activity of short sellers. For the recommendation analysis, I hypothesize that analysts' recommendation decisions, that take place immediately after the disclosure of short interest, are consistent with the signal about future price that is inferred by the change in short interest. The alternative hypothesis is that analysts ignore the information content of short interest when they issue recommendations.

When analysts issue stock recommendations they typically have a twelve-month time horizon and usually change before the twelve months is up. This makes it very challenging to measure the accuracy of analysts' stock recommendations. In contrast, analysts' EPS estimates provide an actual value of their estimate, so when we find out the actual earnings, we will be able to precisely determine how accurate the analysts were. In addition, recommendation decisions are more infrequent than EPS revisions and, therefore, we will have a larger sample of analysts' actions when studying the EPS revisions. One caveat of studying analysts EPS revisions is that their estimates do not directly translate into price predictions like stock recommendations and short selling activity do, so the predictions for EPS estimates are not as strong. Regardless, short selling has been shown to predict future earnings surprises (Christophe et al. 2004) and therefore analysts might learn from shorting activity prior to earnings announcements. For the EPS estimate revision analysis, I hypothesize that analysts' EPS revisions, that take place immediately after the disclosure of short interest, mimic the information conveyed by short sellers' trades. The alternative hypothesis in this case is that analysts ignore the information content of short interest when they EPS revisions.

3. Research Design

III.a. Sample

I utilize three main datasets in my analysis: Compustat, IBES, and Markit. I start by gathering short interest data for NYSE and Nasdaq listed firms from January 1991 to December 2016. I obtain monthly short interest data from the respective exchanges for the period 1991- 2003. After 2003 all short interest data is provided by Compustat. Short interest is reported as the number of shares held short on some "effective date." My identification strategy relies on a gap between the effective date and a disclosure date. For most of the sample, the effective date for short interest across all exchanges is on or before the 15th of each month.[21] However, after September 7, 2007 exchanges were required to report short positions as of the end of the month as well. Therefore, beginning in September 2007, I have two observations per firm-month for short interest. In addition to the short interest data, I collect the disclosure dates of short interest from the WSJ and on the exchange's websites.[22] This gives me 424 disclosure dates for my sample period. Using the disclosure dates of short interest, I merge in analysts' earnings forecast and recommendation data from IBES. IBES data for recommendations (EPS estimates) begins in 1993 (1991). For both recommendations and EPS forecasts, I drop all initiations as I only measure changes around short interest disclosures. I also require that each analyst recommendation and EPS estimate happen

[21] If the 15th is a non-trading day, then Brokers/and dealers are required to report positions as of the previous trading day

[22] I thank Andrew Zhang for providing data on short interest disclosure dates

within the last 250 trading days (about half a year) to eliminate stale recommendations.[23] Finally, I obtain stock price data from CRSP.

After merging the analyst's and stock price data with the short selling data, I require non-missing stock price data for the day of the dissemination of short interest. To eliminate noise of stocks that are rarely, if ever, shorted, I drop observations where the short interest is less than 0.1% of shares outstanding as of the effective date of short interest. I drop all short interest disseminations that take place in 2008 as the financial crisis was taking place and regulators imposed short selling bans and addition restrictions on short sellers.[24] I remove short interest disseminations that happen in the 5-day window around firms' earnings announcement dates. I also require that the stock price be at least $1 on the day of short interest disclosure. After the above data requirements and excluding firms with otherwise missing data, the recommendations sample includes 536,017 firm-month observations and the EPS revision sample includes 641,132 firm-month observations.

III.b. Measuring Short Selling

A widely used measure to proxy for short selling activity is relative short interest (RSI) (Drake et al. 2011; Pownall and Simko 2005). RSI is the number of shares shorted divided by the number of shares outstanding. Following Kecskes et al. (2013), I use changes in RSI from one reporting period to the next as my key independent variable.[25] Short interest is disclosed publicly up to fourteen days after positions effective dates. Nasdaq stocks report short interest after trading hours on the 7th business day after the effective short interest date. NYSE stocks, prior to June 30, 2008,

[23] Prior literature has made a case for eliminating stale recommendations as some analysts stop covering firms. E.g. Jegadeesh and Kim (2009) and Drake et al. (2011).
[24] Rule 203 was enacted that imposed a "locate requirement" on short sellers
[25] Results don't change if I include the change over the previous three reporting periods. Suggesting that the spike itself is important information rather than a trend of changes over time

reported short interest after trading hours on the 4th business day after the effective short interest date. After June 30, 2008 FINRA took over the consolidation of all short interest and NYSE and Nasdaq stocks report short interest after trading hours on the 7th business day after the effective short interest date. Figure 1 illustrates the timing of short interest accumulation and dissemination. In the example provided, the open short selling positions as of December 12th, which settle on or before December 15th, are disclosed after the market closes on December 26th. Therefore, the first trading day where the market can react to short sellers' trade disclosure is December 27th. Under settlement rules at the time, the open short positions as of the 15th are a result of shorting activity that took place three trading days prior.[26] Therefore, short sellers trade disclosure as of the effective date is a result of positions that happened three trading days ago. As seen in the example provided by Figure 1, we have a "Settlement Lag" of 3 days and a "Disclosure Gap" of 11 days. The 14 days (in this example) between the activity of short sellers and the disclosure of their trading is what I use to alleviate the concern of confounding information and contemporaneous signals.

In secondary analysis I proxy for daily level short selling by using equity lending data. I obtain daily equity lending data from Markit, which covers a large fraction of the market for the years 2007-2016. The data includes the number of shares that are on loan at a firm-day level. Although borrowing a share could be done for reasons other than short selling, virtually all shares borrowed are due to investors initiating a short position. When using the equity lending data to measure changes in short selling, I construct a proxy for short interest, relative quantity on loan (*RQOL*) as the quantity of shares on loan scaled by the number of shares outstanding reported in CRSP. I examine how well this proxy performs by examining cross-sectional correlations of this

[26] Beginning on September 5, 2017 equity settlement rules changed, and settlement began taking place two business days after a transaction. Prior to that date, equity settlement took place three business days after a transaction. Given my sample period falls entirely in the three-day settlement window, I will utilize that convention for my analysis.

proxy and actual reported short interest. I find that the cross-sectional correlation is above 91% for both the recommendation and EPS sample.[27] Using *RQOL* as a daily measure of short interest I construct a daily change variable when I measure the change in *RQOL* from one trading day to the next. The Markit data is available to market participants but whether they use this data to make decisions is largely unexamined in the literature.[28] It is important to note that when a share of stock is shorted, settlement of that trade does not happen for 3 days. Figure 2 illustrates the timing. In the example, short positions that are initiated on December 12th are not settled until December 15th. During settlement is when the share is officially borrowed and would show up in the Markit data. Therefore, the observed changes in the equity lending market is coupled with activity that happened three trading days ago. In the example, the activity in the equity lending market on December 15th is an indication of shorting activity that took place on December 12th. I utilize the Markit data and the *RQOL* variable to examine how analysts react to the signal conveyed by short sellers trading activity, with a smaller lag (3 days) than that of short interest (up to 14 days).

III.c. Measuring Analysts Actions

I examine both the recommendation changes and EPS estimate revisions of analysts around the disclosure of short sellers trading activity. Th key dependent variables in my analysis pertain to both recommendations and EPS estimate revisions. I construct both levels of changes and a binary variable for upgraded/downgraded (upward/downward) recommendation (EPS revision). I measure analyst's recommendation activity by calculating the net recommendation changes per analyst firm pair from the day before dissemination to the three days after as:

[27] See Table 2 and discussion in section III.d.
[28] Markit sells subscriptions to their data that provides same day equity loan market conditions

$$\Delta Rec_{i,j,t} = Rec_{i,j,t+3} - Rec_{i,j,t-1} \tag{1}$$

Where $Rec_{i,j,t+3}$ is the recommendation for analyst j, covering firm i, as of 3 days after the dissemination of short interest and $Rec_{i,j,t-1}$ is the recommendation for analyst j, covering firm i, as of the day before the dissemination of short interest. In IBES, recommendation range from 1 (Stong Buy) to 5 (Sell). Therefore, changes in recommendations can range from - 4 (Sell to Strong Buy) to 4 (Strong Buy to Sell). In addition, to the changes variable I construct a binary variable for upgrade or downgrade. The variable *Upgrade* takes on the value of 1 if the recommendation change is less than or equal to -1, and zero otherwise. The variable *Downgrade* takes on the value of 1 if the recommendation change is greater than or equal to 1, and zero otherwise.

For EPS revisions, I follow the same methodology as for recommendation changes except for changes in EPS estimates I standardize the change by the share price to reduce noise associated with small or large earnings amounts.[29] The formula for changes in EPS estimates is as follows:

$$\Delta EPS_{i,j,t} = \frac{EPS_{i,j,t+3} - EPS_{i,j,t-1}}{Price_{i,t-1}} \tag{2}$$

Where $EPS_{i,j,t+3}$ is the EPS estimate for analyst j, covering firm i, as of 3 days after the dissemination of short interest, $EPS_{i,j,t-1}$ is the recommendation for analyst j, covering firm i, as of the day before the dissemination of short interest, and $Price_{i,t-1}$ is the share price as of close

[29] See Gleason and Lee (2003); Hilary and Hsu (2013); and Lim (2001)

of trading on the day before the dissemination. When measuring the EPS change in a binary fashion, I label the variables *Downward* and *Upward*.

