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PhD in	Economics and Finance
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Abstract

The first chapter of this thesis uses an innovative dataset that includes detailed short sales information and fails-to-deliver (FTDs) at the settlement dates for all stocks listed in the New York Stock Exchange and NASDAQ to provide empirical evidence that the FTDs arising from naked short selling contribute to this mispricing around earnings announcements. Furthermore, this paper provides empirical evidence that, even after new regulation for restricting naked short sales, such misbehavior still causes price distortion during negative corporate events. This work also identifies multiple factors that could influence the (naked) short sales constraints of trading securities. The results show that institutional ownership, insider sales, short interests, and trading volume in a dark pool are important factors in the (naked) short sales of underlying stocks.

The second chapter documents that the positive association between hedge fund activism and long-term firm value documented in prior studies seems driven by selection effects. Using matched samples that incorporate that activist hedge funds tend to target poorly performing firms, we find that firms targeted by activist hedge funds improve less in value than ex-ante similarly poorly performing control firms that are targeted by activist hedge funds. Their relative underperformance is driven by firms that are more engaged in innovation and where stakeholder relationships seem more important for long-term value creation, suggesting that activist hedge fund campaigns may undermine longer-term commitments.

The third chapter is the first research to explore the use by firms of independent valuation experts that are employed to certify goodwill impairment disclosure. Given the increased use of such experts in recent years this research explores how the empirical properties of impairment disclosure differ with or without the use of an expert being disclosed. In the case were a firm publicly discloses the use of an independent expert we find that the properties of the associated goodwill impairments are systematically different than that of other firms. Thus in that sense it is credible to form an opinion that the disclosed use of experts is associated with changes in impairment recognition strategy. We suggest that prior studies predicated on (universal) strategic under-reporting of impairments have potentially omitted the role experts in practice can play in influencing disclosures. Furthermore, we suggest this increased use of independent experts is interesting not least because it appears to be transferring some, or perhaps all, of the responsibility for a key reported item in financial statements away from management and the external auditor.

CHAPTER 1. NAKED SHORT SELLING AROUND EARNINGS ANNOUNCEMENT

1. Introduction

Short selling plays an important role in asset pricing theory and the efficient market hypothesis. Although in that work a critical assumption is that there is no costs associated with short (Fama, 1965, 1970), this assumption is not realistic in practice. Short selling, one of the most important trading strategies, is widely used by investors (e.g., hedge funds, mutual funds) for hedging and/or speculation. Different costs arise when a trading strategy involves short selling. There is now a large body of literature on the relation between short sales and stock price mispricing. Beginning with Diamond and Verrecchia (1987) and Miller (1977) discussing the role of short selling in equity markets, with ongoing developments in methods and analysis, the research related to short selling is likely to see further developments in the forthcoming years. However, little is known about the role of naked short sales in equity market reactions, which is also very important in understanding about price formation and the security settlement process.

The connection between short selling and stock market reactions has received increasing attention in recent years. Because of the large total market of short interest—see Figures 1 and 2 for the total short interest volume and short interest ratio of stocks listed on the New York Stock Exchange (NYSE) and NASDAQ from 2005 to 2015—it is interesting and important to investigate the stock market's reaction to changes in short positions. Given the greater transparency of the U.S. equity market in terms of short selling behavior, we now have access to comprehensive high-frequency data on short sale positions, which is valuable for research. Figure 3 shows daily short sales volumes of the NYSE and NASDAQ. Similar to early research observations (e.g., Boehmer, Jones, & Zhang 2008 and Diether, Lee, & Werner 2009), Figure 4 shows that the short sales volume comprises a large proportion of the daily total trading volume.

[Place Figure 1 here]

[Place Figure 2 here]

[Place Figure 3 here]

[Place Figure 4 here]

Naked short sales can be also thought of as an arbitrage strategy. After negative corporate events are observed, naked short sellers can immediately react by short selling the relevant stocks before they find lendable stocks in the security lending market and could then purchase these stocks to cover their positions later on at a lower market price. However, arbitrage is not a riskless game, since an arbitrage strategy could fail to be profitable due to market incompleteness. In our case, when naked short sellers cannot find lendable stocks in time to deliver to their dealers. This potential profit may fail to be realized and these arbitrageurs would have to take a loss caused by margin call which forces them to cover their broker positions at an unfavorable market price. Abreu and Brunnermeier (2002) use a theoretical model to demonstrate that rational arbitrageurs do not fully correct the mispricing of securities and allow mispricing to persist.

The cost of seeking lendable stocks and the cost associated with the failure to find these stocks contribute to the persistent mispricing and other potential short sellers will realize the opportunity and rush in to further extract lendable stocks, increasing the shortage in the security lending market. In addition, based on one of the results in the authors' model, the duration of mispricing is positively associated with holding costs. So, if we consider that FTDs lead to higher costs for both short sellers and naked short sellers, we expect the abnormal returns of stocks to be positively related to the FTD volume. Figures 5 and 6 display the monthly total FTD volume and corresponding monetary value of stocks listed on the NYSE and NASDAQ from October 2009 to December 2015. Figures 7 and 8 display the ratio of monthly FTD volume to total trading volume and the ratio of monthly FTD volume to short selling volume respectively.

[Place Figure 5 here]

[Place Figure 6 here]

[Place Figure 7 here]

[Place Figure 8 here]

1.1. Literature review

Some of the research mentioned above focuses on the impact of short selling on stock market reactions. For example, Senchack and Starks (1993) provide empirical evidence that, after announcements of unanticipated high short interest, significantly negative abnormal returns of stocks are observed. This effect is weaker for stocks with tradable options. To link the predictability of short selling and future stock performance, Diether et al. (2009a) use the daily short sales data of stocks in 2005 to provide empirical evidence that supports the predictability of future negative abnormal stock returns by short sellers. Another recent work associated with this area is that of Rapach, Ringgenberg, and Zhou (2016), who investigate whether short interest predicts aggregate stock returns. Their findings provide evidence that short interest is a predictor outperforms several other alternatives with the main channel of such predictability being through cash flows. The authors conclude that the results suggest short sellers are informed market participants who have an information advantage in anticipating changes in cash flow and stock returns. Boehmer et al. (2015) identify the source of the information advantage of short sellers. Their results show that announcements of negative corporate events are associated with more short sales. Angel et al. (2003) systematically examine the short sales of stocks listed on the NASDAQ in a sub-period in 2000. Boehmer, Jones, and Zhang (2008) and Boehmer and Wu (2013) suggest that short selling contributes to stock price efficiency.

Related to our research, Christophe et al. (2004) investigate short selling before earnings announcements. They focus on a sample of 913 stocks listed on the NASDAQ during a short time period in 2000. They find a significant link between abnormal short selling in the pre- and post-announcement stock returns. This evidence suggests informed short selling before earnings. Richardson (2003) finds evidence that short sellers do not trade based on information contained in accruals. This implies that, regarding accounting information and earnings quality, short sellers have no information advantage compared to other traders. Drake, Rees, and Swanson (2011) show that short sellers use public information differently from financial analysts and short interest predicts future stock performance, which could benefit traders in designing a profitable trading strategy. Massa, Zhang, and Zhang (2015) also provide empirical evidence that short selling can discipline earnings management. Drake et al. (2015) investigate the trading behavior of short sellers around accounting restatements. Based on their results, short sellers cannot anticipate the restatement but passively react to the event by increasing the short selling volume. A review paper by Reed (2015) introduces several changes to the research related to short sales.

Short sales restrictions under the consideration of naked short sales could provide a different picture from classic financial theory. Previous theoretical (e.g., Miller 1977) and empirical works show that short sales constraints could lead to the overpricing of financial assets and the presence of both such constraints and irrational naked short sellers could mitigate such effects. More detail will be discussed later, with short sales constraints leading to an increasing in settlement failure from naked short selling during negative corporate events, which could dilute shares when traders place their market orders. These "phantom shares" could cause underpricing due to higher short sales constraints. Models related to the effects of short sales constraints on the mispricing of financial assets should also take this unintentional consequence of settlement failure into account.

Berkman et al. (2009) directly test the prediction of Miller (1977), where different options about firm value and short sales constraints contribute to stock overvaluation. They use earnings announcement as events that reduce the difference options among market participants and use institutional ownership as a measure of short sales constraints. Their results are consistent with Miller's (1977), where stocks with the highest differences of opinion and the lowest institutional ownership earn lower earnings announcement period returns. Unlike these authors, we focus on the effect of the earnings surprise shock on imbalance in the security lending market that causes failure at settlement. The new information could update the opinion of stock market participants to modify their trading strategy, but this still does not fully explain the different market reactions when we take into account FTDs. Mashruwala and Mashruwala (2014) show the asymmetric pattern of stock prices in response to different earnings news. Prices decrease strongly to negative earnings announcements when short sales constraints are combined with different investor options.

Shleifer (2000) points out that limits to arbitrage are one of the necessary conditions in explaining the mispricing of financial assets in behavioral finance. The deviation of market prices from fundamental values caused by noisy traders cannot be avoided because of these constraints. Ofek et al. (2004) test the put–call parity no-arbitrage relation under short sales restrictions by using the rebate rates of short selling as a measure to show that limits to arbitrage could explain the asymmetric pattern of the violation of put–call parity.

During the most critical times in 2008 of the recent financial crisis, the U.S. Securities and Exchange Commission (SEC) took emergency action to temporarily ban investors from short selling a number of financial companies to prevent the further fall of these securities. Devaney (2014) tests the impact of the short selling ban on real estate investment trusts (REITs) and show that REIT returns became more volatile, which was the opposite of the SEC's intention. Recent theoretical research by Nezafat et al. (2016) demonstrates that the implementation of the short selling ban during the financial crisis may not have achieved its goal of supporting asset prices.

Diether et al. (2009b) evaluate the Regulation (Reg) SHO Pilot program, an experiment implemented by the SEC to temporary suspend short sales price tests for a set of securities listed on the NYSE and NASDAQ. The short sales price tests were intended to prevent increases in volatility and destabilization when the stock price falls. The authors' results do not suggest that market quality or volatility is affected by the suspension of these price tests. Fang, Huang, and Karpoff (2016) also use the Reg SHO Pilot program as an experiment to show that fewer short sales restrictions by the temporary suspension of price tests help to increase earnings quality and detect accounting fraud. This suggests an improvement in price efficiency caused by short selling. Grullon, Michenaud, and Weston (2015) test the effects of the pilot program on financing and investment decisions and suggest a real effect from the relaxation of short sales constraints.

Several works focus on short sales in countries besides the United States. By using a large sample of quarterly data, Bris, Goetzmann, and Zhu (2007) empirically analyze 46 equity markets around the world to examine the effect of short sales restrictions on individual stocks and stock markets as a whole. According to the results, equity markets in countries with short sales constraints are inefficient at incorporating negative information, which impedes the price discovery process when market moves downward. Saffi and Sigurdsson (2011) investigate the connection between short sales constraints and stock price efficiency by using stock data from 26 countries. Their results also suggest higher short sales constraints contribute to lower price efficiency. Easton and Uylangco (2013) use a case study to investigate short sales in the Australian stock market. Chang, Cheng, and Yu (2007) study short sales constraints in the Hong Kong equity market.

Boulton and Braga-Alves (2010) use FTDs as a proxy of naked short sales and temporary restrictions on the naked short sales of 19 financial firms in 2008 as a natural experiment. They find this policy benefits stocks that are subject to more naked short selling and that naked short sales decrease among restricted stocks. The negative effect of such restrictions influences liquidity, bid–ask spreads, and stock trading volume. Fotak, Raman, and Yadav (2014) directly test the effect of FTDs on market quality. They show a positive relation between FTDs and liquidity and price efficiency and no evidence that FTDs distort stock prices in their test periods. Unlike these authors, we later show that FTDs contribute to price distortion during the earnings announcement period. Our result complements theirs in showing different effects of FTDs surrounding significant corporate events.

Compared to other short sellers who borrow stocks before short selling, naked short sellers bear higher costs due to the margin required by dealers and potential penalties if they cannot deliver the stocks by the settlement date. In a laboratory experiment, Bhojraj et al. (2009) show that the relaxation of margin requirements reduces asset overpricing but delays the convergence to equilibrium price levels. In our setting, more restrictive margin requirements for naked short sellers could accelerate the convergence from overpricing to equilibrium levels. The inclusion of naked short sellers could theoretically benefit the price discovery process, but, if we also consider the costs of settlement failure, such a benefit may not be worth the effort. Evan et al. (2009) introduce an interesting perspective, viewing FTDs as equity loans from security buyers to short sellers, and show that option market makers strategically fail to deliver stocks when equity loans are hard to obtain and expensive. The costs associated with this delivery failure are also passed on to options pricing. This argument suggests that the cost–benefit evaluation by these market makers promotes FTDs under desirable conditions.