III.d. Descriptive Statistics

I present summary statistics in Table 1 that are average cross-sectional statistics. Reported are the mean and standard deviation, along with the 5th, 25th, 50th, 75th, and 95th percentiles separately for both the recommendation (Panel A) and EPS (Panel B) samples. All of the main variables are winsorized at the 1st and 99th percentile. The average short position is about 4.4% (4.0%) of shares outstanding for the recommendation (EPS) sample. This value is much larger than the median level of 2.8% (2.5%) due to large values on the right tail of the distribution, as seen by a value of 14% (13.2%) at the 95th percentile. This short interest level is slightly higher, but comparable to that found in both Drake et al. (2011) and Kecskes et al. (2013) who find the average short interest position is 3.2% and 3.0% respectively.[30] For the recommendation sample, the *RSI_Change* has a mean value of 0.03 percent, which translates to a position change of about \$1.5 million, at the mean market value of a firm (\$5.375 billion * 0.03%). The 25th and 75th percentile of *RSI_Change* are -0.22% and 0.25%, respectively. Translating those figures to market value of equity leads to a position reduction of \$11.6 million at the 25th percentile of *RSI_Change* and a position increase of \$13.3 million at the 75th percentile of *RSI_Change*. The values for the EPS sample are very similar.

The average number of analysts that have an active recommendation for a firm when short interest is disclosed is 3.77. This number is larger than that found in other research because other articles impose more stringent filters. For example, Jegadeesh and Kim (2009) require that when

[30] The sample period used in these papers do not include more recent data, and short interest in recent years has increased as shown in Rapach Ringgenberg, and Zhou (2016) and Drake et al. (2011).

a recommendation revision takes place there must be at least two other analysts that have active recommendations on the day before the revision. Therefore, they find the mean number of analysts issuing recommendations is 7.45. Other articles impose similar sample filters for reasons pertaining to the questions their studies are answering. Since the question in this study is about how individual analysts react to short interest disclosure these filters are unnecessary. The mean consensus recommendation is 2.3 which represents a recommendation that is slightly worse than a buy (would be coded as a 2 in my sample). This is consistent with the average recommendation found in Drake et al. (2011) who find the average recommendation is slightly below a buy (coded as a 4 in their sample) with a value of 3.76.[31] Not surprisingly, the consensus recommendations are skewed toward a buy as seen by both the mean and median value being less than 3 (a Hold decision) and the 95th percentile being only slightly above a Hold decision (3.3). For the EPS sample, the mean (median) number of active estimates issued for a firm when short interest is disclosed is 6.99. The mean and median consensus estimate for EPS is \$0.30 and \$0.25, respectively.

For the recommendation sample, the mean (median) market value of equity (*MVE*) for a firm is \$5.3 billion (\$1.4 billion). Those same numbers for the EPS sample are \$4.4 billion and \$1.1 billion, respectively. The size of the firms in the sample are comparable to that of prior research studying analysts and short sellers. With a sample period of 1994 to 2006, Drake et al. (2006) find the mean (median) *MVE* to be \$3.5 billion (\$0.8 billion). The mean share price for a stock in the recommendation (EPS) sample is \$31.55 (\$29.94).

[31] IBES provides recommendation decisions that range from 1 (Strong Buy) to 5 (Sell). Some papers in the literature reverse the convention so that a 5 represents a strong buy and a 1 represents a sell.

Table 2 presents correlations for the same variables listed in Table 1 for both the recommendations (Panel A) and EPS (Panel B) samples. The correlations presented are average cross-sectional correlations. For robustness purposes I construct an alternative measure of change in short interest called *ABSS*. Similar to Pownall and Simko (2005) and Kecskes et al. (2013), I subtract from the raw change the average change in the prior year and then scale by the standard deviation of change. The correlation of *ABSS* and *RSI_Change* is 0.75 (0.74) in the recommendation (EPS Revision) sample. Another correlation that is noteworthy is the correlation between *MVE* and *NumRec* (*NumEPS*). This correlation for the recommendation (EPS) sample is 0.38 (0.49). This is consistent with larger firms having more analyst coverage for both recommendations and EPS estimates.

4. Analysis

IV.a. Univariate Results

Prior literature has documented an asymmetry between analysts' willingness to downgrade versus upgrade their recommendations, and therefore I study the decision to upgrade separately from downgrade decisions. In addition to the continuous variable for a change in short interest, I create groups based on the top and bottom quartile and decile of short interest changes. I label these variables *Top25*, *Bot25*, *Top10*, and *Bot10*. The idea is that observations where *Top25* or *Top10* take on the value of 1 are instances where short interest increased a significant amount. Therefore, the signal conveyed by short sellers is unambiguously negative as short sellers are indicating a stock is overvalued. Along those same lines the observations where *Bot25* or *Bot10* take on the value of 1 are instances where short interest decreased a significant amount (short sellers covered more positions than they initiated). The signal that is conveyed when short interest decreases is noisy relative to short interest increases because short sellers can cover for a variety of reasons, some of which are exogenous to stock price. These reasons include short sellers being squeezed out of their positions by adverse price movements and facing capital constraints. Table 3 provides descriptive statistics and univariate results on recommendation changes that take place after the dissemination of short interest based on these groups.

When comparing the *Top10* versus *Bot10* we see a one-unit downgrade is more likely than a one-unit upgrade when short interest increased, and a one-unit upgrade is more likely than a one-unit downgrade when short interest decreased. In particular, there are 10.8% more one-unit downgrades than upgrades when in the *Top10* and there are 7.3% more one-unit upgrades than downgrades when in the *Bot10*. I also calculated the weighted-average change based on the frequencies and magnitude of the changes. The weighted-average changes reveal that there is a net downgrade if short interest increases and a net upgrade when short interest decreases. Also, the magnitude of the average change is stronger when the short interest change is higher. This can be seen by comparing the average change on *Top25* (*Bot25*) of 0.024 (-0.032) to the average change on *Top10* (*Bot10*) of 0.075 (-0.046).

IV.b. Multivariate Analysis

I examine analysts' actions in the 3 trading days after short interest disclosure using separate probit models for downgrades and upgrades. Analysts' actions are affected by other factors in the market, so I include control variables to account for known predictors of recommendations. These variables are shown in equation (3). There is one observation per disclosure date for each analyst-firm pair. I denote a downgrade by analyst j, covering firm i, at time period t as $Downgrade_{i,j,t}$, which takes on the value of 1 if analyst j downgraded their recommendation for firm i, and zero otherwise. I denote an upgrade by analyst j, covering firm i, at time period t as $Upgrade_{i,j,t}$, which takes on the value of 1 if analyst j upgraded their recommendation for firm i, and zero otherwise. Equation (3) shows the model for the downgrade decision, I use the same equation for an upgrade except for the dependent variable is $Upgrade_{i,j,t}$.

$$Downgrade_{i,j,t} = \alpha_0 + \alpha_1 RSI\_Change_{i,j,t} + \alpha_2 ConRec_{i,t-1} + \alpha_3 ConRec\_Change_{i,t} + \alpha_4 NumRec_{i,t} + \alpha_5 MVE_{i,t-1} + \alpha_6 Prc_{i,t-1} + \alpha_7 Ret3_{i,t-1} + \varepsilon_{i,j,t} \quad (3)$$

In equation (3), *RSI_Change* is the change in relative short interest since the prior reporting period. *ConRec* is the consensus recommendation level as of the day before the dissemination of short interest. *ConRec_Change* is the change in the consensus recommendation in the 3 trading days leading up to the disclosure date. *NumRec* is the number of analysts that have an active recommendation of the day before the disclosure date.[32] *MVE* and *Prc* are the market value of equity and stock price for the firm as of the day before dissemination of short interest. *MVE* is calculated as the number of share outstanding times the share price. *Ret3* is the stock return for the firm leading up to the disclosure date.

Using this model, I aim to determine analysts' response to the signals conveyed by short sellers after the disclosure of their trading. In addition to the continuous variable for change in short interest, I run the same model with the binary variables based on the top and bottom quartiles and deciles of short interest changes. Including the stock level variables *ConRec*, *ConRec_Change*, and *NumRec* control for analysts' tendency to update their recommendation based on other analysts' actions or the environment for analysts covering that firm. I include *MVE* and *Prc* to

[32] For robustness I test using the number of recommendations as of the end of the previous month for robustness purposes and the main results are unchanged

control for firm characteristics that could influence analysts to act in different ways. Including *Ret3* is primarily motivated by Conrad et al. (2006), who find that analysts update their recommendations around large price changes. In all the analysis using the probit model I cluster standard errors at both the analyst level and by disclosure date period.[33]

In addition to studying recommendations, I examine analysts' EPS revisions using equation (4). As with recommendations, I study upward revisions separately from downward revisions in the 3 trading days after the disclosure of short sellers trading activity using a probit model where the decision to revise their EPS upward or downward is coded as a 1 if there is an upward or downward revision, respectively. The variable *Upward* takes on the value of 1 if the change in EPS, as calculated in equation (2), is positive, and zero otherwise. The variable *Downward* takes on the value of 1 if the change in EPS is negative, and zero otherwise.

$$Downward_{i,j,t} = \alpha_0 + \alpha_1 RSI\_Change_{i,j,t} + \alpha_2 ConEst_{i,t-1} + \alpha_3 ConEst\_Change_{i,t} + \alpha_4 NumEst_{i,t} + \alpha_5 MVE_{i,t-1} + \alpha_6 Prc_{i,t-1} + \alpha_7 Ret3_{i,t-1} + \varepsilon_{i,j,t} \quad (4)$$

Equation (4) shows the model for the downward revision, I use the same equation for an upward revision except for the dependent variable is $Upward_{i,j,t}$. The controls variables are similar to that used in the recommendation analysis except the consensus is now based on EPS estimate as of the day before disclosure of short interest. Also, the number of analysts which have an active EPS estimate replaces the number of analysts with an active recommendation. In addition to the

[33] All results are robust to clustering standard errors by industry as opposed to analyst

continuous variable for change in short interest, I run the same model with binary variables based on the top and bottom quartiles and deciles.

IV.c. Main Results

Table 4 presents the probit results for equation (3). Column 1 presents results using the continuous measure of change in short selling. Column 2 (3) presents results using a discrete level of change that falls into the top or bottom quartile (decile). Looking first at the main variable of interest, I find a positive significant relationship between analysts' actions after the disclosure of short interest and the signal conveyed by the disclosure. In particular, the coefficient on *RSI_Change* is 2.77 and is significant at the 1% level. This evidence suggests that analysts are more likely to downgrade their recommendation after a disclosed increase in short selling. In order to determine if this relationship is driven by a reduced likelihood in the event of a decrease in shorting or by an increased likelihood in the event of an increase in shorting we turn to columns 2 and 3. The results from column 2 and 3 indicate that the relationship is primarily driven by increases in short selling. In particular, the coefficient on Top25 and Top10 is 0.052 and 0.081, respectively. Both of these results are significant at the 1% level. To put this into economic terms, the results suggests that if the increase in short selling is in the top 25% (10%) then there is an increased marginal likelihood of a downgrade of about 5% (8%). When comparing these coefficients to the loadings on *Bot25* and *Bot10* we see that the coefficients on the bottom groups are much smaller in magnitude and not significant. The difference in magnitude of the coefficients for the top quartile versus top decile suggest that the larger the change in short selling the more likely analysts are to downgrade.

Consistent with the findings in Conrad et al. (2006) I find a negative and significant relationship between returns and analysts' likelihood of downgrading a firm. This can be seen by looking at the loading on Ret3, which is -0.488 and significant at the 5% level. The interpretation here is that if returns are positive for a firm, there is a decreased likelihood of an analyst covering that firm issuing a downgrade. In comparison, the economic magnitudes documented in Table 4 are along the same order, but larger than those documented in Conrad et al. (2006) who examine analysts' actions around large return events. In addition, I find that the more analysts that have active recommendations, the more likely a downgrade is. This is consistent with Hong and Kacperczyk (2010) who find that competition among analysts reduces bias. In addition, the worse the consensus recommendation to begin with, the lower the likelihood of a downgrade. This could, in part, be due to their being less bias when the consensus is worse. Also, larger firms have lower likelihoods of downgrades after the disclosure of short sellers trading. Overall, Table 4 provides evidence that analysts are willing to incorporate negative information conveyed by the trading activity disclosure of short sellers. This suggests that analysts, to some degree, value being accurate more than they value the benefits enjoyed by being overly optimistic, which include access to management and revenues from additional business generated.

The probit results for likelihood of an upgrade can be found in Table 5. The model used in this table is the same as seen in Equation (3) with the exception of the dependent variable now being $Upgrade_{i,j,t}$. There is no meaningful relationship between the change in short selling and the likelihood of issuing an upgrade recommendation. This result is consistent with prior findings on the asymmetry in analysts' actions after good versus bad news. For example, Conrad et al (2006) find that following stock price increases analysts are no more likely to upgrade versus downgrade, however, after stock price decreases analysts are much more likely to downgrade. This could be driven by analysts being overly optimistic to begin with because they want to appease management at the firm or because the signal conveyed by short interest decreases is unclear. However, analysts also value being accurate in their recommendations. Therefore, if analysts know they are overly optimistic then they may choose to ignore positive signals because the new information makes their current recommendation more accurate without action.

Taken together the results from Table 4 and Table 5 suggest that there is an asymmetry in the willingness of analysts to mimic the trading activity of short sellers. I find that analysts are willing to downgrade after a negative signal is provided, but they are not willing to upgrade after a positive signal is provided.[34] There are various reasons why an asymmetry can exist in analysts' recommendation decisions. One plausible explanation of why this asymmetry exists is that analysts can't reliably interpret positive signals conveyed by short sellers. An alternative explanation is that short sellers are viewed as informed market participants that provide negative information to the market, but not reliable positive information. Given that short sellers are known to be informed, these results suggest that an input that goes into analysts' decision making is the trading activity of short sellers. Put another way, short sellers provide information to sell-side equity analysts via the disclosure of their trades.

Next, I examine the propensity of analysts to revise their EPS estimates after the disclosure of short interest. I start by looking at the likelihood of a downward EPS revision after the disclosure of short interest. Table 6 presents the probit results for equation (4). Column 1 presents results using the continuous measure of change in short selling. Column 2 (3) presents results using a discrete level of change that falls into the top or bottom quartile (decile). I find a positive significant relationship between the likelihood analysts' revise their EPS estimates down after the disclosure of short interest and the signal conveyed by the disclosure. In particular, the coefficient on

[34] Using an alternative measure of change in short interest (*ABSS*) I find similar results. The construction of the measure is explained in section III.b.

RSI_Change is 1.994 and is significant at the 1% level. This suggests that analysts are more likely to revise their EPS estimate down after a disclosed increase in short selling. In order to determine if this relationship is driven by a reduced likelihood in the event of a decrease in shorting or by an increased likelihood in the event of an increase in shorting we turn to columns 2 and 3. The results from column 2 and 3 indicate that the relationship is driven by increases in short selling. In particular, the coefficient on Top25 and Top10 is 0.023 and 0.047, respectively. The coefficient on Top25 (Top10) is statistically significant at the 5% (1%) level. To put this into economic terms, the results suggests that if the increase in short selling is in the top 25% (10%) then there is an increased marginal likelihood of a downward EPS revision of about 2% (5%). The coefficient on *Top10* is more than twice that of the coefficient on *Top25*. This suggests that the strength of the signal conveyed by short sellers impacts the willingness of analysts to act. When comparing these coefficients to the loadings on *Bot25* and *Bot10* we see that the coefficients on the bottom groups are not significantly different from zero.

In the recommendation analysis for downgrades, the coefficient on *Ret3* is negative and significant. The interpretation is that if returns are positive then there is a reduced likelihood of a downward EPS revision. Also, the number of analysts that have an active EPS estimate is positively correlated with the likelihood of a downward revision. Unlike the recommendation analysis, firm size does not appear to be related to the likelihood of a EPS revision. Overall, Table 6 provides evidence that analysts are willing to incorporate negative information conveyed by the trading activity disclosure of short sellers when they issue EPS revisions. This suggests that analysts, to some degree, value being accurate in the EPS estimates more than they value the benefits enjoyed by being overly optimistic.

The probit results for likelihood of an upward EPS revision can be found in Table 7. The model used in this table is the same as seen in Equation (4) with the exception of the dependent variable now being $Upward_{i,j,t}$. There is not a significant relationship between the change in short selling and the likelihood of issuing an upward revision. In particular, the coefficients on the main variables of interest insignificantly differ from zero. This result is consistent with the findings in the recommendation analysis. Prior research has documented that analysts "walk down" their EPS estimates after initiation of the estimate until the earnings date (Richardson, Teoh, and Wysocki 2004). That is, analysts are overly optimistic, and the bias is reduced up until the earnings date. That being said, there is an unconditionally higher probability of a downward revision than an upward revision. This helps explain why analysts do not revise upward after the disclosure of short selling regardless of the signal being conveyed by short sellers.

Taken together the results from Table 6 and Table 7 suggest that there is an asymmetry in the willingness of analysts to mimic the trading activity of short sellers when they issue EPS revisions. I find that analysts are willing to revise down after a negative signal is provided, but they are not willing to revise up after a positive signal is provided. The reasons for why this asymmetry exist are like those provided for the recommendation analysis.

IV.d. Alternative Tests

The purpose of the tests to this point have been to identify the relationship between analysts and short sellers trading using the lagged disclosure of their trading. In more recent years (after 2006), equity loan data provides a proxy for daily level short selling that is comparable to *RSI*. For the recommendation (EPS) sample, the average *RQOL* is 4.9% (4.6%).[35] As mentioned in the data

[35] In un-tabulated results I compute the average *RSI* for observations where Markit data is available (Post 2007) and confirm that *RQOL* is a subset of *RSI* which is consistent with Markit providing data from most of the largest equity lenders, but not all.

section, the correlation between *RSI* and *RQOL* is of high importance as I am using *RQOL* as a daily-level proxy for *RSI* that can only be constructed at the monthly or bi-monthly level. The correlation reported for the recommendation (EPS) sample is 91.28% (91.40%). This correlation is measuring the cross-sectional correlation between the *RQOL*, on the effective date for reported short interest, and the *RSI* that is reported as of that same date. This correlation provides strong evidence that *RQOL* is serving as an adequate daily-level proxy for the level of short sellers' positions.

Although this data is not a direct measure of short selling, the quantity on loan is highly correlated with shares held short. In other words, information about shorting has become more knowable, though it may be costly to acquire. Therefore, analysts could infer the trading of short sellers based on the equity loan data well before the disclosure date. Whether or not analysts do this is an empirical question. To answer this question, I conduct a set of tests that rely on *RQOL_Change* as the key independent variable. This variable is constructed as a change from one trading day to the next. Giving that analysts could update this information from one trading day to the next, I shrink the window where I examine analysts' recommendations and EPS revisions to just the next trading day. In other words, the dependent variables used in this analysis are created based on equation (1) and (2) but I shorten the window from 3 trading days to 1 to eliminate information being inferred in the days in between. The results from the tests that incorporate the equity loan data are reported in Tables 8, which examine recommendations and IX, which examine EPS revisions.