Beginning in 2005, a threshold list of securities failing to deliver more than 10,000 shares, or 0.5% of total shares outstanding, in consecutive five settlements days has been published to monitor FTDs. Autore et al. (2015) utilize such a list to test dynamic short sales constraints. Based on their empirical results, the appearance of stocks on the threshold list is accompanied by positive abnormal stocks returns and the removal of stocks from the list is associated with negative stocks returns. The authors suggest these results are consistent with the overvaluation of securities with short sales constraints and that the information revealed by the threshold list captures such binding constraints.

By using proxies of search costs, Kolasinski, Reed, and Ringgenberg (2013) provide empirical evidence of a positive relation between search costs and borrowing costs in the security lending market. Their result is also consistent with our conjecture. An FTD is an extreme case of high searching costs in the security lending market; naked short sellers unable to find stocks in the security lending market directly cause their delivery failure.

1.2. Structure of the paper

We examine short sales and FTDs around earnings announcements. Section 2 describes the sample selection and the major variables in the tests. Section 3 develops our major hypotheses. Section 4 presents the empirical analysis results. Section 5 tests for the effects of several factors on short sales and FTDs during the earnings announcement period. The final section summarizes our conclusions and suggestions for future research.

2. Data and variable definitions

2.1. Data sources and the sample

To assess the association between stock market reactions, short sales, and FTDs around earnings announcements, we combine data from several sources. Our sample selection criteria follow those of Fotak et al. (2014). We start with stocks covered by the Center for Research in Security Prices (CRSP) with exchange codes (item EXCHCD) equal to one and three, which consists of stocks listed on the NYSE and NASDAQ, respectively. The stocks must also have a share code (item SHRCD) equal to 10 or 11. By restricting ourselves to this sample, we focus only on ordinary common shares issued by companies incorporated in the United States, excluding American depository receipts (ADRs), shares of beneficial interest (SBIs), shares of mutual funds and REITs, and so forth. To calculate the earnings surprise score (standardized unexpected earnings, or SUE score), we obtain the consensus analyst earnings estimate and actual earnings from I/B/E/S. The Financial Industry Regulatory Authority (FINRA) started reporting short sales transaction files to the public on their website on September 30, 2009. These files contain detailed information on high-frequency intra-day data for each short sales transaction for stocks listed on the NYSE and NASDAQ and over-the-counter (OTC) equity securities. We use this information to aggregate the daily short sales volume for each stock in our sample. For the FTD data, we collect raw data from the SEC Fails-to-Deliver Archive Data. The National Securities Clearing Corporation (NSCC) has been recording these FTD data in its Continuous Net Settlement (CNS) system, aggregated over all NSCC members, since March 2004. The requirements for the shares recorded in these files have been modified since 2008. Before September 16, 2008, only securities with a balance of FTDs of at least 10,000 shares for a particular settlement date were documented; since September 16, 2008, the database has also been covering securities with a balance of FTDs under 10,000. As discussed later, the sample data start in October 2009, so all FTD data in the sample represent the real balance of FTD shares, without omitting observations with small numbers of FTD shares. Our main sample consists of 1,645 NYSE stocks and 2,360 NASDAQ stocks.

Regarding additional tests, we combine other data sources to create proxies related to the measure of potential factors that could influence short sales and FTDs. The short interest data is from the Compustat Supplemental Short Interest File. These data was originally provided by FT Interactive. The short interest data file contains the reported total of uncovered short positions of listed stocks on the settlement date. Before the change to the current reporting schedule, short interest information was reported once a month (normally in the middle of each month). Since September 2009, it has been published biweekly. Institutional ownership data is collected from the Thomson Reuters 13F Institutional Ownership database. This quarterly data was originally collected from the SEC 13-F form, which institutional investors with over $100 million in qualifying assets are required to file. We collected data on the sales of stocks by insiders from the Thomson Reuters Insiders database. These insiders' shares transaction data are originally reported in SEC forms 3, 4, 5, and 144, which are required as a statement of ownership by a company's officers, directors, and any beneficial owners of more than 10% of the company's equity securities. FINRA currently regularly provides trading information on each alternative trading system (ATS), including all market facilities commonly called dark pools, to increase market transparency. ATSs are required to report the volume of each security in their trading facilities. The current range of securities from ATSs covers Tier 1 and Tier 2 National Market System (NMS) stocks and OTC equity securities subject to FINRA trade reporting requirements. To control for data quality, we use data from January 1, 2015 onward. Tables 1 and 2 report detailed definitions and summary statistics for our major variables, respectively.

[Place Table 1 here]

[Place Table 2 here]

2.2. Short sales measure

For our measure of short sales, we use the outstanding short sales ratio (OSR) for each day, which is defined as the daily short sales volume divided by the total number of shares outstanding (Fotak et al. (2014)). The short sales volume data are collected from the FINRA's Short Sale

Transaction Files. The original short sales data are at the tick level, reporting each individual short sale size and the corresponding transaction price. Because we focus on the total short sales volume each day around earnings announcements, we aggregate all short sales sizes each day to create a daily measure. We then merge this short sales data with the CRSP data by tickers to build a link table. The total number of shares outstanding, obtained from the CRSP, is then used as the denominator of our main OSR measure. This OSR provides a direct measure of the short sales level each day, which is exactly what we want to track to observe daily changes in short sales around earnings announcements. Because we use the total number of shares outstanding rather than the total trading volume to standardize the short sales volume, we can compare the level across different stocks and earnings announcement events without the influence of changes in trading volume.

2.3. FTD measure

Our measure of FTDs, which follows the work of Fotak et al. (2014), is the daily outstanding FTD ratio (OFR) for day t, which is defined as the ratio of reported FTDs at $t + 3$ divided by the total number of shares outstanding on day t. The number of FTD shares is obtained from the SEC's Fails-to-Deliver Archive Data and the daily total number of shares outstanding is collected from the CRSP. For the same reason as described by Fotak et al. (2014), we also use FTDs at $t + 3$ scaled by shares outstanding at t as the OFR on day t, because we focus on the day the transaction actually happens (i.e., day t) rather than the reported date of the FTD, which is the settlement date (day $t + 3$) of the previous transaction. The OFR therefore measures the levels of FTD shares each day around earnings announcements, a measure that is also comparable with the OSR for each day.

3. Hypothesis development

To illustrate our hypothesis development, we incorporate two relevant markets in our analysis: the security lending market and the stock trading market around earnings announcement dates.

Since our focus is the effect of short sales and FTDs on stock prices, we start by analyzing the supply and demand of stocks in the security lending market. The following discussion only considers the case of negative earnings surprise, because this is more linked to short sales and FTDs.

[Place Figure 9 here]

Panel A of Figure 9 plots the supply and demand curves in the security lending market. The horizontal axis is the quantity of shares to borrow and vertical axis is the rebate rate, which is the cost of borrowing stock from a lender. Because we consider only a short period around earnings announcement dates, the supply curve is inelastic, since stock lenders cannot acquire sufficient shares to lend during such a limited time window. We also assume that, during this period, no other stock owners change their role from non-lenders to stock lenders. This assumption is supported by institutional and legal requirements that prevent these non-lenders from changing their type. On the other hand, the demand side of shares to borrow is strongly influenced by any negative earnings surprise during the period under investigation. A negative earnings surprise reflects disappointing performance compared to previous market expectations. Based on this new information, short sellers update their trading strategy and increase the level of short sales. Because short sellers are supposed to be sophisticated investors, their reaction to any material corporate news should be timely. Figure 9 shows the case of a negative earnings surprise, such that, after the earnings announcement, short sellers have a strong incentive to increase the short selling volume to benefit from the stock price drop. Thus, this negative earnings surprise is a shock to the stock lending market’s demand curve, which shifts outward. The figure clearly shows that, shortly after this earnings surprise shock, the demand for shares to borrow deviates from its original equilibrium level to the right, to create excess demand. This excess demand cannot be fulfilled by the inelastic supply side, so this portion of demand will not be supported by the stocks (i.e., the short sellers short sell these shares without really borrowing them) and become naked short sales. One direct consequence of this excess demand is that, on the settlement date, these short sellers cannot find shares in the stock lending market to cover their positions, which leads to FTDs on the settlement date. This excess demand cannot exist for very long, since, as discussed before, if short sales cannot be covered on the settlement date, dealers are forced to acquire such stocks to deliver them to the counterparties and charge short sellers a cost and other penalties according to the new regulation. The excess demand therefore decreases along with an increase in the rebate rate (i.e., the cost of borrowing stock). In the end, the supply and demand return to equilibrium levels but the rebate rate remains higher than before.

We now consider the stock trading market, which is directly relevant to price formation, as shown in Figure 9, Panel B. The horizontal axis is the quantity of shares traded and the vertical axis is stock price. In the stock trading market, a negative earnings surprise is a shock to both the supply and demand sides. Both stock sellers and buyers would update their information set based on this corporate news to modify their trading strategy. For stock buyers, since lower realized earnings reflect poor firm performance, a firm's value and dividend payments would decrease in the future. Therefore, the demand for such stocks sharply decreases. On the other hand, the potential value mitigation of stocks would lower the wealth of current stock owners, who would then also have a strong incentive to sell these stocks to prevent any further loss. We also need to distinguish between the stock owners here and lenders in security lending market. The lenders want to keep their ownership of stocks which they currently hold, even the stock price decrease after negative earnings surprise, these lenders would still wish to hold the stocks rather than directly sell them.

Thus, stock owners who are potential sellers in stock trading market are not the same group as lenders in security lending market. Figure 9 Panel B, shows that the demand curve receives a shock from the earnings surprise and shifts downward. We should therefore be more careful when considering changing the supply curve in the stock trading market. If there is no security lending market, the earnings surprise shock should only shift the supply curve from Supply to Supply*. Based on the discussion in the previous section, shortly after an earnings surprise, the stock lending market experiences excess demand for shares to borrow by short sellers, which is transmitted to the stock trading market through naked short selling. When we combine the two markets together, the shock from the earnings surprise will shift the supply curve in the stock trading market from Supply to Supply* + FTD, the latter curve representing additional supply from naked short sales that will become FTDs on the settlement date. This additional stock supply will push the stock price below P*. This abnormally lower stock price is caused by the dilution from the FTD shares of naked short sellers. In the following empirical tests, we provide evidence to substantiate this point. As discussed above, excess demand in the security lending market will diminish shortly after the FTDs on the settlement date; so, in the stock trading market, the supply curve will return to Supply* and a new equilibrium level (Q*, P*) is achieved.

Another pertinent factor is the size of the earnings surprise shock on the announcement date. When realized reported earnings are much lower than expectations, market participants will react to this news more aggressively and we would then observe a larger shock in both the stock lending and stock trading markets. Another factor that could influence the shock is the short sales constraints for stocks related to the earnings announcement. The reasoning is as follows: When this short sales constraint is strong, short sellers implement their short selling trading strategy with much greater effort. This constraint creates friction in the security lending market and we should therefore observe a smaller shock to the demand for shares to borrow among stocks with strong short sales constraints.

[Place Figure 10 here]

As a quick way to provide initial evidence supporting our previous conclusion, Figure 10 illustrates an event study of cumulative abnormal returns (CARs) around the earnings announcement. Depending on our previous discussion, the effect of FTDs on stock price reactions is much stronger when there is a negative shock to firm performance. We therefore only focus on observations in the lowest SUE score decile, which coincides with the most negative earnings surprise. We then use the level of the OFR on earnings announcement date $T = 0$ to create two portfolios: Large OFR contains stocks in the highest OFR decile and Small OFR contains stocks in the lowest OFR decile. Figure 10 clearly shows different patterns between the two portfolios. For the Small OFR portfolio, shortly after the earnings announcement with negative earnings surprise, the CAR curve reaches a low level and then remains stable. However, the Large OFR portfolio, which contains the highest level of FTDs, shows a pattern very similar to our theoretical analysis in the previous section. The CAR curve first sharply decreases to its minimum at $T + 3$ and then increases with time. This is consistent with the path in Figure 9, Panel B, where the supply curve receives an external shock from both negative corporate news and access demand in the security lending market, shifting from curve Supply to curve Supply* + FTD with stock price overreaction. Then, FTD shares are covered and the curve returns to Supply*. During this process, stock returns should move in the exact pattern shown in Figure 10.