Table 8 reports results for both the downgrade decision (columns 1-3) and the upgrade decision (columns 4-6). The main results reported are largely similar to those from the main specification. The coefficient on RQOL_Change for the likelihood of a downgrade is 6.891 and significant at the 1% level. The coefficient on RQOL_Change for the likelihood of an upgrade is 0.961 and not statistically significant. Taken together these results indicate that analysts' propensity to downgrade is related to the trading of short sellers from three days prior, but the likelihood of an upgrade is unrelated. I refrain from making any strong inferences from these tests as the timing of the events are very close and confounding information is of consequence.

Table 9 reports results for both downward EPS revisions (columns 1-3) and upward EPS revisions (columns 4-6). The results reported are similar to those from the main specification. The coefficient on RQOL_Change for the likelihood of a downward revision is 4.846 and significant at the 1% level. The coefficient on RQOL_Change for the likelihood of an upgrade is 2.089 and not statistically significant. The results contained in Table 9 indicate that the downward EPS revisions of analysts are correlated with increased trading activity of short sellers three days prior. Also, there is no relationship between upward EPS revisions and short sellers trading activity three days prior. Again, I refrain from making strong inferences as the three-day window between when short sellers trade and when analysts revise is not long enough to ensure they both are not acting on a common signal. Taken together, the results using the equity loan data suggest that the relationship documented around the dissemination of short interest could be an underestimate as in recent years analysts might be clued into what short sellers are doing before dissemination.

IV.e. Tests around Global Settlement and RegFD

There is reason to believe that the willingness of analysts to incorporate negative information has not been constant over time. There was speculation in the early 2000's that investment bankers had undue influence on equity analysts, and this conflict of interest led to biased public reports issued by analysts. In addition, there was speculation that managers were engaging in selective disclosure of material information. These speculations resulted in both regulatory and legal actions. Regulation FD (Reg FD) was enacted in October 2000 and the Global Analyst Research Settlement (Global Settlement) was announced in December 2002. As a result of the Global Settlement the distribution of analyst's recommendations became more balanced. Barber et al. (2006) show that analysts were more likely to issue negative recommendations in the post-regulation period compared to the pre-regulation period. Also, Reg FD has been shown to have an impact on analysts' recommendations and their earnings forecasts. Gintschel and Markov (2004) find that the price impact of recommendations and earnings forecast was reduced significantly after Reg FD. Their conclusions suggest that the regulation was successful in curtailing selective disclosure.

The impact these two particular regulatory actions had on analysts motivates testing how their willingness to incorporate negative information has changed over time. I conduct my main tests on recommendations before and after the Global Settlement and Reg FD. Given the date that the regulations happen are in close proximity to each other, I label the five-year period before Reg FD (1995-1999) as the "Pre" period and the five-year period after the Global Settlement (2003-2007) as the "Post" period. The results of these test are reported in Table 10. I run two separate specifications for the "Pre" and "Post" periods to allow for different loadings on the control variables as the structure of these relationships could change as well. The model used for these tests is like that in equation (3).

Panel A of Table 10 reports the results for the likelihood of a downgraded recommendation. Columns 1-3 report results for the "Pre" period and Columns 4-6 report results for the "Post" period. In Panel A, I find that the relationship between willingness to downgrade and the disclosed value of short interest does not exist in the "Pre" period. The coefficient on *RSI_Change* is 1.132 and is not significant. I find that the relationship between willingness to downgrade and the disclosed value of short interest does exist in the "Post" period. The coefficient on *RSI_Change* is 6.197 and is significant at the 1% level. Panel B reports results for the likelihood of an upgrade decision. The results on the upgrade decisions largely do not exist in either the "Pre" or "Post" period.

Taken together the results in Table 10 provide evidence consistent with these regulations increasing the likelihood of analysts incorporating pessimistic information into their recommendation decisions. The purpose of this paper is not to determine if these regulations were effective in removing the optimistic bias of analysts. However, the results reported are consistent with a story that analysts were overly optimistic prior to these regulations and less willing to downgrade based on available negative information, but after the regulations they are more willing to impound negative information. Worth noting is that the economic magnitude of the coefficient on *RSI_Change* for the downgrade analysis is more than two times the economic magnitude of that same coefficient from the main analysis. This suggests that the main results may underestimate the relationship in a post Reg FD and Global Settlement market.

IV.f. Falsification Test Using non-disclosure dates

The identification strategy in this paper relies heavily on the lagged disclosure of short interest coming on a date that is removed from the information environment at the time short sellers built their positions. Using this lagged disclosure alleviates concerns associated with confounding information. Also, the strategy assumes analysts find out about short sellers trading on the dissemination date itself and not before. Taking together, these two points would lead to a prediction of no relationship prior to the disclosure date itself. That is, there should be no association between *RSI_Change* and the actions of analysts prior to the discourse date of short interest. To conduct this falsification test, I slide the window that I analyze the actions of analysts back to the three days before the dissemination of short interest. In other words, I replicate the main tests but use the three days before the dissemination as a false dissemination date. At this time short sellers' positions are already established just not yet disclosed. The model used in this table is the same as seen in Equation (3) and all the controls are formed the same way as in Table 4. I report the results of the falsification test for recommendations in Table 11.

Panel A of Table 11 reports the probit results for the likelihood of a downgrade. Column 1 presents results using the continuous measure of change in short selling. Column 2 (3) presents results using a discrete level of change that falls into the top or bottom quartile (decile). Looking first at the main variable of interest, I find no significant relationship between analysts' actions after the falsified disclosure of short interest and the signal conveyed by the disclosure. The coefficient on *RSI_Change* is 0.94 and is not statistically significant. By itself, this result should give confidence that the prior results are not being driven by confounding information and there seems to be something particularly important about the disclosure date itself. Looking at the results in column 2 and 3 we see a positive relationship for both positive and negative signals from short sellers. This is particularly interesting as this suggest that analysts are more likely to downgrade a firms' stock when there is a large change in short selling regardless if it is negative or positive. This could in part be due to analysts speculating when a firm has more volatility in their stock.

Turning now to the falsification test for upgrades, we look at Panel B from Table 11. I find that there is a negative and marginally significant relationship between the change in short interest and the likelihood of an upgrade. Specifically, the coefficient on *RSI_Change* is -1.26 and is significant only at the 10% level. Given the level of significance we can't make any strong causal interpretation. In sum, when using a nearby date as a falsified dissemination date, the result on the relationship between analysts' likelihood of a downgrade and the change in short selling goes away. If the disclosure itself was the important event this is what we would expect.

5. Conclusion

This study examines analysts' willingness to incorporate information conveyed by short sellers into their stock recommendations and EPS revisions. Casual inferences on the relationship between the actions of short sellers and analysts have been elusive in the literature because of identification concerns. However, I take advantage of a large lag in the disclosure of short sellers trading to alleviate concerns of confounding information and contemporaneous signals.

I find that after the disclosure of an increase in short interest analysts have an increased propensity to downgrade a firm and issue a downward EPS revision. The fact that short interest is publicly disclosed up to fourteen days after its effective date suggests analysts are responding to the disclosure of short interest rather than private signals they discover at the same time short sellers do. Using a falsification test I find that this relation goes away when using a date near and prior to the disclosure date. This gives confidence that the disclosure date itself is important in this relationship. In other tests, I find this result does not exist in the time-period before Reg FD and the Global Settlement, which where regulations that are documented to have reduced the optimistic bias in both recommendations and EPS estimates. I find no change in the likelihood of an upgrade or upward EPS revision after a disclosed change in short selling.

In summary, this study provides evidence that sell-side equity analysts view short interest disclosures as informative about negative information. However, there is an asymmetry in that they don't interpret positive information from decreases in short interest. This asymmetry could be driven by the ambiguous nature of short interest decreases or by analysts being overly optimistic to begin with. These findings add to the literature on both analysts and short sellers. Regarding short sellers, I add to our understanding of how they can serve as information intermediaries in our equity markets. The results in this paper suggest that short sellers provide information to the equity markets through their trade disclosure. An implication of this finding is that restricting short sellers' ability to transact might have indirect consequences on analysts, and ultimately the informational efficiency of price. Also, the results in this paper suggest that more frequent trade disclosure of short sellers could result in better informational efficiency. Regarding analysts, I add to our understanding of inputs in the "black box" of analysts. Also, I add to prior findings that analysts are asymmetric in their actions which suggests they can have incentives that lead to less accuracy. The asymmetry documented in this paper provide evidence that analysts have the skill to interpret negative signals conveyed by increases in short interest, but not when it comes to decreases in short interest. Understanding the effect short sellers can have on analysts' actions can have major implications for the informational content of analysts' recommendations, EPS estimates, and reports.

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Appendix

Table 2.1 Sample Summary Statistics

This table provides summary statistics for short interest disclosure events in the sample. Panel A presents results from the recommendations sample which runs from 1993 to 2016. Panel B presents results from the EPS revisions sample which runs from 1991 to 2016. The statistics provided are average cross-sectional statistics taken at the disclosure dates of short interest across all firms. The variable *RSI* is the relative short interest, calculated as the number of shares held short in a firm divided by the number of shares outstanding. *NumRec* is the number of recommendations that are active for a given firm at the disclosure of short selling. *ConRec* is the average recommendation across all analysts following a given firm at the time of disclosure of short interest. *NumEst* is the number of EPS estimates that are active for a given firm at the disclosure of short selling. *ConEst* is the average EPS estimate across all analysts following a given firm at the time of disclosure of short interest. *RSI_Change* is the change in *RSI* from the prior period to the current. *PRC* is the share price for a firm as of the day before the disclosure of short interest. *Ret3* is the three-day return for a given firm leading up to the disclosure of short interest. *MVE* is the market value of equity for a firm as of the day before the disclosure of short interest, which is calculated as the share price times the number of shares outstanding. The statistics reported are the mean, standard deviation (SD), and the 5th, 25th, 50th, 75th, and 95th percentile. I winsorize all variables at the 1% level.

Panel A: Recommendation Sample

	Mean	SD	P5	P25	P50	P75	P95
RSI (%)	4.353	4.595	0.496	1.431	2.787	5.473	13.998
NumRec	3.768	2.881	1.000	1.526	2.874	5.110	9.667
ConRec	2.314	0.673	1.028	1.892	2.313	2.839	3.305
RSI_Change (%)	0.028	0.718	-1.060	-0.217	0.008	0.248	1.198
RET3 (%)	0.020	4.028	-6.281	-2.071	-0.065	1.995	6.680
PRC ($)	31.55	23.27	5.55	14.80	26.00	42.05	77.45
MVE ($millions)	5,375	12,791	132	498	1,357	4,169	24,789
N	536,017						

Panel B: EPS Sample

	Mean	SD	P5	P25	P50	P75	P95
RSI (%)	3.999	0.044	0.004	0.013	0.025	0.050	0.132
NumEst	6.987	5.721	1.014	2.668	5.260	9.782	18.745
ConEst ($)	0.30	0.39	-0.22	0.09	0.25	0.47	0.98
RSI_Change (%)	0.026	0.007	-0.010	-0.002	0.000	0.002	0.011
RET3 (%)	0.011	4.100	-6.419	-2.113	-0.070	2.023	6.790
PRC ($)	29.94	22.32	5.19	13.89	24.46	39.91	73.90
MVE ($millions)	4,419	10,668	108	396	1,086	3,332	20,490
N	641,132						

Table 2.2 Sample Correlation Matrix

This table provides correlations for key variables in the analysis. Panel A represents results for the recommendations sample. Panel B presents results for the EPS estimates sample. The statistics reported are average cross-sectional correlations. The variable RSI is the relative short interest, calculated as the number of shares held short in a firm divided by the number of shares outstanding. *NumRec* is the number of recommendations that are active for a given firm at the disclosure of short selling. *ConRec* is the average recommendation across all analysts following a given firm at the time of disclosure of short interest. *NumEst* is the number of EPS estimates that are active for a given firm at the disclosure of short selling. *ConEst* is the average EPS estimate across all analysts following a given firm at the time of disclosure of short interest. *RSI_Change* is the change in RSI from the prior period to the current. *ABSS* measures an abnormal level of short interest. It is calculated by taking the change in short interest less the average change over the prior year, divided by the standard deviation of change over the prior year. *PRC* is the share price for a firm as of the day before the disclosure of short interest. *MVE* is the market value of equity for a firm as of the day before the disclosure of short interest, which is calculated as the share price times the number of shares outstanding.

Panel A: Recommendation Sample

	ABSS	*PRC*	*ConRec*	*MVE*	*NumRec*	*RSI*	*RSI_Change*
ABSS	1.0000						
PRC	-0.0061	1.0000					
ConRec	-0.0211	-0.0383	1.0000				
MVE	-0.0076	0.3726	-0.0106	1.0000			
NumRec	-0.0196	0.2585	0.0674	0.3753	1.0000		
RSI	0.0023	-0.0444	0.0589	-0.1455	0.0999	1.0000	
RSI_Change	0.7522	0.0001	-0.0213	-0.0100	-0.0031	0.1198	1.0000

Panel B: EPS Sample

	ABSS	*PRC*	*ConRec*	*MVE*	*NumEst*	*RSI*	*RSI_Change*
ABSS	1.000						
PRC	-0.007	1.000					
ConEPS	0.005	0.530	1.000				
MVE	-0.008	0.393	0.207	1.000			
NumEst	-0.021	0.336	0.171	0.485	1.000		
RSI	0.000	-0.024	-0.116	-0.125	0.100	1.000	
RSI_Change	0.742	0.004	0.004	-0.009	-0.008	0.121	1.000

Table 2.3 Descriptive Statistics on Recommendation Change

This table presents descriptive statistics of recommendation changes around the disclosure of short interest. I summarize the recommendation changes based on which of the four groups the short interest changes fall into. These groups have the following names: *Bot10*, *Bot25*, *Top10*, and *Top25*. If the change in short interest is in the bottom 10th percentile, then you are classified into the group *Bot10*. If the change in short interest is in the bottom 25th percentile, then you are classified into the group *Bot25*. If the change in short interest is in the top 10th percentile, then you are classified into the group *Top10*. If the change in short interest is in the top 25th percentile, then you are classified into the group *Top25*. Panel A presents the results for the short interest increases that are in the *Top25* and *Top10*. Panel B presents the results for the short interest decreases that are in the *Bot25* and *Bot10*. The recommendations are coded as follows: 1 equates to a Strong Buy, 2 equates to a Buy, 3 equates to a Hold, 4 equates to an Underperform, and 5 equates to a Sell. A positive change in recommendation indicates a downgrade, and vice versa for a negative change. The average recommendation change provided in the table is computed by multiplying the frequency of change by the magnitude of the change (computed as the difference in previous recommendation and new recommendation (ranges from -4 to 4)).

Panel A

	***Top25* Sample**		***Top10* Sample**	
Rec Change	**Frequency**	**% Total**	**Frequency**	**% Total**
Upgrade 4	13	0.0021	5	0.0021
Upgrade 3	8	0.0013	3	0.0012
Upgrade 2	520	0.0854	206	0.0846
Upgrade 1	998	0.1639	416	0.1708
No Change	605,863	99.4792	242,247	99.4389
Downgrade 1	1059	0.1739	461	0.1892
Downgrade 2	538	0.0883	257	0.1055
Downgrade 3	21	0.0034	11	0.0045
Downgrade 4	15	0.0025	8	0.0033
	Avg. Change	0.0236	Avg. Change	0.0751

Panel B

	***Bot25* Sample**		***Bot10* Sample**	
Rec Change	**Frequency**	**% Total**	**Frequency**	**% Total**
Upgrade 4	9	0.0015	4	0.0016
Upgrade 3	18	0.0030	9	0.0037
Upgrade 2	519	0.0852	226	0.0928
Upgrade 1	1018	0.1671	409	0.1679
No Change	606,050	99.5099	242,389	99.4963
Downgrade 1	920	0.1511	381	0.1564
Downgrade 2	480	0.0788	188	0.0772
Downgrade 3	11	0.0018	4	0.0016
Downgrade 4	10	0.0016	6	0.0025
	Avg. Change	-0.0317	Avg. Change	-0.0456

Table 2.4 Probit Analysis Likelihood of a Downgrade

This table provides results from a Probit analysis on the likelihood of a downgrade. The model used in this table is represented in equation (3). The observation level is at the analyst-firm pair level. The dependent variable in all columns is $Downgrade_{i,j,t}$, which is a binary variable that measures whether a given analysts downgraded their recommendation for a firm. The recommendations are coded as follows: 1 equates to a Strong Buy, 2 equates to a Buy, 3 equates to a Hold, 4 equates to an Underperform, and 5 equates to a Sell. A positive change in recommendation indicates a downgrade. $Downgrade_{i,j,t}$ takes on the value of 1 if the change in recommendation from prior to the disclosure, to three days after is positive. It is calculated as seen in equation (1). *RSI_Change* is the change in *RSI* from the prior period to the current. *Top25* and *Bot25* are dummy variables that equal 1 when the *RSI_Change* is at or above the 75th percentile or at or below the 25th percentile, respectively. *Top10* and *Bot10* are dummy variables that equal 1 when the *RSI_Change* is at or above the 90th percentile or at or below the 10th percentile, respectively. Standard errors are clustered by both the analyst and time dimension. The z-statistics are reported in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01

	(1)	(2)	(3)
RSI_Change	2.7782***		
	(3.06)		
Top25		0.0515***	
		(3.95)	
Bot25		0.0193*	
		(1.65)	
Top10			0.0808***
			(4.93)
Bot10			0.0233
			(1.39)
ConRec	-0.2104***	-0.2112***	-0.2117***
	(-20.48)	(-20.63)	(-20.70)
ConRec_Change	-0.4903***	-0.4913***	-0.4903***
	(-4.18)	(-4.19)	(-4.19)
MVE	-0.0011***	-0.0009***	-0.0009***
	(-4.32)	(-3.91)	(-4.01)
Prc	-0.0005***	-0.0005***	-0.0005***
	(-3.27)	(-3.27)	(-3.20)
NumRec	0.0065***	0.0061***	0.0061***
	(5.08)	(4.74)	(4.75)
Ret3	-0.4884**	-0.4897**	-0.4896**
	(-1.97)	(-1.99)	(-1.99)
Constant	-2.3519***	-2.3664***	-2.3586***
	(-87.16)	(-85.30)	(-86.89)
Clusterd SE's	Yes	Yes	Yes
Observations	2,105,508	2,105,508	2,105,508

Table 2.5 Probit Analysis Likelihood of an Upgrade

This table provides results from a Probit analysis on the likelihood of an upgrade. The model used in this table is represented in equation (3). The observation level is at the analyst-firm pair level. The dependent variable in all columns is $Upgrade_{i,j,t}$, which is a binary variable that measures whether a given analysts upgraded their recommendation for a firm. The recommendations are coded as follows: 1 equates to a Strong Buy, 2 equates to a Buy, 3 equates to a Hold, 4 equates to an Underperform, and 5 equates to a Sell. A negative change in recommendation indicates an upgrade. $Upgrade_{i,j,t}$ takes on the value of 1 if the change in recommendation from prior to the disclosure, to three days after is negative. It is calculated as seen in equation (1). *RSI_Change* is the change in *RSI* from the prior period to the current. *Top25* and *Bot25* are dummy variables that equal 1 when the *RSI_Change* is at or above the 75th percentile or at or below the 25th percentile, respectively. *Top10* and *Bot10* are dummy variables that equal 1 when the *RSI_Change* is at or above the 90th percentile or at or below the 10th percentile, respectively. Standard errors are clustered by both the analyst and time dimension. The z-statistics are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