Based on the discussions in this section, our hypotheses are as follows.

H1: Short sales increase when the earnings surprise is more negative.

H2: Short sales increase FTDs during earnings announcement periods.

H3: Higher FTDs levels lead to more negative abnormal returns during earnings announcement periods.

4. Empirical results and analyses

This section focuses on the two main analyses of FTD and short sales around earnings announcements: The first group of tests is to determine potential connections between FTDs, short sales, and stock reactions. As discussed in Section 3, one of the most important questions about FTDs is whether, after the new regulation restricting naked short sales, short sales still contribute a significant portion of FTDs during the earnings announcement period (especially for events with a very negative earnings surprise), in other words, whether FTDs are a useful proxy for naked short sales in this study. After answering this question, our next focus is to determine whether FTDs are a channel for realizing the overreactions in the stock market during the earnings announcement period, as noted in our previous discussion. From a theoretical point of view, we have demonstrated in Section 3 that market overreaction could be explained by the temporary mismatch between naked short selling deals in the stock market and the settlement in the following days. FTDs due to naked short sales on the settlement date could create phantom shares, which would dilute the stock price on the day the deal is made. This section provides empirical evidence to support this conjecture.

The second group of tests attempts to identify the potential mechanism of (naked) short sales constraints. We select several proxies as candidates to form different portfolios of stocks based on the level of these proxies to provide insights about their effects on both short sales and FTDs (naked short sales) around earnings announcements. Based on the level of institutional ownership, short sale open interest, insider sales, and trading volume for each stock in ATSs before the earnings announcement date, a portfolio is created to test differences in short sales and FTDs.

4.1. Short sales around earnings announcements

The reaction of short sellers during the period of new information disclosure is normally based on the nature of the corporate news they observe in the market (Engelberg et al., 2012, and Daske et al., 2005). We therefore first test short sales around earnings announcement dates to see whether this pattern can also be observed.

[Place Figure 11 here]

Figure 11 shows the OSR in each portfolio based on the SUE score. Decile 1 represents the portfolio with the most negative earnings surprise, decile 10 represents the portfolio with the most positive earnings surprise, and deciles 2 to 9, in between, consist of portfolios with increasingly higher SUE scores. Similar to the pattern in Figure 2 of Engelberg et al. 2012, a peak in short sales for different levels of earnings surprise (for both positive and negative corporate news) can be found at around earnings announcement. The inclusion of naked short sellers could theoretically benefit the price discovery process. This pattern shows that short sellers react to the corporate news and update their beliefs to form new trading strategies based on information contained in the earnings announcement; that is, short sellers have no great information advantage before the actual level of earnings is disclosed to the public. The reason is because, if short sellers are well informed before the earnings announcement date, we should observe more short sales before the announcement for an extreme earnings surprise. Another pattern can be identified in Figure 11, where the reaction from short sellers is timely and, during the day of the announcement and the days shortly thereafter, outstanding short sales reach a maximum and decrease to near pre-announcement levels. To determine the connection between the level of earnings surprise and outstanding short sales, we also need to control for other factors, such as previous stock returns and unobserved factors related to the period and firm characteristics. In the following tests, we therefore introduce the ordinary least squares (OLS) regressions for relative days around earnings announcements and control for lagged stock returns and month and firm fixed effects. The estimation equation is

$$\mathrm{OSR}_{i,t} = \alpha + \beta_1(\mathrm{SUE}score_{i,t}) + \beta_2(ret_{i,t-1}) + \beta_3(ret_{i,t-2}) + FE + \varepsilon \quad (1)$$

where $OSR_{i,t}$ is the outstanding short sale ratio, defined as the short sales volume on day *t* divided by total shares outstanding for firm *i* on day *t* and $SUEscore_{i,t}$ is the earnings surprise score for firm *i* at the earnings announcement event. The SUE score is set to be the same for all days around certain earnings announcements; therefore, for the same earnings announcement event of firm *i*, the value of the SUE score is the same for each day within [*T* - 3, *T* + 3] (*T* = 0 is the announcement date) in the regression analysis. The OLS regression results can be found below,

[Place Table 3 here]

[Place Table 4 here]

Table 3 presents the summary statistics of variables in each SUE score decile. Table 4 presents the results of an OLS analysis to show the relation between the OSR and the level of earnings surprise by using SUE scores as a proxy for each day around earnings announcement dates. Panel A includes all observations of both exchanges and Panels B and C are for the NYSE and NASDAQ, respectively. In each regression, we use both firm and month fixed effects to capture the factors caused by firm characteristics and other factors in different periods. As a further control, we also include lagged stock returns in the previous two days. Based on H1, we expect a significantly negative coefficient for the SUE score around earnings announcement dates. The results are consistent with our predictions. Consistent with H1, the regression results, on average, show more outstanding short sales when the SUE score is negative (i.e., the actual reported earnings are less than the consensus analysts' earnings forecasts). When the SUE score is positive (i.e., the actual reported earnings are larger than the consensus analysts' earnings forecasts), the OSR decreases. This effect of the change in outstanding short sales caused by earnings surprise is both economically and statistically significant, after we apply control variables and incorporate firm and month fixed effects to capture any latent factors.

The columns for $T = 0$ (i.e., the regression on the earnings announcement date) and $T + 1$ (i.e., one day after the earnings announcement date) in all three panels provide support for the fact that the effects of earnings surprise on short sales are stronger upon earnings announcements and one day after, based on the coefficients of the SUE score. This confirms that short sellers react to corporate news in a very timely manner and opportunities for profitable short selling exist only for a limited time window. Short selling later than two days after earning announcements may not produce sufficient payoffs because the level of short sales is almost back to that of the pre-announcement period after sharply increasing during the announcement dates. As in Table 4 Panel A, at T - 3, the coefficient of the SUE score is -0.039 and decreases slowly to -0.060 at T - 1. Then, upon the announcement day, the coefficient of the SUE score changes sharply to -0.181, around threefold the level at T - 1. It then continues dropping to -0.356 at $T + 1$, which is almost six times the level at T - 1. After $T + 2$, the coefficients of the SUE score return to that of the pre-announcement period, around -0.5. This pattern is similar for the NYSE and NASDAQ individually.

4.2. FTDs around earnings announcements

This section discusses the pattern of the OFR around earnings announcements. In contrast to the above analysis of short sales during earnings announcement periods, FTDs are associated with the settlement of shares after the transaction. If any one fails to meet their respective obligations, an FTD could lead to the cost of the counterparty in this transaction. First, we investigate whether FTDs change their pattern during the earnings announcement period. The interesting point is that

FTDs are caused by settlements after the transaction, so it should not be directly caused by the firm's fundamental performance, in our case, the earnings surprise. Based on this logic, short sales should more directly impact FTDs because, based on the period discussion, FTDs are also treated as a potential measure of naked short sales. In next section, we use the earnings announcement as a natural experiment to demonstrate the causal effect of short sales on FTDs.

[Place Figure 12 here]

Based on similar portfolio formation rules as in Section 4.1, we create 10 portfolios of stocks based on SUE scores for each earnings announcement event. Figure 12 clearly shows a rather similar OFR pattern around earnings announcement dates. For decile 1 (i.e., earnings announcement events with the lowest SUE scores), the OFR sharply increases upon the announcement date and decrease later but still remains higher than in all the other portfolios. This pattern for FTDs is even stronger than the graphs for short sales in Figure 11. In other words, FTDs react to earnings surprise more aggressively than short sales do and the patterns of the OFR for the NYSE and NASDAQ are similar but not exactly the same. For stocks listed on the NYSE, the pattern is clearer and the reason could reflect the different market's mechanism for these two exchanges, with NYSE being an auction market and NASDAQ a dealer's market. However, the exact source should be investigated in future work.

As discussed before, according to the mechanism of settlement, FTDs should not be directly caused by earnings surprise, which reflects a firm's fundamental performance and the market's expectation of the firm's earnings. Therefore, FTDs should be more strongly correlated with short sales and stock liquidity. In the OLS regression analysis, we therefore use the following equation:

$$\mathrm{OFR}_{i,t} = \alpha + \beta_1(\mathrm{OSR}_{i,t}) + \beta_2(illiquid_{i,t-1}) + \beta_3(illiquid_{i,t-2}) + FE + \varepsilon \qquad (2)$$

where $\mathrm{OFR}_{i,t}$ is the outstanding FTD ratio, defined as the FTD volume on day *t* divided by total shares outstanding for firm *i* on day *t*, and $\mathrm{OSR}_{i,t}$ is the OSR for firm *i* on day *t,* which is defined the same way as before. We also control for lagged illiquidity measures (Amihud, 2002) of stocks in the previous two days. The OLS regression results can be found below.

[Place Table 5 here]

Table 5 presents the OLS regression results for each day around earnings announcements. Panel A is for all observations in both exchanges and Panels B and C are for stocks listed on the NYSE and NASDAQ, respectively. For all days from *T* - 3 until *T* + 3, the coefficients of the OSR are all significantly positive, which reflects the fact that there is a positive correlation between the level of short sales and FTDs. One concern about these OLS coefficients is that only the correlation between short sales and FTDs can be explained here; the causality of short sales cannot be tested by these simple OLS regressions. That is, from this estimation, we cannot conclude that FTDs are a measure of naked short sales. In the following section, we therefore use instrumental variable estimation to provide more robust support for H2.

4.3. Instrumental variable estimation of FTDs and short sales

To mitigate concerns of reverse causality in the OLS regressions, we apply a different econometric tool to estimate the effect of short sales on FTDs around earnings announcement dates. In this test, we use the SUE score (i.e., the level of earnings surprise) as an instrumental variable. The basic idea is as follows: When actual reported earnings are lower than the consensus analysts' earnings forecasts, the market would view this as negative corporate news. Thus, short sellers have greater motivation to short sell stocks of such a firm after the announcement of this level of earnings surprise. However, because FTDs are produced at the settlement, after the transaction, they should not be directly caused by this earnings announcement, which reflects the firm's fundamental aspects. All the effect of the SUE score on FTDs must pass on through its direct influence on short sales, which makes the SUE score an ideal instrumental variable in this setting.

[Place Table 6 here]

In Table 6, we use two-stage least squares estimation by using the SUE score as an instrumental variable in all tests. As in the previous OLS regressions, in each estimation, we include both firm and month fixed effects to capture any hidden factors other than control variables. The results are similar to those found in the previous OLS regressions: Short sales increase the level of FTDs. It is interesting to observe that the pattern of this causal effect is only significant upon the earnings announcement date and the days afterward. In addition, when we use this instrumental variable estimation, almost all the coefficients of lagged illiquidity are insignificant. This result provides empirical evidence to indicate that short sales cause FTDs during earnings announcement periods, because if short sellers have already borrowed the stocks before short selling, this should also prevent any FTDs. Therefore, FTDs could be a reliable measure of naked short sales during the period of our tests, that is, around earnings announcements. Thus, in the following sections, we use FTDs measured by the OFR as a measure in empirical tests regarding naked short sales.