	(1)	(2)	(3)
RSI_Change	0.5836		
	(0.70)		
Top25		0.0229*	
		(1.87)	
Bot25		0.0185	
		(1.53)	
Top10			0.0114
			(0.72)
Bot10			0.0115
			(0.75)
ConRec	0.1677***	0.1671***	0.1672***
	(17.63)	(17.53)	(17.54)
ConRec_Change	-0.0133	-0.0135	-0.0135
	(-0.07)	(-0.07)	(-0.07)
MVE	-0.0005***	-0.0004**	-0.0005***
	(-2.82)	(-2.44)	(-2.68)
Prc	-0.0004***	-0.0004***	-0.0004***
	(-2.77)	(-2.76)	(-2.75)
NumRec	0.0030***	0.0028**	0.0029**
	(2.63)	(2.43)	(2.55)
Ret3	0.9770***	0.9724***	0.9741***
	(3.62)	(3.61)	(3.62)
Constant	-3.2459***	-3.2542***	-3.2468***
	(-122.89)	(-121.40)	(-123.10)
Clusterd SE's	Yes	Yes	Yes
Observations	2,105,508	2,105,508	2,105,508

Table 2.6 Probit Analysis Likelihood of a Downward EPS Revision

This table provides results from a Probit analysis on the likelihood of a downward EPS revision. The model used in this table is represented in equation (4). The observation level is at the analyst-firm pair level. The dependent variable in all columns is $Downward_{i,j,t}$, which is a binary variable that measures whether a given analysts revised their EPS estimate down for a firm. $Downward_{i,j,t}$ takes on the value of 1 if the change in an analysts' EPS estimate from the day prior to the short interest disclosure, to three days after, is negative. The change is calculated as seen in equation (2). *RSI_Change* is the change in *RSI* from the prior period to the current. *Top25* and *Bot25* are dummy variables that equal 1 when the *RSI_Change* is at or above the 75th percentile or at or below the 25th percentile, respectively. *Top10* and *Bot10* are dummy variables that equal 1 when the *RSI_Change* is at or above the 90th percentile or at or below the 10th percentile, respectively. Standard errors are clustered by both the analyst and time dimension. The z-statistics are reported in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01

	(1)	(2)	(3)
RSI_Change	1.9935***		
	(2.62)		
Top25		0.0227**	
		(2.31)	
Bot25		-0.0005	
		(-0.05)	
Top10			0.0466***
			(4.00)
Bot10			0.0059
			(0.49)
ConEst	0.0000**	0.0000*	0.0000*
	(1.96)	(1.95)	(1.92)
ConEst_Change	0.0001*	0.0001*	0.0001*
	(1.93)	(1.91)	(1.88)
MVE	0.0000	0.0000	0.0001
	(0.16)	(0.38)	(0.44)
Prc	-0.0000	-0.0000	-0.0000
	(-0.34)	(-0.33)	(-0.32)
NumEst	0.0060***	0.0060***	0.0060***
	(9.91)	(9.76)	(9.76)
Ret3	-1.7870***	-1.7878***	-1.7859***
	(-6.48)	(-6.49)	(-6.49)
Constant	-2.1216***	-2.1265***	-2.1262***
	(-142.98)	(-136.46)	(-141.43)
Clusterd SE's	Yes	Yes	Yes
Observations	4,855,397	4,855,397	4,855,397

Table 2.7 Probit Analysis Likelihood of an Upward EPS Revision

This table provides results from a Probit analysis on the likelihood of an upward EPS revision. The model used in this table is represented in equation (4). The observation level is at the analyst-firm pair level. The dependent variable in all columns is $Upward_{i,j,t}$, which is a binary variable that measures whether a given analysts revised their EPS estimate up for a firm. $Upward_{i,j,t}$ takes on the value of 1 if the change in an analysts' EPS estimate from the day prior to the short interest disclosure, to three days after, is positive. The change is calculated as seen in equation (2). *RSI_Change* is the change in *RSI* from the prior period to the current. *Top25* and *Bot25* are dummy variables that equal 1 when the *RSI_Change* is at or above the 75th percentile or at or below the 25th percentile, respectively. *Top10* and *Bot10* are dummy variables that equal 1 when the *RSI_Change* is at or above the 90th percentile or at or below the 10th percentile, respectively. Standard errors are clustered by both the analyst and time dimension. The z-statistics are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

	(1)	(2)	(3)
RSI_Change	-0.8404		
	(-1.09)		
Top25		-0.0129	
		(-1.15)	
Bot25		-0.0065	
		(-0.57)	
Top10			-0.0212
			(-1.45)
Bot10			-0.0074
			(-0.52)
ConEst	0.0000**	0.0000**	0.0000**
	(2.19)	(2.20)	(2.20)
ConEst_Change	0.0000**	0.0000**	0.0000**
	(2.04)	(2.07)	(1.96)
MVE	0.0003*	0.0002*	0.0002*
	(1.81)	(1.67)	(1.70)
Prc	0.0000	0.0000	0.0000
	(1.20)	(1.19)	(1.19)
NumEst	0.0045***	0.0045***	0.0045***
	(7.20)	(7.09)	(7.13)
Ret3	1.5071***	1.5097***	1.5098***
	(4.81)	(4.82)	(4.82)
Constant	-2.0547***	-2.0504***	-2.0523***
	(-99.94)	(-94.87)	(-97.76)
Clusterd SE's	Yes	Yes	Yes
Observations	4,855,397	4,855,397	4,855,397

Table 2.8 Likelihood of a Recommendation Change- Equity Loan Data

This table provides results from a Probit analysis on the likelihood of a recommendation change. The model used in this table is represented in equation (3). The observation level is at the analyst-firm pair level. The dependent variable in columns 1-3 is *Downgrade*$_{i,j,t}$, which is a binary variable that measures whether a given analysts downgraded their recommendation for a firm. The dependent variable in columns 4-6 is *Upgrade*$_{i,j,t}$, which is a binary variable that measures whether a given analysts upgraded their recommendation for a firm. The recommendations are coded as follows: 1 equates to a Strong Buy, 2 equates to a Buy, 3 equates to a Hold, 4 equates to an Underperform, and 5 equates to a Sell. A positive change in recommendation indicates a downgrade and a negative change indicates an upgrade. These changes are calculated in equation (1). *RQOL_Change* is the change in *RQOL* from one trading day to the next. *Top25* and *Bot25* are dummy variables that equal 1 when the *RQOL_Change* is at or above the 75th percentile or at or below the 25th percentile, respectively. *Top10* and *Bot10* are dummy variables that equal 1 when the *RQOL_Change* is at or above the 90th percentile or at or below the 10th percentile, respectively. Standard errors are clustered by both the analyst and time dimension. The z-statistics are reported in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01

	Downgrade = 1			*Upgrade* = 1		
	(1)	(2)	(3)	(4)	(5)	(6)
RQOL_Change	6.8782***			0.9265		
	(4.16)			(0.56)		
Top25		0.0879***			0.0311***	
		(11.10)			(4.01)	
Bot25		0.0596***			0.0324***	
		(7.42)			(3.95)	
Top10			0.1085***			0.0592***
			(11.07)			(5.80)
Bot10			0.0679***			0.0499***
			(6.73)			(4.93)
ConRec	-0.1318***	-0.1349***	-0.1347***	0.2146***	0.2133***	0.2128***
	(-16.84)	(-17.27)	(-17.29)	(38.20)	(37.87)	(37.84)
ConRec_Change	-0.2307	-0.2290	-0.2284	-0.1318	-0.1312	-0.1317
	(-0.82)	(-0.82)	(-0.82)	(-0.47)	(-0.47)	(-0.47)
MVE	-0.0009***	-0.0006***	-0.0007***	-0.0003*	-0.0002	-0.0002
	(-4.36)	(-3.42)	(-3.71)	(-1.81)	(-1.25)	(-1.23)
Prc	0.0000	0.0000	0.0000	-0.0003***	-0.0003***	-0.0003***
	(0.20)	(0.22)	(0.19)	(-2.97)	(-2.94)	(-2.85)
NumRec	0.0061***	0.0050***	0.0052***	0.0046***	0.0042***	0.0040***
	(4.13)	(3.40)	(3.55)	(3.57)	(3.14)	(3.08)
Ret3	-2.7854***	-2.7645***	-2.7599***	3.4938***	3.4806***	3.4753***
	(-16.75)	(-16.77)	(-16.75)	(20.13)	(20.10)	(20.12)
Constant	-2.8453***	-2.8727***	-2.8546***	-3.6867***	-3.6980***	-3.6918***
	(-89.46)	(-90.57)	(-90.44)	(-177.52)	(-181.11)	(-179.15)
Clusterd SE's	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,753,640	10,753,640	10,753,640	10,753,640	10,753,640	10,753,640

Table 2.9 Likelihood of an EPS Revision- Equity Loan Data

This table provides results from a Probit analysis on the likelihood of an EPS revision. The model used in this table is represented in equation (4). The observation level is at the analyst-firm pair level. The dependent variable in columns 1-3 is $Downward_{i,j,t}$, which is a binary variable that measures whether a given analysts revised their EPS estimate down. The dependent variable in columns 4-6 is $Upgrade_{i,j,t}$, which is a binary variable that measures whether a given analysts revised their EPS estimate up. A negative change in an EPS estimate indicates a downward revision and a positive change indicates an upward revision. These changes are calculated in equation (2). *RQOL_Change* is the change in *RQOL* from one trading day to the next. *Top25* and *Bot25* are dummy variables that equal 1 when the *RQOL_Change* is at or above the 75th percentile or at or below the 25th percentile, respectively. *Top10* and *Bot10* are dummy variables that equal 1 when the *RQOL_Change* is at or above the 90th percentile or at or below the 10th percentile, respectively. Standard errors are clustered by both the analyst and time dimension. The z-statistics are reported in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01

	Downward = 1			*Upward* = 1		
	(1)	(2)	(3)	(4)	(5)	(6)
RQOL_Chg	4.8464***			2.0891		
	(3.53)			(1.37)		
Top25		0.0224***			0.0073	
		(3.49)			(0.96)	
Bot25		-0.0007			-0.0017	
		(-0.11)			(-0.24)	
Top10			0.0309***			0.0115
			(3.76)			(1.25)
Bot10			0.0101			-0.0128
			(1.23)			(-1.48)
ConEst	0.0000***	0.0000***	0.0000***	-0.0000***	-0.0000***	-0.0000***
	(3.71)	(3.72)	(3.72)	(-3.59)	(-3.58)	(-3.59)
ConEst_Chg	-0.0001*	-0.0001*	-0.0001*	-0.0112***	-0.0112***	-0.0112***
	(-1.72)	(-1.71)	(-1.72)	(-2.63)	(-2.63)	(-2.63)
MVE	0.0003***	0.0003***	0.0003***	0.0004***	0.0004***	0.0004***
	(3.18)	(3.52)	(3.52)	(4.58)	(4.77)	(4.61)
Prc	-0.0000	-0.0000	-0.0000	0.0000***	0.0000***	0.0000***
	(-0.70)	(-0.69)	(-0.70)	(3.00)	(3.01)	(3.00)
NumEst	0.0113***	0.0113***	0.0113***	0.0138***	0.0138***	0.0138***
	(17.79)	(17.72)	(17.72)	(19.14)	(19.19)	(19.15)
Ret3	-2.3917***	-2.3889***	-2.3894***	2.1769***	2.1760***	2.1773***
	(-14.24)	(-14.24)	(-14.24)	(13.43)	(13.43)	(13.43)
Constant	-2.6471***	-2.6524***	-2.6511***	-2.8015***	-2.8029***	-2.8015***
	(-258.87)	(-244.57)	(-254.15)	(-232.61)	(-220.91)	(-229.09)
Clusterd SE's	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,155,265	8,155,265	8,155,265	8,155,265	8,155,265	8,155,265

Table 2.10 Likelihood of a Recommendation Change- Around Global Settlement and RegFD

This table provides results from a Probit analysis on the likelihood of a recommendation change before and after the Global Settlement and Reg FD. The model used in this table is represented in equation (3). The observation level is at the analyst-firm pair level. The dependent variable in Panel A is $Downgrade_{i,j,t}$, which is a binary variable that measures whether a given analysts downgraded their recommendation for a firm. The dependent variable in Panel B is $Upgrade_{i,j,t}$, which is a binary variable that measures whether a given analysts upgraded their recommendation for a firm. In both panels. Columns 1-3 represent the “Pre” regulation period which spans from 1995 - 1999, and columns 4-6 represent the “Post” regulation period which spans from 2003 - 2007. The recommendations are coded as follows: 1 equates to a Strong Buy, 2 equates to a Buy, 3 equates to a Hold, 4 equates to an Underperform, and 5 equates to a Sell. The recommendations are coded as follows: 1 equates to a Strong Buy, 2 equates to a Buy, 3 equates to a Hold, 4 equates to an Underperform, and 5 equates to a Sell. A positive change in recommendation indicates a downgrade and a negative change indicates an upgrade. These changes are calculated in equation (1). *RSI_Change* is the change in *RSI* from the prior period to the current. *Top25* and *Bot25* are dummy variables that equal 1 when the *RSI_Change* is at or above the 75th percentile or at or below the 25th percentile, respectively. *Top10* and *Bot10* are dummy variables that equal 1 when the *RSI_Change* is at or above the 90th percentile or at or below the 10th percentile, respectively. Standard errors are clustered by both the analyst and time dimension. The z-statistics are reported in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01

Panel A

Dependent Variable	*Downgrade*			*Downgrade*		
Sample	Pre Reg FD and Global Settlement			Post Reg FD and Global Settlement		
	(1)	(2)	(3)	(4)	(5)	(6)
RSI_Chg	1.1321			6.1972***		
	(0.49)			(3.04)		
Top25		0.0039			0.0865***	
		(0.12)			(3.11)	
Bot25		-0.0090			0.0237	
		(-0.34)			(0.82)	
Top10			0.0250			0.1285***
			(0.63)			(3.81)
Bot10			0.0056			-0.0111
			(0.13)			(-0.32)
ConRec	-0.2247***	-0.2249***	-0.2243***	-0.1971***	-0.1972***	-0.1983***
	(-9.68)	(-9.60)	(-9.64)	(-10.63)	(-10.63)	(-10.71)
ConRec_Chg	-0.6497***	-0.6499***	-0.6495***	-0.2679	-0.2714	-0.2693
	(-5.23)	(-5.24)	(-5.24)	(-0.69)	(-0.70)	(-0.70)
MVE	-0.0012	-0.0013	-0.0012	-0.0020***	-0.0018***	-0.0018***
	(-1.59)	(-1.64)	(-1.56)	(-2.92)	(-2.60)	(-2.70)
Prc	0.0000	0.0000	0.0000	-0.0005	-0.0005	-0.0005
	(0.31)	(0.31)	(0.31)	(-0.76)	(-0.70)	(-0.70)
NumRec	0.0087***	0.0087***	0.0086***	0.0028	0.0023	0.0026
	(2.87)	(2.82)	(2.78)	(0.96)	(0.78)	(0.89)
Ret3	-0.1500	-0.1510	-0.1453	-0.6257	-0.6329	-0.6255
	(-0.37)	(-0.38)	(-0.36)	(-1.24)	(-1.26)	(-1.24)
Constant	-2.3097***	-2.3080***	-2.3123***	-2.3422***	-2.3692***	-2.3547***
	(-43.61)	(-41.49)	(-43.38)	(-40.21)	(-40.55)	(-40.93)
Clusterd SE's	Yes	Yes	Yes	Yes	Yes	Yes
Observations	300,958	300,958	300,958	449,232	449,232	449,232

Panel B

Dependent Variable	Upgrade			Upgrade		
Sample	Pre Reg FD and Global Settlement			Post Reg FD and Global Settlement		
	(1)	(2)	(3)	(4)	(5)	(6)
RSI_Chg	0.0202			0.4274		
	(0.01)			(0.26)		
Top25		0.0317			0.0214	
		(1.09)			(0.86)	
Bot25		0.0598**			0.0016	
		(2.21)			(0.06)	
Top10			0.0163			0.0114
			(0.41)			(0.38)
Bot10			-0.0024			0.0203
			(-0.06)			(0.64)
ConRec	0.1739***	0.1761***	0.1742***	0.1728***	0.1729***	0.1726***
	(6.05)	(6.23)	(6.12)	(10.99)	(10.99)	(10.96)
ConRec_Chg	-0.1668	-0.1640	-0.1670	-0.3305	-0.3309	-0.3301
	(-0.77)	(-0.76)	(-0.77)	(-0.90)	(-0.90)	(-0.90)
MVE	0.0002	0.0004	0.0002	0.0001	0.0001	0.0001
	(0.38)	(0.75)	(0.41)	(0.14)	(0.25)	(0.23)
Prc	0.0000	0.0000	0.0000	-0.0001	-0.0000	-0.0000
	(0.14)	(0.14)	(0.15)	(-0.09)	(-0.07)	(-0.07)
NumRec	0.0063**	0.0057*	0.0063**	0.0013	0.0012	0.0012
	(1.99)	(1.82)	(1.97)	(0.62)	(0.61)	(0.59)
Ret3	1.7387***	1.7385***	1.7397***	0.5624	0.5622	0.5589
	(3.91)	(3.93)	(3.92)	(1.10)	(1.10)	(1.09)
Constant	-3.2168***	-3.2395***	-3.2185***	-3.2740***	-3.2811***	-3.2774***
	(-57.44)	(-61.15)	(-58.78)	(-68.37)	(-64.84)	(-68.33)
Clusterd SE's	Yes	Yes	Yes	Yes	Yes	Yes
Observations	300,958	300,958	300,958	449,232	449,232	449,232