4.4. Naked short sales and abnormal stock returns

One characteristic of naked short sales is that it contributes to the formation of stock prices on the trading dates; however, it also leads to FTDs on the settlement date. Because of this mismatching problem, the market price of stocks is diluted by the shares that failed to be settled. As in the discussion in Section 3 and Figure 9, the overreaction in the stock market shortly after the exogenous shock (i.e., negative earnings surprise) is partially caused by the excess demand for stock borrowing in the security lending market. In this section, we provide empirical evidence to support H3, that, after earnings announcements, FTDs contribute to abnormal stock performance. Unlike the previous discussion in Section 4.2, which uses the OFR as a measure of the level of FTDs, in this section we focus on changes of FTDs to estimate the effects on abnormal stock returns. The reason for using changes of FTDs rather the FTD level is that, from the discussion in Section 3 and Figure 9, the direction of the change plays a very important role in the transition process from the security lending market to the stock market. Increases in FTDs would dilute the shares and lead to more stock price drops, but the decrease in FTDs just means more transactions will be successfully settled on the settlement date, so the effect on the stock market is much weaker. The estimation equation is

$$\mathrm{AbRet}_{i,t} = \alpha + \theta_1 \mathrm{Pos}_{i,t} \times abs(\Delta\mathrm{FTD}_{i,t}) + \theta_2 \mathrm{Pos}_{i,t} + \theta_3 abs\,(\Delta\mathrm{FTD}_{i,t}) + FE + \varepsilon \tag{3}$$

where $\mathrm{AbRet}_{i,t}$ is the abnormal stock return for firm *i* on day *t* estimated by using the following procedure: We first create 10 portfolios based on SUE score (earnings surprise level) deciles. We then use the Fama–French four-factor model to estimate the stock returns on each date relative to the earnings surprise date (e.g., $T + 1$, $T + 2$). The final step is to obtain the residual based on the estimated parameters from the Fama–French model for each firm *i* for different dates *t*. These residuals are used to measure abnormal returns. The term $\Delta\mathrm{FTD}_{i,t}$ is defined as the change of the outstanding FTD ratio for firm *i* on day *t* compared to the FTD ratio on one day before earnings announcement date (i.e., $\Delta\mathrm{FTD}_{i,t} = \mathrm{OFR}_{\mathrm{i,t}} - \mathrm{OFR}_{i,\mathrm{T}-1}$, where *T* is earnings announcement date). It measures the relative change in FTDs for each date around the earnings announcement dates. $abs(\Delta\mathrm{FTD}_{i,t})$ is the absolute value of $\Delta\mathrm{FTD}_{i,t}$. The variable $\mathrm{Pos}_{i,t}$ is a dummy variable that equals one for a positive $\Delta\mathrm{FTD}_{i,t}$ and zero otherwise. The OLS regression results can be found below,

[Place Table 7 here]

The most important estimation result is the sign and level of the coefficient of $\mathrm{Pos}_{\mathrm{i},t} \times abs(\Delta\mathrm{FTD}_{i,t})$, which represents the effect of a positive change of FTDs on abnormal stock returns. Panel A of Table 7 includes the estimation results for all the stocks in both stock exchanges and Panels B and Panel C include the sample of stocks listed on the NYSE and NASDAQ, respectively. Consistent with H3, the sign of θ_1, the coefficient of $\mathrm{Pos}_{\mathrm{i},t} \times abs(\Delta\mathrm{FTD}_{i,t})$, is negative. This result shows the correlation between an increase in FTDs and negative abnormal stock returns. Another interesting finding is that this relation is most significant on the day of the earnings announcement and one day after. This result is also consistent with the discussion in Section 3, that, shortly after the demand for stocks borrowed in the security lending market deviates from the equilibrium level, rational naked short sellers will choose to decrease their short selling behavior to prevent losses from forced settlements by dealers. The results could also indicate that, in all regressions, θ_3, the coefficient of

$abs(\Delta \text{FTD}_{i,t})$, is insignificant. This finding shows that, when the FTD decreases or remains the same (i.e., $\text{Pos}_{i,t} = 0$), it does not have a strong effect on abnormal stock returns.

5. Effects of short sales constraints

One of the major assumptions of neoclassical asset pricing theory is based on cost-free and unlimited amounts of short sales in the security market. In practice, such an assumption is not realistic because of short selling costs, legal requirements, and other institutional settings. A recent example is the temporary short selling ban in the United States during the financial crisis. Short sales constraints are normally viewed as contributing to overpricing (e.g., Jones and Lamont, 2002). Different proxies used to measure the level of short sales constraints are studied in the literature. In this section, we adopt several such measures to test their effects on short sales and FTDs (naked short sales) during the earnings announcement period. These new results could provide additional empirical evidence of whether the measures used in the previous literature as proxies for short sales constraints can be supported.

5.1. Short interest as a proxy for short sales constraints

Short interest is the total quantity of uncovered stock shares sold short by investors. A higher short interest is traditionally considered a greater short sales constraint from the demand side. Several previous works use short interest as a measure of short sales constraint and show its effect on stock prices. Boehmer et al. (2010) shows the asymmetrical pattern of abnormal stock returns with high and low short interest. The positive abnormal returns of stocks with low short interest are larger than the negative abnormal returns of stocks with high short interest. This pattern casts doubt on previous options where short sellers could improve the efficiency of information transition. Asquith et al. (2005) use short interest as a proxy for the demand for shares to borrow and provide empirical evidence that a high level of short interest does not lead to the overpricing of stocks. They conclude that a higher short interest cannot increase short sales constraints. To test whether higher levels of short interest increase short sales constraints for both short sales and naked short sales in our setting, we create two portfolios of stocks based on the level of short interest scaled by the total number of shares outstanding before each earnings announcement event. We then implement different tests similar to those in previous sections for each sub-group.

[Place Table 8 here]

In Table 8, we create groups based on the earnings surprise level measured by the SUE score and classify stocks into a High Short Interest and a Low Short Interest portfolio, based on the median of total short sales before each earnings announcement. The variables compared in Table 8 are the aggregate outstanding FTD ratio and the aggregate OSR. The aggregate FTD ratio is defined as the total number of FTD shares from earnings announcement date T until $T + 3$ scaled by the total number of shares outstanding at T. Similarly, the aggregate short sales ratio is defined as the total number of short sales shares from the earnings announcement date T until $T + 3$ scaled by the total number of shares outstanding at T. The results in Table 8 show that, for both short sales and the FTD ratio, in each SUE decile, portfolios with higher short interest are significantly larger than portfolios with lower short interest. These results counter the prediction of the previous theory, which claims higher short interest increases short sales constraints. Our empirical evidence is consistent with the results of Asquith et al. (2005).

To explain this contradiction, we note that the previous literature treats short interest as a proxy of short sales constraints on the demand side, based on the argument that, if there have been already large numbers of uncovered short selling positions, short sellers will demand less in the stock lending market because they have to hold these positions. However, this argument does not consider that different stocks are not preferred by short sellers in the same way in the security lending market. A high short interest could be preferred by these short sellers or the cost of borrowing these stocks (rebate rate) is cheaper than for stocks with a lower short interest. As shown in our tests, this latter explanation can better characterize the role of short interest when we want to select a measure of short sales constraint.

[Place Table 9 here]

Table 9, Panel A, presents the results of tests that are similar to those in Tables 4 and 5. The only difference is that we use aggregate measures of OSR and OFR as the dependent variables. From the estimation of the coefficients, we note that, consistent with the results in Table 8, stocks in the high short interest group react to earnings surprise more strongly. For a given level of the SUE score, the effects on the aggregate short sales ratio are about three times stronger for the high short interest group compared to the low short interest group. Table 9, Panel B, shows the results of a test that is similar to that in Table 7 to estimate the effect of changes in FTDs on abnormal stock returns. It is interesting to note that this effect lasts longer for stocks in the high short interest group and remains at a significant level at $T + 1$ compared to stocks in the low short interest group, where such an effect can only be observed upon the earnings announcement date T. This additional evidence supports the previous conclusion that the level of short interest could be a measure of how active the stock lending market is rather than a measure of short sales constraint.

[Place Figure 13 here]

[Place Figure 14 here]

To test the effects of short interest on short sales and naked short sales, we can also use graphs to observe the patterns more directly. In Figures 13 and 14, we create five portfolios based on different short interest levels and SUE scores. We first obtain deciles based on the short interest level and deciles based on earnings surprise SUE scores. Portfolio 1 contains all stocks in the lowest decile of short interest and the lowest SUE score decile. Portfolio 2 contains all stocks with the highest decile of short interest and the lowest SUE score decile. Portfolio 3 contains all stocks with the lowest decile of short interest and the highest SUE score decile. Portfolio 4 contains all stocks with the highest decile of short interest and the highest SUE score decile. Portfolio 5 contains all the other stocks. Panels A and B of Figure 13 show the OSR around earnings announcements for each portfolio. We can clearly see a pattern where, besides Portfolio 2 and 4, which are in the highest decile of short interest, none of the other portfolios show a significant increase around earnings announcement dates. This pattern differs from what we observe in Figure 11, where the stocks react to earnings surprise at all levels. In other words, when we do not take into account differences in short interest, then we observe a sharp increase in short sales around earnings announcements, no matter the surprise level. However, when we also consider the level of short interest, only those portfolios with high short interest experience a significant increase in short sales around earnings announcements. This pattern therefore also shows that short interest is not a suitable proxy for short sales constraints, at least around significant corporate events, or earnings announcements in our case. Figure 14 reports the OFR results around earnings announcements. An even clearer pattern can be found for the OFR, where only portfolios with a high short interest sharply increase naked short sales around earnings announcement dates. More importantly, Portfolio 2, which contains those stocks with the most negative earnings surprise, show a much stronger increase in naked short sales during earnings announcement periods. Combined with our previous analysis in Section 3, stock prices will be the most distorted when short interest is high and the earnings surprise is more negative.

5.2. Institutional ownership as a proxy of short sales constraint

Some studies on short selling use the institutional ownership of stocks as short sales constraints on the supply side to investigate the relation between institutional ownership and short sales (e.g., Chen et al. 2002, D'Avolio 2002, and Nagel 2005). The logic behind this recent literature is that the greater the amount of shares owned by institutional investors, such as mutual funds, then the greater the number of lendable shares available in the security lending market to lower short sales constraints. To test the effect of institutional ownership on short sales and naked short sales in our setting, we use data from the Thomson Reuters 13F Institutional Ownership database to group stocks based on the percentage ownership by institutional investors.

[Place Table 10 here]

In Table 10, we create groups based on the earnings surprise level measured by the SUE score and classify the stocks into high and low institutional ownership portfolios based on the median of the percentage ownership by institutional investors before each earnings announcement. As in the previous section, the variables compared in Table 10 are the aggregate outstanding FTD ratio and the aggregate OSR. Table 10 shows, in each SUE decile, OSR of portfolios with high institutional ownership are significantly larger than portfolios with low institutional ownership. However, it is surprising to see that, for the OFR, although the differences between the two groups are small, this pattern is reversed, such that low institutional ownership portfolios are significantly larger than high institutional ownership portfolios. One explanation for these patterns could be that, when the institutional investor percentage of share ownership is high, such as for mutual funds, these institutional investors, because of their investment nature (a strategy investment or keeping certain stocks in their investment portfolios), do not want to change their holding shares, even after observing an earnings surprise. However, they could make a profit by lending these shares to obtain a rebate rate. Thus, during the earnings announcement period, these institutional investors play a role as security lenders to short sellers. This could partially relieve the constraints on the supply side in the security lending market. We could therefore observe high levels of short sales in the portfolio with high institutional ownership. On the other hand, because of this willingness to lend stocks and fewer constraints on the supply side in the security lending market, naked short sellers can more easily find stock shares to cover their short positions, which leads to lower FTDs on the settlement day.

[Place Table 11 here]

The results in Table 11, Panel A, verify the conclusion above. From the estimation of the coefficients, we note that the short sales of stocks with high institutional ownership react more strongly to earnings surprise. For a given SUE score, the effects on the aggregate short sales ratio are almost five times greater in the high institutional ownership group compared to the low institutional ownership group, but aggregate short sales have a weaker effect of FTDs in the former group. From the estimation results in Table 11, Panel B, we observe that the effects of change in FTDs on abnormal returns last longer for stocks with high institutional ownership. At $T + 2$ and $T + 3$, the coefficients of $\mathrm{Pos}_{i,t} \times abs(\Delta \mathrm{FTD}_{i,t})$ become positive and insignificant for low institutional ownership stocks but remain negative and significant at $T + 3$ for high institutional ownership stocks. This could indicate that institutional ownership lowers the constraints of short selling, but it makes the effects of naked short sales on abnormal returns stronger and longer.

5.3. FTDs, short sales, and insider sales

Khan and Lu (2013) provide empirical evidence that leaked information could explain the front-running of short sellers before large insider sales. Significantly positive abnormal short sales are observed days before large insider sales, but this pattern is not observed for small insider sales. From an earnings quality point of view, the authors explain this pattern by arguing that upcoming insider sales could confirm the view of short sellers about firm performance when the financial reporting quality is poor. Since short sellers are considered informed traders, in our setting, we expect their selling behavior to relate to the selling behavior of insiders.

[Place Table 12 here]

[Place Table 13 here]

By using data from the Thomson Reuters Insiders database, we group stocks based on insider sales from 15 days before until one day before each earnings announcement date. Table 12 shows that the short sales of stocks with large insider sales are significantly greater than those with small insider sales. This result is consistent with our previous view about the similar selling behavior of short sellers and insiders. These insider sales enforce the belief of short sellers about firm value and potential future performance, even after the earnings information is revealed to the public. The comparison of FTDs between the two groups shows an ambiguous pattern. This is also not a surprise because, although short sellers as a group could have an information advantage compared to noisy traders and other market participants, naked short sellers cannot be considered informed traders. Because the costs associated with FTD shares are incurred on the settlement day, if short sellers have an information advantage, they would prepare their short position by borrowing these shares before their short sale and deliver them after the stock prices drop. Therefore, the FTDs should not be due to these informed short sellers; in other words, the FTD shares arise from the inappropriate short sales preparation of naked short sellers without an information advantage. Table 13, Panel A, shows rather similar coefficient estimations of the SUE scores and aggregate short sales for stocks with low and high insider sales. Panel B shows that the effect of FTD changes on abnormal returns is only significant for stocks in the group of low insider sales but is insignificant for all days after the earnings announcement.

5.4. FTDs, short sales, and trading volumes in dark pools

Since the second quarter of 2014, FINRA started reporting the trading volume of NMS stocks as well as OTC equity securities in ATSs. These ATSs are not regulated as exchanges, but they match buy and sell orders for their subscribers. To avoid a price impact, increasing numbers of transactions involving a large portion of shares have been moving to ATSs. Because the details of such transactions are not available to the public, so it is with dark pools. According to Zhu (2014) and Samadi (2016), the composition of trades differs between dark pools and exchanges. Relatively more informed traders prefer to execute transactions on an exchange and uninformed traders prefer dark pools.

[Place Table 14 here]

[Place Table 15 here]

The intuition of including trading in dark pools is that, if large trading volumes of certain stocks are executed in a dark pool, more informed short sellers will trade on an exchange. The supply of lendable stocks also shrinks. Because large amounts of trading in a dark pool lower the number of available shares in the market, naked short sellers find it harder to obtain shares to cover their short positions. Therefore, more shares will fail to be delivered on the settlement date. These conjectures are supported by the results in Table 14. To test the effect of trading in a dark pool, we use data covering stocks listed on the NYSE and NASDAQ for the period from January 1, 2015, to August 31, 2015, as well as data on ATS trading volumes that are contained in the FINRA database. The ATS trading volume is based on the latest reporting date before the earnings announcement date. The measure is the total ATS trading volume during the reporting period divided by total shares outstanding. The results for both aggregate FTDs and short sales show that stocks with high ATS trading have significantly more FTDs and short sales for each SUE score level. The results in Table 15 do not provide very significant results. One potential reason is that we use a smaller sample in these regression estimations.

6. Conclusion

After the 2007–2008 financial crisis, regulatory restrictions were placed on naked short sales by introducing new rules and increasing the cost of settlement failure. Although this change has significantly lowered the level of naked short sales in recent years, during periods of significantly negative corporate events, naked short sales sharply increase. This setting provides an ideal environment to test the effect of naked short sales on stock price (over)reaction and the transmission mechanism from the security lending market to stock market. In this paper, we first use instrumental variable analysis to demonstrate that FTDs are a proper measure of naked short sales around earnings announcements. Then we show that increases in naked short sales lead to significantly negative abnormal returns shortly after earnings announcements. We also show that this effect lasts longer on the NYSE than on NASDAQ. We then test the effects of several measures that are associated with short selling constraints and other factors that could be related to transactions on short sales and naked short sales during the earnings announcement period. Based on the empirical evidence, stocks with high short interest, high institutional ownership, high insider sales before earnings announcement events, and a large trading volume in dark pools would be heavily short sold after earning announcement dates. Stocks with high short interest, low institutional ownership, high insider sales before earnings announcements, and a large trading volume in dark pools would be heavily naked short sold.

Our research introduces a potential explanation for stock price overreactions shortly around negative earnings surprises. The FTD shares of stocks caused by naked short sales on the settlement date dilute shares on the trading date when prices are formed by the market trading volume. This mechanism builds a connection between the security lending market and stock markets. In our large sample of stocks listed on the NYSE and NASDAQ, we observe that such effects are significant when earnings surprises are more negative. In this paper, we introduce a fresh view on how to explain abnormal market reactions by fundamental transactions and a settlement mechanism rather than the traditional asset pricing view. This could improve our understanding of the effect of market friction in security price formation.

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Table 1 – Variable Definitions

Table 1 provides a definition of the main variables used in the paper.

Dependent Variable:	
OFR	OFR is outstanding fail-to-deliver ratio which is defined as the ratio of daily fail-to-deliver volume at t+3 to total share outstanding at t to account of the fact that settlement date is normally three days after the trade date.
OSR	OSR is outstanding short sales ratio which is defined as the ratio of daily short sales volume to total share outstanding for each stock.
Control Variables:	
SUE score	SUE score is measure of earnings surprise which is defined as actual earnings per share (EPS) minus mean of consensus analyst earnings forecast, then this difference is standardized by dividing standard deviation of consensus analyst earnings forecast. Positive value of SUE score represents positive earnings surprise, and higher score means higher positive surprise; negative value of SUE score represents negative earnings surprise, and lower score means higher negative surprise. (In order to make the scale at similar level with OFR and OSR, we scale SUE score by dividing 100)
Amihud Illiquidity measure	Amihud Illiquidity measure is proxy of illiquidity based on Amihud (2002). The measure is defined as the ratio of absolute daily stock return to dollar value of daily trading volume scaled by 10^5. Higher value represents higher level of illiquidity.

Table 2 – Summary Statistics

This table reports descriptive statistics for the main variables used in the paper. The sample includes daily data of all sample from the stocks listed in NYSE and NASDAQ for the period October 1st, 2009 – August 31st, 2015. See Table 1, for detailed variable definitions.

All Sample								
	mean	**sd**	**p5**	**p25**	**p50**	**p75**	**p95**	**N**
OFR	0.006%	0.020%	0.000%	0.000%	0.000%	0.003%	0.025%	2,287,066
OSR	0.124%	0.182%	0.006%	0.029%	0.064%	0.139%	0.448%	2,289,454
Fail-to-Deliver Vol / Total Vol	0.751%	2.274%	0.000%	0.000%	0.036%	0.375%	3.845%	2,284,896
Short Sales Vol / Total Vol	11.546%	6.653%	2.815%	6.947%	10.457%	14.868%	24.181%	2,287,078
Stock Return	0.040%	2.511%	-3.934%	-1.148%	0.000%	1.195%	4.075%	2,289,419
Amihud Illiquidity measure	0.002	0.009	0.000	0.000	0.000	0.001	0.009	2,287,049

Stocks listed in Exchange NYSE								
	mean	**sd**	**p5**	**p25**	**p50**	**p75**	**p95**	**N**
OFR	0.005%	0.018%	0.000%	0.000%	0.000%	0.002%	0.018%	1,070,742
OSR	0.125%	0.177%	0.010%	0.034%	0.069%	0.141%	0.439%	1,070,966
Short Sales Vol / Total Vol	10.841%	6.000%	3.052%	6.740%	9.889%	13.818%	21.889%	1,069,610
Fail-to-Deliver Vol / Total Vol	0.485%	1.666%	0.000%	0.000%	0.029%	0.238%	2.198%	1,069,390
Stock Return	0.055%	2.271%	-3.497%	-1.001%	0.042%	1.100%	3.608%	1,070,953
Amihud Illiquidity measure	0.001	0.004	0.000	0.000	0.000	0.000	0.002	1,069,596

Stocks listed in Exchange NASDAQ								
	mean	**sd**	**p5**	**p25**	**p50**	**p75**	**p95**	**N**
OFR	0.007%	0.022%	0.000%	0.000%	0.000%	0.003%	0.031%	1,216,324
OSR	0.122%	0.187%	0.004%	0.025%	0.060%	0.138%	0.456%	1,218,488
Short Sales Vol / Total Vol	12.165%	7.120%	2.584%	7.176%	11.053%	15.850%	25.883%	1,217,468
Fail-to-Deliver Vol / Total Vol	0.985%	2.676%	0.000%	0.000%	0.049%	0.562%	5.464%	1,215,506
Stock Return	0.027%	2.704%	-4.288%	-1.294%	0.000%	1.297%	4.479%	1,218,466
Amihud Illiquidity measure	0.004	0. 011	0.000	0.000	0.000	0.002	0. 018	1,217,453

Table 3 – Summary Statistics at the earnings announcement date by decile of SUE score

This table reports descriptive statistics for the main variables on earnings announcement date by decile of SUE score. The sample includes daily data of all sample from the stocks listed in NYSE and NASDAQ for the period October 1st, 2009 – August 31st, 2015. Securities are ranked by SUE score and allocated to decile 1 (lowest) through 10 (highest). The mean of measures are calculated for each portfolio. The differences between decile 1 and 10 are reported, along with a t-test for differences. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

All Sample											Difference	
Decile of SUE Score	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**D1 - D10**	**p-value**
OFR	0.013%	0.013%	0.010%	0.009%	0.008%	0.008%	0.008%	0.008%	0.007%	0.008%	0.006%	0.000***
OSR	0.264%	0.262%	0.247%	0.237%	0.233%	0.250%	0.258%	0.254%	0.264%	0.258%	0.046%	0.399
Fail-to-Deliver Vol / Total Vol	0.858%	0.862%	0.727%	0.651%	0.644%	0.567%	0.513%	0.528%	0.470%	0.553%	0.305%	0.000***
Short Sales Vol / Total Vol	12.817%	13.146%	13.255%	12.932%	12.883%	12.736%	12.801%	12.718%	12.673%	12.223%	0.594%	0.000***
SUE Score	-5.741	-1.650	-0.631	-0.011	0.485	0.972	1.583	2.425	3.787	8.431	-14.172	0.000***
Stock Return	-1.817%	-1.259%	-0.746%	-0.301%	0.071%	0.423%	0.735%	1.027%	1.284%	1.632%	-3.449%	0.000***
Amihud Illiquidity measure	0.004	0.003	0.002	0.002	0.002	0.002	0.002	0.002	0.002	0.002	0.002	0.000***

NYSE											Difference	
Decile of SUE Score	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**D1 - D10**	**p-value**
OFR	0.013%	0.012%	0.008%	0.007%	0.007%	0.007%	0.007%	0.006%	0.006%	0.007%	0.006%	0.000***
OSR	0.325%	0.302%	0.279%	0.261%	0.256%	0.277%	0.278%	0.271%	0.285%	0.279%	0.046%	0.000***
Fail-to-Deliver Vol / Total Vol	0.677%	0.588%	0.446%	0.413%	0.380%	0.346%	0.361%	0.342%	0.305%	0.373%	0.304%	0.000***
Short Sales Vol / Total Vol	12.445%	12.534%	12.819%	12.451%	12.397%	12.359%	12.482%	12.236%	12.217%	11.742%	0.703%	0.000***
SUE Score	-5.350	-1.646	-0.626	-0.010	0.478	0.964	1.587	2.420	3.791	8.208	-13.558	0.000***
Stock Return	-2.322%	-1.400%	-1.009%	-0.399%	-0.051%	0.384%	0.711%	1.088%	1.538%	2.045%	-4.367%	0.000***
Amihud Illiquidity measure	0.001	0.001	0.001	0.001	0.001	0.001	0.000	0.000	0.000	0.001	0.000	0.000***

NASDAQ											Difference	
Decile of SUE Score	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**D1 - D10**	**p-value**
OFR	0.013%	0.013%	0.011%	0.010%	0.009%	0.010%	0.009%	0.009%	0.008%	0.009%	0.004%	0.000***
OSR	0.224%	0.230%	0.222%	0.214%	0.213%	0.224%	0.238%	0.237%	0.245%	0.241%	-0.017%	0.068*
Fail-to-Deliver Vol / Total Vol	0.978%	1.081%	0.948%	0.872%	0.882%	0.782%	0.671%	0.711%	0.627%	0.703%	0.275%	0.000***
Short Sales Vol / Total Vol	13.062%	13.634%	13.596%	13.378%	13.323%	13.105%	13.131%	13.193%	13.107%	12.621%	0.441%	0.009***
SUE Score	-5.999	-1.653	-0.636	-0.013	0.490	0.980	1.579	2.429	3.782	8.615	-14.615	0.000***
Stock Return	-1.484%	-1.146%	-0.541%	-0.210%	0.182%	0.460%	0.760%	0.968%	1.042%	1.289%	-2.773%	0.000***
Amihud Illiquidity measure	0.006	0.004	0.004	0.003	0.003	0.003	0.003	0.003	0.003	0.003	0.003	0.000***

Table 4 – OLS regression analysis of outstanding short sales around earnings announcement

This table presents the coefficient estimates from OLS regressions that examine outstanding short sales around earnings announcement for stocks listed in NYSE and NASDAQ for the period from October 1st, 2009 to August 31st, 2015. In each regression the dependent variable is daily ratio (percentage) of short sales volume to total share outstanding. Independent variables of interest is the score of earnings surprise (SUE score) which measures the relative level of actual earnings reported and consensus analyst earnings forecast. Lower value of SUE score means the relative level of actual earnings is lower compared to the earnings forecast. In panel A, we examine the short sales for stocks listed in both exchange, in panel B and C we examine stocks listed in NYSE and NASDAQ respectively. In each panel, we examine seven different regressions based on the timing of the dependent variables relative to the earnings announcement date. For example, T-1 indicates the dependent variables is observed one day before the announcement date, T-2 and T-3 are defined similarly. T+1 indicates the dependent variables is observed one day after the announcement date, T+2 and T+3 are defined similarly. T=0 indicates the dependent variables is observed at the same day of announcement date. For the time series of single stock around one earnings announcement event, SUE score is the same for different regressions, but is different for other stocks or the same stock around other earnings announcement events. To control the response of short sales to previous tock returns, we also include two lags of daily returns. The lags are relative to the timing of the dependent variables. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively. In the table, t-statistics appear in brackets and are based on robust standard errors clustered by firm.