Table 2.11 Probit Analysis Falsification Test

This table provides results from a Probit analysis on the likelihood of a recommendation change using a falsified dissemination date for short interest. The date window used to capture analysts' recommendation changes is the three days prior to the dissemination. The model used in this table is represented in equation (3). The observation level is at the analyst-firm pair level. The dependent variable in Panel A is $Downgrade_{i,j,t}$, which is a binary variable that measures whether a given analysts downgraded their recommendation for a firm. The dependent variable in Panel B is $Upgrade_{i,j,t}$, which is a binary variable that measures whether a given analysts upgraded their recommendation for a firm. The recommendations are coded as follows: 1 equates to a Strong Buy, 2 equates to a Buy, 3 equates to a Hold, 4 equates to an Underperform, and 5 equates to a Sell. The recommendations are coded as follows: 1 equates to a Strong Buy, 2 equates to a Buy, 3 equates to a Hold, 4 equates to an Underperform, and 5 equates to a Sell. A positive change in recommendation indicates a downgrade and a negative change indicates an upgrade. More details on the formation of the dependent variables for the falsification test can be found in Section IV.f. *RSI_Change* is the change in *RSI* from the prior period to the current. *Top25* and *Bot25* are dummy variables that equal 1 when the *RSI_Change* is at or above the 75th percentile or at or below the 25th percentile, respectively. *Top10* and *Bot10* are dummy variables that equal 1 when the *RSI_Change* is at or above the 90th percentile or at or below the 10th percentile, respectively. Standard errors are clustered by both the analyst and time dimension. The z-statistics are reported in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01

Panel A

	(1)	(2)	(3)
RSI_Change	0.8601		
	(1.10)		
Top25		0.0293***	
		(2.87)	
Bot25		0.0314***	
		(2.81)	
Top10			0.0591***
			(4.27)
Bot10			0.0339**
			(2.31)
ConRec	-0.2023***	-0.2029***	-0.2030***
	(-22.43)	(-22.51)	(-22.53)
ConRec_Change	-0.2928**	-0.2927**	-0.2918**
	(-2.37)	(-2.36)	(-2.35)
MVE	-0.0036***	-0.0032***	-0.0032***
	(-7.91)	(-7.14)	(-7.07)
Prc	-0.0003**	-0.0003**	-0.0003**
	(-1.97)	(-2.04)	(-1.99)
NumRec	0.0087***	0.0082***	0.0082***
	(7.80)	(7.24)	(7.23)
Ret3	-1.3842***	-1.3806***	-1.3768***
	(-7.44)	(-7.44)	(-7.43)
Constant	-2.2597***	-2.2724***	-2.2666***
	(-94.69)	(-94.03)	(-95.32)
Clusterd SE's	Yes	Yes	Yes
Observations	2,282,261	2,282,261	2,282,261

Panel B

	(1)	(2)	(3)
RSI_Change	-1.2589*		
	(-1.68)		
Top25		-0.0119	
		(-1.07)	
Bot25		0.0111	
		(0.97)	
Top10			0.0094
			(0.65)
Bot10			0.0213
			(1.52)
ConRec	0.1722***	0.1722***	0.1721***
	(20.05)	(20.07)	(20.04)
ConRec_Change	0.0028	0.0029	0.0033
	(0.02)	(0.02)	(0.02)
MVE	-0.0006	-0.0006	-0.0005
	(-1.42)	(-1.39)	(-1.12)
Prc	-0.0003**	-0.0003**	-0.0003**
	(-2.44)	(-2.44)	(-2.44)
NumRec	0.0043***	0.0043***	0.0042***
	(3.78)	(3.78)	(3.58)
Ret3	1.0271***	1.0271***	1.0274***
	(4.58)	(4.58)	(4.59)
Constant	-3.1640***	-3.1639***	-3.1665***
	(-130.91)	(-126.63)	(-130.80)
Clusterd SE's	Yes	Yes	Yes
Observations	2,286,261	2,286,261	2,286,261

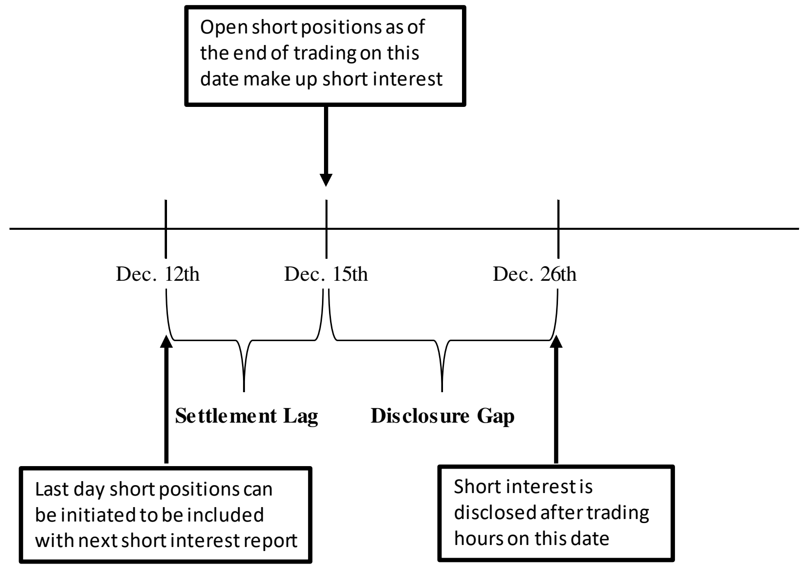

Figure 2.1 An example of the timeline of events for the disclosure of short interest

This figure provides an example of the timing that takes place and that we consider when constructing our key variables and our sample. In this example, the Short Interest Period (seen above) is from August 30th to September 15th. The SEC will report the aggregate amount of short interest per firm as of 15th of September, we refer to this level as Short Interest, t-1. They will release the short interest level as of the end of the end of trading on the 15th on September 25th because they wait eight trading days after the end of the Short Interest Period before they release said information, we refer to this period as the Waiting Period (seen above). From September 25th until October 10th there will be no additional short selling information released by the SEC and therefore insiders will only have the information about the level of short selling based on what was released on the September 25th. I study the transactions of insiders from September 25th to October 10th, a period we refer to as the Observation Period (seen above) and aggregate them in order to construct the Net Insider Transactions, t variable.

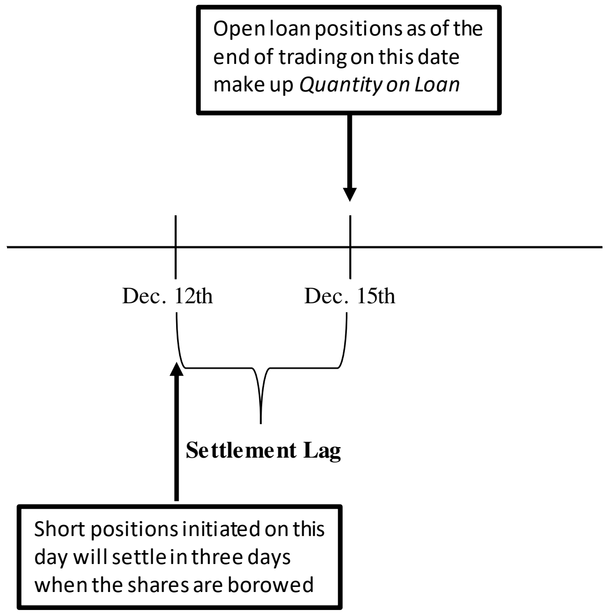

Figure 2.2 An example of the timeline of events for the equity loan data

This figure provides an example of the timing that takes place and that we consider when constructing our key variables and our sample. In this example, the short interest period (seen above) is from august 30th to September 15th. The sec will report the aggregate amount of short interest per firm as of 15th of September, we refer to this level as short interest, t-1. They will release the short interest level as of the end of the end of trading on the 15th on September 25th because they wait eight trading days after the end of the short interest period before they release said information, we refer to this period as the waiting period (seen above). From September 25th until October 10th there will be no additional short selling information released by the sec and therefore insiders will only have the information about the level of short selling based on what was released on the September 25th. I study the transactions of insiders from September 25th to October 10th, a period we refer to as the observation period (seen above) and aggregate them in order to construct the net insider transactions, t variable.

CONCLUSION

In this dissertation, I examine the role that short sellers play in our equity markets. In particular, I examine how their presence can improve liquidity for the entire market and how they serve as informational intermediaries. In the first chapter of the dissertation we investigate how restricting the ability of short sellers impacts market quality. Specifically, we examine how exogenously limiting the ability of short sellers to establish their position impacts liquidity costs. We find evidence that when short sellers are constrained liquidity costs rise, particularly for stocks that have less slack in lending supply. Our evidence that liquidity deteriorates suggests shorting supply constraints affect the cost of transacting faced by all traders. This message is important as a growing body of research argues liquidity influences expected returns (Amihud and Mendelsen, 1986; Acharya and Pedersen, 2005). Authors studying crisis periods argue that policies prohibiting or limiting short selling decrease liquidity. Our results suggest such a decrease would occur in normal economic times as well and bolster the conclusions that the ability to short-sell improves the functioning of financial markets. The second chapter of the dissertation studies whether sell side equity analysts use the trading activity of short sellers when they form their recommendations and EPS estimates. For my identification, I study analysts' actions after the disclosure of short interest, which happens with a lag. This lag divorces the information environment when short sellers initiate their positions from when they are disclosed. This allows for stronger causal inferences that have been elusive in prior literature. I find that analysts exhibit an increased propensity to downgrade their recommendations for a stock after a disclosed increase in short selling. I also find a significantly positive relationship between changes in short interest and the likelihood of a downward EPS revision. These results suggest that analysts extract information from short-sellers' trading activity.

VITA

Corbin Fox was born and raised in Richmond, Virginia. He went to Virginia Commonwealth University in Richmond, Virginia in 2009 to pursue a Bachelor's degree in Finance. He graduated with his undergraduate degree in 2013 and immediately began pursuing his Master's degree in finance, also from Virginia Commonwealth University. He finished his Master's degree in 2014 and then began working on his PhD in Finance at the University of Tennessee in Knoxville, Tennessee. Over the past five year working on his Doctorate he has worked very hard and expects to graduate in Spring of 2019. Corbin's enjoys conducting research in the field of Finance. His research interest include: financial markets, short sellers, equity analysts, market quality, and equity lending.