	Dependent variable: Outstanding Short Sales Ratio (OSR)						
	T-3	T-2	T-1	T = 0	T+1	T+2	T+3
Panel A: Both Exchange							
SUE Score	-0.0394***	-0.0598***	-0.0597***	-0.1805***	-0.3555***	-0.0550**	-0.0508**
	[-2.674]	[-4.049]	[-3.614]	[-4.866]	[-7.226]	[-2.189]	[-2.435]
Return 1-day lagged	0.0029***	0.0035***	0.0028***	0.0053***	0.0001	-0.0017***	0.0018***
	[8.414]	[10.545]	[7.788]	[8.380]	[0.348]	[-7.639]	[5.303]
Return 2-days lagged	0.0014***	0.0015***	0.0026***	0.0015**	0.0012	-0.0015***	-0.0019***
	[4.628]	[5.025]	[7.107]	[2.332]	[1.595]	[-6.322]	[-11.336]
Firm Fixed-Effect	yes	yes	yes	yes	yes	yes	yes
Month Fixed Effect	yes	yes	yes	yes	yes	yes	yes
Observations	55,997	56,003	56,007	56,016	56,010	56,002	55,996
Adjusted R2	0.025	0.029	0.026	0.021	0.019	0.029	0.027
Panel B: NYSE							
SUE Score	-0.0243	-0.0288	-0.0469*	-0.2758***	-0.4532***	-0.0821**	-0.0868***
	[-1.035]	[-1.272]	[-1.803]	[-4.630]	[-6.176]	[-2.164]	[-2.720]
Return 1-day lagged	0.0018***	0.0026***	0.0019***	0.0053***	-0.0010*	-0.0024***	0.0009*
	[3.461]	[4.929]	[3.613]	[5.010]	[-1.861]	[-6.769]	[1.762]
Return 2-days lagged	0.0012***	0.0010*	0.0022***	0.0015	0.0020*	-0.0021***	-0.0023***
	[2.642]	[1.944]	[4.025]	[1.440]	[1.907]	[-6.603]	[-7.586]
Firm Fixed-Effect	yes	yes	yes	yes	yes	yes	yes
Month Fixed Effect	yes	yes	yes	yes	yes	yes	yes
Observations	26,192	26,197	26,197	26,202	26,199	26,196	26,195
Adjusted R2	0.022	0.024	0.025	0.019	0.020	0.037	0.029
Panel C: NASDAQ							
SUE Score	-0.0498***	-0.0812***	-0.0686***	-0.1117**	-0.2820***	-0.0293	-0.0248
	[-2.626]	[-4.179]	[-3.233]	[-2.361]	[-4.266]	[-0.878]	[-0.901]
Return 1-day lagged	0.0037***	0.0042***	0.0033***	0.0054***	0.0012**	-0.0013***	0.0023***
	[8.011]	[9.634]	[7.105]	[6.802]	[2.005]	[-4.800]	[5.265]
Return 2-days lagged	0.0015***	0.0019***	0.0029***	0.0015*	0.0008	-0.0008**	-0.0017***
	[3.773]	[4.988]	[5.930]	[1.917]	[0.763]	[-2.491]	[-8.563]
Firm Fixed-Effect	yes	yes	yes	yes	yes	yes	yes
Month Fixed Effect	yes	yes	yes	yes	yes	yes	yes
Observations	29,805	29,806	29,810	29,814	29,811	29,806	29,801
Adjusted R2	0.030	0.034	0.028	0.026	0.021	0.026	0.028

Table 5 – OLS regression analysis of outstanding fail-to-deliver around earnings announcement

This table presents the coefficient estimates from OLS regressions that examine outstanding fail-to-deliver around earnings announcement for stocks listed in NYSE and NASDAQ for the period from October 1st, 2009 to August 31st, 2015. In each regression the dependent variable is daily ratio (percentage) of fail-to-deliver volume to total share outstanding. Independent variables of interest is outstanding short sale. In panel A, we examine the fail-to-deliver for stocks listed in both exchange, in panel B and C we examine stocks listed in NYSE and NASDAQ respectively. In each panel, we examine seven different regressions based on the timing of the dependent variables relative to the earnings announcement date. For example, T-1 indicates the dependent variables is observed one day before the announcement date, T-2 and T-3 are defined similarly. T+1 indicates the dependent variables is observed one day after the announcement date, T+2 and T+3 are defined similarly. T=0 indicates the dependent variables is observed at the same day of announcement date. To control the response of short sales to previous tock returns, we also include two lags of daily Amihud illiquidity measure. The lags are relative to the timing of the dependent variables. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively. In the table, t-statistics appear in brackets and are based on robust standard errors clustered by firm.

	Dependent variable: Outstanding Fail to Deliver Ratio (OFR)						
	T-3	T-2	T-1	T = 0	T+1	T+2	T+3
Panel A: Both Exchange							
OSR	0.0252***	0.0286***	0.0289***	0.0253***	0.0266***	0.0334***	0.0346***
	[18.002]	[18.917]	[18.412]	[19.834]	[20.564]	[19.540]	[18.936]
Illiquidity 1-day lagged	-0.0210*	-0.0257**	-0.0488***	-0.0639***	-0.1016***	-0.0554**	-0.0586***
	[-1.898]	[-2.009]	[-3.559]	[-3.001]	[-3.205]	[-2.055]	[-2.979]
Illiquidity 2-days lagged	-0.0286***	-0.0392***	-0.0205	-0.0325	-0.0742***	-0.0666***	-0.0551**
	[-2.765]	[-3.134]	[-1.574]	[-1.625]	[-2.772]	[-2.908]	[-2.415]
Firm Fixed-Effect	yes	yes	yes	yes	yes	yes	yes
Month Fixed Effect	yes	yes	yes	yes	yes	yes	yes
Observations	55,831	55,841	55,843	55,902	55,901	55,892	55,872
Adjusted R2	0.045	0.049	0.047	0.052	0.057	0.048	0.048
Panel B: NYSE							
OSR	0.0214***	0.0219***	0.0240***	0.0226***	0.0263***	0.0302***	0.0305***
	[10.736]	[10.135]	[9.982]	[13.061]	[11.180]	[11.116]	[10.655]
Illiquidity 1-day lagged	-0.0140	0.0208	-0.0414*	-0.0210	-0.0198	0.0214	-0.0562
	[-0.539]	[1.062]	[-1.694]	[-0.634]	[-0.234]	[0.409]	[-1.042]
Illiquidity 2-days lagged	0.0306	-0.0102	0.0168	-0.0024	-0.1429**	-0.0113	0.0611
	[0.941]	[-0.403]	[0.625]	[-0.069]	[-2.179]	[-0.246]	[0.981]
Firm Fixed-Effect	yes	yes	yes	yes	yes	yes	yes
Month Fixed Effect	yes	yes	yes	yes	yes	yes	yes
Observations	26,134	26,139	26,137	26,155	26,158	26,157	26,152
Adjusted R2	0.037	0.034	0.035	0.049	0.052	0.040	0.039
Panel C: NASDAQ							
OSR	0.0281***	0.0337***	0.0326***	0.0277***	0.0269***	0.0356***	0.0375***
	[14.498]	[16.366]	[16.084]	[15.051]	[17.486]	[16.148]	[15.923]
Illiquidity 1-day lagged	-0.0220*	-0.0282**	-0.0452***	-0.0604***	-0.1086***	-0.0617**	-0.0569***
	[-1.848]	[-2.077]	[-3.038]	[-2.634]	[-3.223]	[-2.126]	[-2.726]
Illiquidity 2-days lagged	-0.0315***	-0.0397***	-0.0228*	-0.0321	-0.0586**	-0.0707***	-0.0651***
	[-2.936]	[-2.953]	[-1.673]	[-1.483]	[-2.043]	[-2.874]	[-2.691]
Firm Fixed-Effect	yes	yes	yes	yes	yes	yes	yes
Month Fixed Effect	yes	yes	yes	yes	yes	yes	yes
Observations	29,697	29,702	29,706	29,747	29,743	29,735	29,720
Adjusted R2	0.052	0.063	0.056	0.055	0.060	0.053	0.055

Table 6 – Instrumental variable regression of outstanding fail-to-deliver around earnings announcement

This table presents the coefficient estimates from instrumental variable regressions that examine outstanding fail-to-deliver around earnings announcement for stocks listed in NYSE and NASDAQ for the period from October 1st, 2009 to August 31st, 2015. The instrumental variable in each regression is the SUE score which has indirect effect on outstanding fail-to-deliver through daily outstanding short sale. In each regression the dependent variable is daily ratio (percentage) of fail-to-deliver volume to total share outstanding. Independent variables of interest is outstanding short sale. In panel A, we examine the fail-to-deliver for stocks listed in both exchange, in panel B and C we examine stocks listed in NYSE and NASDAQ respectively. In each panel, we examine seven different regressions based on the timing of the dependent variables relative to the earnings announcement date. For example, T-1 indicates the dependent variables is observed one day before the announcement date, T-2 and T-3 are defined similarly. T+1 indicates the dependent variables is observed one day after the announcement date, T+2 and T+3 are defined similarly. T=0 indicates the dependent variables is observed at the same day of announcement date. To control the response of short sales to previous tock returns, we also include two lags of daily Amihud illiquidity measure. The lags are relative to the timing of the dependent variables. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively. In the table, t-statistics appear in brackets and are based on robust standard errors clustered by firm.

	Dependent variable: Outstanding Fail to Deliver Ratio (OFR)						
	T-3	T-2	T-1	T = 0	T+1	T+2	T+3
Panel A: Both Exchange							
OSR	0.0395	0.0295	0.0612	0.0958***	0.0705***	0.0495**	0.0651**
	[0.753]	[0.800]	[1.455]	[3.557]	[4.687]	[2.112]	[2.574]
Illiquidity t-1	-0.0118	-0.0250	-0.0220	0.0501	0.0127	-0.0288	-0.0205
	[-0.331]	[-0.731]	[-0.588]	[1.009]	[0.243]	[-0.622]	[-0.542]
Illiquidity t-2	-0.0147	-0.0385	0.0085	0.0611	-0.0046	-0.0455	-0.0161
	[-0.288]	[-1.150]	[0.209]	[1.437]	[-0.123]	[-1.170]	[-0.396]
Firm Fixed-Effect	yes	yes	yes	yes	yes	yes	yes
Month Fixed Effect	yes	yes	yes	yes	yes	yes	yes
Observations	55,662	55,673	55,672	55,733	55,730	55,717	55,704
Panel B: NYSE							
OSR	0.1050	0.0765	-0.0152	0.0834***	0.0550***	0.0254	0.0440
	[0.690]	[0.634]	[-0.176]	[3.376]	[3.633]	[1.033]	[1.470]
Illiquidity t-1	0.0719	0.0888	-0.0530	0.0352	-0.0036	0.0141	-0.0536
	[0.446]	[0.576]	[-1.447]	[0.497]	[-0.031]	[0.231]	[-0.913]
Illiquidity t-2	0.0822	0.0145	-0.0452	0.0632	-0.1541*	-0.0110	0.0753
	[0.765]	[0.219]	[-0.318]	[0.659]	[-1.680]	[-0.247]	[1.013]
Firm Fixed-Effect	yes	yes	yes	yes	yes	yes	yes
Month Fixed Effect	yes	yes	yes	yes	yes	yes	yes
Observations	26,089	26,094	26,091	26,108	26,112	26,110	26,106
Panel C: NASDAQ							
OSR	0.0164	0.0153	0.0843*	0.1156*	0.0906***	0.0888*	0.0930**
	[0.282]	[0.411]	[1.689]	[1.742]	[2.982]	[1.849]	[2.090]
Illiquidity t-1	-0.0288	-0.0430	0.0010	0.0857	0.0637	0.0256	0.0154
	[-0.793]	[-1.291]	[0.021]	[0.756]	[0.705]	[0.308]	[0.248]
Illiquidity t-2	-0.0429	-0.0551	0.0189	0.0841	0.0533	0.0044	0.0070
	[-0.749]	[-1.579]	[0.439]	[0.922]	[0.858]	[0.060]	[0.109]
Firm Fixed-Effect	yes	yes	yes	yes	yes	yes	yes
Month Fixed Effect	yes	yes	yes	yes	yes	yes	yes
Observations	29,561	29,567	29,569	29,613	29,606	29,595	29,586

Table 7 – Fail-to-deliver and Abnormal Stock Returns

This table presents the coefficient estimates from OLS regressions that examine the impact of outstanding fail-to-deliver on abnormal daily returns around earnings announcement for stocks listed in NYSE and NASDAQ for the period from October 1st, 2009 to December 31st, 2015. In each regression the dependent variable is daily abnormal returns. Independent variables of interest are the absolute value of the change of fail-to-deliver and the direction of such change. abs(ΔFTD) equals to the absolute value of the difference between current day outstanding fail-to-deliver in percentage and the T-1 outstanding fail-to-deliver. Pos is a dummy that equals to one for the positive ΔFTD and zero elsewise. Pos x abs(ΔFTD) is the interaction of two variables. In panel A, we examine the abnormal returns of stocks listed in both exchange, in panel B and C we examine stocks listed in NYSE and NASDAQ respectively. In each panel, we examine four different regressions based on the timing of the dependent variables relative to the earnings announcement date. For example, T+1 indicates the dependent variables is observed one day after the announcement date, T+2 and T+3 are defined similarly. T=0 indicates the dependent variables is observed at the same day of announcement date. The lags are relative to the timing of the dependent variables. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively. In the table, t-statistics appear in brackets and are based on robust standard errors clustered by firm.

	Dependent variable: Abnormal Return			
	T = 0	T+1	T+2	T+3
Panel A: Both Exchange				
Pos x abs(ΔFTD)	-5.6259**	-10.5488***	-0.3928	-1.7044
	[-2.562]	[-4.331]	[-0.346]	[-1.634]
Pos	0.2398***	0.4464***	0.1784***	0.1941***
	[6.110]	[8.759]	[7.967]	[9.645]
abs(ΔFTD)	1.2256	-0.5877	0.1846	0.0666
	[0.666]	[-0.280]	[0.211]	[0.086]
Firm Fixed-Effect	yes	yes	yes	yes
Month Fixed Effect	yes	yes	yes	yes
Observations	55,937	55,918	55,905	55,881
Adjusted R2	0.003	0.006	0.003	0.003
Panel B: NYSE				
Pos x abs(ΔFTD)	-6.8348**	-9.6381***	-0.6761	-2.9471*
	[-2.002]	[-3.178]	[-0.432]	[-1.951]
Pos	0.2371***	0.2669***	0.1544***	0.1830***
	[4.243]	[4.505]	[5.517]	[7.391]
abs(ΔFTD)	1.7035	2.4720	-0.1518	1.2749
	[0.608]	[0.998]	[-0.132]	[1.212]
Firm Fixed-Effect	yes	yes	yes	yes
Month Fixed Effect	yes	yes	yes	yes
Observations	26,190	26,188	26,183	26,179
Adjusted R2	0.005	0.004	0.003	0.004
Panel C: NASDAQ				
Pos x abs(ΔFTD)	-4.8008*	-10.9645***	-0.4230	-0.9845
	[-1.670]	[-3.075]	[-0.265]	[-0.686]
Pos	0.2444***	0.6109***	0.2040***	0.2069***
	[4.421]	[7.510]	[5.901]	[6.573]
abs(ΔFTD)	0.8602	-2.8595	0.5393	-0.6508
	[0.353]	[-0.903]	[0.424]	[-0.588]
Firm Fixed-Effect	yes	yes	yes	yes
Month Fixed Effect	yes	yes	yes	yes
Observations	29,747	29,730	29,722	29,702
Adjusted R2	0.003	0.008	0.004	0.003

Table 8 – Fail-to-deliver, Short Sales and Short Interest

This table presents the differences of outstanding fail-to-deliver and short sales between stocks with high and low short interest for stocks listed in NYSE and NASDAQ for the period from October 1st, 2009 to August 31st, 2015. Aggregate Fail-to-Deliver is the aggregate outstanding fail-to-deliver in [T, T+3] divided by total share outstanding, in which T is the date of earnings announcement. Aggregate Short Sales is the aggregate outstanding short sales in [T, T+3] divided by total share outstanding. We create the portfolio based on 10 deciles of SUE score. Decile 1 of SUE score represents the most negative earnings surprise. Decile 10 of SUE score represents the most positive earnings surprise. Short Interest is based on the latest reporting date before the earnings announcement date. Low Short Interest is the ones which have short interest lower than the median level of whole sample, High Short Interest is the ones which have short interest higher than the median level of whole sample. For each decile portfolio, the Diff is the difference of the measure between Small Short Interest group and Large Short Interest group. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

	Aggregate Fail-to-Deliver [0, +3]			
Decile of SUE score	**Low Short Interest**	**High Short Interest**	**Diff**	**p-value**
1	0.027%	0.078%	-0.052%	0.000***
2	0.027%	0.064%	-0.037%	0.000***
3	0.021%	0.056%	-0.036%	0.000***
4	0.019%	0.049%	-0.030%	0.000***
5	0.020%	0.048%	-0.028%	0.000***
6	0.018%	0.045%	-0.027%	0.000***
7	0.015%	0.044%	-0.028%	0.000***
8	0.015%	0.045%	-0.030%	0.000***
9	0.016%	0.046%	-0.030%	0.000***
10	0.020%	0.048%	-0.028%	0.000***

	Aggregate Short Sales [0, +3]			
Decile of SUE score	**Low Short Interest**	**High Short Interest**	**Diff**	**p-value**
1	0.547%	1.351%	-0.804%	0.000***
2	0.548%	1.249%	-0.701%	0.000***
3	0.549%	1.172%	-0.623%	0.000***
4	0.516%	1.120%	-0.604%	0.000***
5	0.493%	1.121%	-0.628%	0.000***
6	0.526%	1.160%	-0.633%	0.000***
7	0.535%	1.194%	-0.660%	0.000***
8	0.526%	1.227%	-0.701%	0.000***
9	0.560%	1.273%	-0.713%	0.000***
10	0.563%	1.296%	-0.733%	0.000***

Table 9 – Regression analysis of Fail-to-deliver, Short Sales and Short Interest

This table presents the differences of outstanding fail-to-deliver and short sales between stocks with high and low short interest for stocks listed in NYSE and NASDAQ for the period from October 1st, 2009 to August 31st, 2015. The regressions are the same of the ones in Table 4, Table 5 and Table 7. Aggregate Fail-to-Deliver is the aggregate outstanding fail-to-deliver in [T, T+3] divided by total share outstanding, in which T is the date of earnings announcement. Aggregate Short Sales is the aggregate outstanding short sales in [T, T+3] divided by total share outstanding. We create the portfolio based on 10 deciles of SUE score. Decile 1 of SUE score represents the most negative earnings surprise. Decile 10 of SUE score represents the most positive earnings surprise. Short Interest is based on the latest reporting date before the earnings announcement date. Low Short Interest is the ones which have short interest lower than the median level of whole sample, High Short Interest is the ones which have short interest higher than the median level of whole sample. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively. In the table, t-statistics appear in brackets and are based on robust standard errors clustered by firm.

Panel A:

Both Exchange	**Low Short Interest**		**High Short Interest**	
	Aggregate Short Sales	**Aggregate Fail-to-Deliver**	**Aggregate Short Sales**	**Aggregate Fail-to-Deliver**
SUE Score	-0.4279***		-1.2244***	
	[-4.052]		[-6.709]	
Return 1-day lagged	0.0093***		0.0079***	
	[4.367]		[3.119]	
Return 2-days lagged	0.0025		0.0041	
	[1.155]		[1.452]	
Aggregate Short Sales		0.0311***		0.0362***
		[11.143]		[20.086]
Illiquidity 1-day lagged		-0.1992***		0.2301
		[-3.160]		[0.646]
Illiquidity 2-days lagged		-0.0584		-0.5380
		[-0.993]		[-1.488]
Firm Fixed-Effect	yes	yes	yes	yes
Month Fixed Effect	yes	yes	yes	yes
Observations	28,007	27,922	28,009	28,004
Adjusted R2	0.033	0.077	0.043	0.088

Panel B:

Both Exchange	Dependent variable: Abnormal Return							
	Low Short Interest				High Short Interest			
	T = 0	T+1	T+2	T+3	T = 0	T+1	T+2	T+3
Pos x abs(ΔFTD)	-11.9714***	-3.9759	0.2611	-1.2472	-4.7468*	-11.8626***	-0.2784	-1.0634
	[-2.874]	[-0.901]	[0.104]	[-0.576]	[-1.787]	[-3.956]	[-0.208]	[-0.866]
Pos	0.2436***	0.3577***	0.1910***	0.2449***	0.2589***	0.5313***	0.1786***	0.1361***
	[4.778]	[5.495]	[6.262]	[8.772]	[4.162]	[6.553]	[5.136]	[4.569]
abs(ΔFTD)	10.3822***	-1.1241	-0.6499	-0.3678	-0.5085	-1.0721	0.6181	0.0242
	[3.171]	[-0.299]	[-0.324]	[-0.219]	[-0.226]	[-0.415]	[0.608]	[0.027]
Firm Fixed-Effect	yes	yes	yes	yes	yes	yes	yes	yes
Month Fixed Effect	yes	yes	yes	yes	yes	yes	yes	yes
Observations	27,946	27,931	27,924	27,903	27,991	27,987	27,981	27,978
Adjusted R2	0.003	0.003	0.003	0.006	0.004	0.010	0.005	0.002

Table 10 – Fail-to-deliver, Short Sales and Institutional Ownership

This table presents the differences of outstanding fail-to-deliver and short sales between stocks with high and low institutional ownership for stocks listed in NYSE and NASDAQ for the period from October 1st, 2009 to August 31st, 2015. Aggregate Fail-to-Deliver is the aggregate outstanding fail-to-deliver in [T, T+3] divided by total share outstanding, in which T is the date of earnings announcement. Aggregate Short Sales is the aggregate outstanding short sales in [T, T+3] divided by total share outstanding. We create the portfolio based on 10 deciles of SUE score. Decile 1 of SUE score represents the most negative earnings surprise. Decile 10 of SUE score represents the most positive earnings surprise. Institutional ownership is based on the latest 13F reporting date before the earnings announcement date. Low Institutional Ownership is the ones which have institutional ownership lower than the median level of whole sample, High Institutional Ownership is the ones which have institutional ownership higher than the median level of whole sample. For each decile portfolio, the Diff is the difference of the measure between Small Institutional Ownership group and Large Institutional Ownership group. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

	Aggregate Fail-to-Deliver [0, +3]			
Decile of SUE score	**Low Institutional Ownership**	**High Institutional Ownership**	**Diff**	**p-value**
1	0.052%	0.051%	0.000%	0.826
2	0.050%	0.040%	0.011%	0.000***
3	0.043%	0.034%	0.009%	0.000***
4	0.039%	0.028%	0.011%	0.000***
5	0.039%	0.028%	0.010%	0.000***
6	0.033%	0.029%	0.004%	0.001***
7	0.035%	0.026%	0.009%	0.000***
8	0.034%	0.026%	0.008%	0.000***
9	0.034%	0.029%	0.005%	0.000***
10	0.039%	0.029%	0.010%	0.000***

	Aggregate Short Sales [0, +3]			
Decile of SUE score	**Low Institutional Ownership**	**High Institutional Ownership**	**Diff**	**p-value**
1	0.742%	1.212%	-0.470%	0.000***
2	0.716%	1.106%	-0.390%	0.000***
3	0.696%	1.058%	-0.363%	0.000***
4	0.696%	0.936%	-0.240%	0.000***
5	0.651%	0.945%	-0.293%	0.000***
6	0.701%	0.968%	-0.267%	0.000***
7	0.741%	0.977%	-0.236%	0.000***
8	0.722%	0.978%	-0.256%	0.000***
9	0.763%	1.027%	-0.264%	0.000***
10	0.744%	1.056%	-0.311%	0.000***

Table 11 – Regression analysis of Fail-to-deliver, Short Sales and Institutional Ownership

This table presents the differences of outstanding fail-to-deliver and short sales between stocks with high and low institutional ownership for stocks listed in NYSE and NASDAQ for the period from October 1st, 2009 to August 31st, 2015. The regressions are the same of the ones in Table 4, Table 5 and Table 7. Aggregate Fail-to-Deliver is the aggregate outstanding fail-to-deliver in [T, T+3] divided by total share outstanding, in which T is the date of earnings announcement. Aggregate Short Sales is the aggregate outstanding short sales in [T, T+3] divided by total share outstanding. We create the portfolio based on 10 deciles of SUE score. Decile 1 of SUE score represents the most negative earnings surprise. Decile 10 of SUE score represents the most positive earnings surprise. Institutional ownership is based on the latest 13F reporting date before the earnings announcement date. Low Institutional Ownership is the ones which have institutional ownership lower than the median level of whole sample, High Institutional Ownership is the ones which have institutional ownership higher than the median level of whole sample. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively. In the table, t-statistics appear in brackets and are based on robust standard errors clustered by firm.

Panel A:

Both Exchange	Low Institutional Ownership		High Institutional Ownership	
	Aggregate Short Sales	Aggregate Fail-to-Deliver	Aggregate Short Sales	Aggregate Fail-to-Deliver
SUE Score	-0.2855**		-1.3619***	
	[-2.094]		[-8.422]	
Return 1-day lagged	0.0090***		0.0082***	
	[3.913]		[3.165]	
Return 2-days lagged	0.0023		0.0038	
	[0.904]		[1.492]	
Aggregate Short Sales		0.0405***		0.0289***
		[17.118]		[15.809]
Illiquidity 1-day lagged		-0.2240***		0.1334
		[-3.198]		[0.907]
Illiquidity 2-days lagged		-0.1496**		0.4661**
		[-2.282]		[2.424]
Firm Fixed-Effect	yes	yes	yes	yes
Month Fixed Effect	yes	yes	yes	yes
Observations	26,628	26,544	26,629	26,629
Adjusted R2	0.035	0.093	0.040	0.076

Panel B:

Both Exchange	Dependent variable: Abnormal Return							
	Low Institutional Ownership				High Institutional Ownership			
	T = 0	T+1	T+2	T+3	T = 0	T+1	T+2	T+3
Pos x abs(ΔFTD)	-8.4831**	-7.8827**	1.1364	0.5255	-3.4440	-13.5247***	-1.0458	-3.2826**
	[-2.482]	[-2.123]	[0.649]	[0.328]	[-1.068]	[-3.998]	[-0.713]	[-2.420]
Pos	0.2703***	0.4836***	0.1856***	0.1915***	0.1933***	0.4257***	0.1818***	0.1808***
	[4.502]	[6.210]	[5.067]	[5.775]	[3.393]	[5.874]	[6.303]	[7.012]
abs(ΔFTD)	4.5472	-4.7621	-1.1987	-1.2217	-2.8399	2.9165	1.7152	1.3321
	[1.585]	[-1.420]	[-0.849]	[-0.957]	[-1.029]	[1.014]	[1.530]	[1.355]
Firm Fixed-Effect	yes	yes	yes	yes	yes	yes	yes	yes
Month Fixed Effect	yes	yes	yes	yes	yes	yes	yes	yes
Observations	26,563	26,546	26,533	26,513	26,617	26,615	26,615	26,612
Adjusted R2	0.004	0.008	0.003	0.006	0.004	0.006	0.004	0.003

Table 12 – Fail-to-deliver, Short Sales and Insider Sales

This table presents the differences of outstanding fail-to-deliver and short sales between stocks with high and low insider sales for stocks listed in NYSE and NASDAQ for the period from October 1st, 2009 to December 31st, 2014. Aggregate Fail-to-Deliver is the aggregate outstanding fail-to-deliver in [T, T+3] divided by total share outstanding, in which T is the date of earnings announcement. Aggregate Short Sales is the aggregate outstanding short sales in [T, T+3] divided by total share outstanding. We create the portfolio based on 10 deciles of SUE score. Decile 1 of SUE score represents the most negative earnings surprise. Decile 10 of SUE score represents the most positive earnings surprise. The insider sales is from Thomson Reuters Insiders database. The value of insider sales is the aggregate insiders stock sales in [T-15, T-1] standardized by dividing the total share outstanding, in which T=0 is the date of earnings announcement. Low Insider Sales is the ones which have insider sales lower than the median level of whole sample, High Insider Sales is the ones which have insider sales higher than the median level of whole sample. For each decile portfolio, the Diff is the difference of the measure between Small Insider Sales group and Large Insider Sales group. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

	Aggregate Fail-to-Deliver [0, +3]			
Decile of SUE score	**Low Insider Sales**	**High Insider Sales**	**Diff**	**p-value**
1	0.051%	0.054%	-0.003%	0.203
2	0.045%	0.047%	-0.002%	0.330
3	0.040%	0.036%	0.004%	0.033**
4	0.034%	0.035%	-0.001%	0.385
5	0.032%	0.038%	-0.007%	0.000***
6	0.032%	0.030%	0.002%	0.096*
7	0.031%	0.028%	0.002%	0.119
8	0.029%	0.031%	-0.002%	0.172
9	0.029%	0.037%	-0.008%	0.000***
10	0.032%	0.038%	-0.005%	0.000***

	Aggregate Short Sales [0, +3]			
Decile of SUE score	**Low Insider Sales**	**High Insider Sales**	**Diff**	**p-value**
1	0.876%	1.146%	-0.270%	0.000***
2	0.853%	1.029%	-0.175%	0.000***
3	0.835%	0.952%	-0.117%	0.000***
4	0.761%	0.982%	-0.221%	0.000***
5	0.751%	0.931%	-0.181%	0.000***
6	0.789%	0.989%	-0.201%	0.000***
7	0.833%	0.980%	-0.147%	0.000***
8	0.789%	1.072%	-0.282%	0.000***
9	0.848%	1.086%	-0.239%	0.000***
10	0.842%	1.128%	-0.286%	0.000***

Table 13 – Regression analysis of Fail-to-deliver, Short Sales and Insider Sales

This table presents the differences of outstanding fail-to-deliver and short sales between stocks with high and low insider sales for stocks listed in NYSE and NASDAQ for the period from October 1st, 2009 to December 31st, 2014. The regressions are the same of the ones in Table 4, Table 5 and Table 7. Aggregate Fail-to-Deliver is the aggregate outstanding fail-to-deliver in [T, T+3] divided by total share outstanding, in which T is the date of earnings announcement. Aggregate Short Sales is the aggregate outstanding short sales in [T, T+3] divided by total share outstanding. We create the portfolio based on 10 deciles of SUE score. Decile 1 of SUE score represents the most negative earnings surprise. Decile 10 of SUE score represents the most positive earnings surprise. The insider sales is from Thomson Reuters Insiders database. The value of insider sales is the aggregate insiders stock sales in [T-15, T-1] standardized by dividing the total share outstanding, in which T=0 is the date of earnings announcement. The sample just includes the observations which have non-zero insider sales during the period of test. Low Insider Sales is the ones which have insider sales lower than the median level of whole sample, High Insider Sales is the ones which have insider sales higher than the median level of whole sample. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively. In the table, t-statistics appear in brackets and are based on robust standard errors clustered by firm.

Panel A:

Both Exchange	Low Insider Sale		High Insider Sale	
	Aggregate Short Sales	**Aggregate Fail-to-Deliver**	**Aggregate Short Sales**	**Aggregate Fail-to-Deliver**
SUE Score	-0.8458***		-0.7823***	
	[-7.282]		[-3.063]	
Return 1-day lagged	0.0087***		0.0046	
	[4.515]		[1.145]	
Return 2-days lagged	0.0043**		-0.0026	
	[2.158]		[-0.580]	
Aggregate Short Sales		0.0370***		0.0311***
		[20.412]		[13.120]
Illiquidity 1-day lagged		-0.2051***		-0.2757
		[-2.901]		[-1.216]
Illiquidity 2-days lagged		-0.1713***		0.3143
		[-2.632]		[1.366]
Firm Fixed-Effect	yes	yes	yes	yes
Month Fixed Effect	yes	yes	yes	yes
Observations	40,404	40,323	12,845	12,842
Adjusted R2	0.034	0.094	0.031	0.071

Panel B:

Both Exchange	**Dependent variable: Abnormal Return**							
	Low Insider Sales				**High Insider Sales**			
	T = 0	T+1	T+2	T+3	T = 0	T+1	T+2	T+3
Pos x abs(ΔFTD)	-5.1284*	-10.0582***	-0.9935	-1.0198	-7.0534	-9.2385	0.5278	0.1634
	[-1.869]	[-3.720]	[-0.716]	[-0.804]	[-1.487]	[-1.570]	[0.254]	[0.073]
Pos	0.2268***	0.4769***	0.1815***	0.1963***	0.1607*	0.3143***	0.1700***	0.1473***
	[4.767]	[7.939]	[6.693]	[8.097]	[1.900]	[2.793]	[3.532]	[3.543]
abs(ΔFTD)	0.0228	-1.3646	1.0841	0.5188	1.9252	-2.6810	-0.9882	-2.7976
	[0.010]	[-0.589]	[1.016]	[0.549]	[0.508]	[-0.472]	[-0.554]	[-1.573]
Firm Fixed-Effect	yes	yes	yes	yes	yes	yes	yes	yes
Month Fixed Effect	yes	yes	yes	yes	yes	yes	yes	yes
Observations	40,344	40,329	40,323	40,303	12,836	12,832	12,825	12,822
Adjusted R2	0.003	0.006	0.004	0.004	0.004	0.007	0.004	0.005

Table 14 – Fail-to-deliver, Short Sales and Alternative Trading System (ATS) Trading Volume

This table presents the differences of outstanding fail-to-deliver and short sales between stocks with high and low trading volume in Alternative Trading System (ATS) for stocks listed in NYSE and NASDAQ for the period from January 1st, 2015 to August 31st, 2015. Aggregate Fail-to-Deliver is the aggregate outstanding fail-to-deliver in [T, T+3] divided by total share outstanding, in which T is the date of earnings announcement. Aggregate Short Sales is the aggregate outstanding short sales in [T, T+3] divided by total share outstanding. We create the portfolio based on 10 deciles of SUE score. Decile 1 of SUE score represents the most negative earnings surprise. Decile 10 of SUE score represents the most positive earnings surprise. The ATS trading volume data is from The Financial Industry Regulatory Authority (FINRA). ATS Trading Volume is based on the latest reporting date before the earnings announcement date. It is the total ATS trading volume during the reporting period divided by total share outstanding. Low ATS Trading is the ones which have ATS trading lower than the median level of whole sample, High ATS Trading is the ones which have ATS trading higher than the median level of whole sample. For each decile portfolio, the Diff is the difference of the measure between Small ATS Trading group and Large ATS Trading group. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

	Aggregate Fail-to-Deliver [0, +3]			
Decile of SUE score	**Low ATS Trading**	**High ATS Trading**	**Diff**	**p-value**
1	0.029%	0.085%	-0.056%	0.000***
2	0.026%	0.066%	-0.040%	0.000***
3	0.027%	0.062%	-0.035%	0.000***
4	0.023%	0.057%	-0.034%	0.000***
5	0.018%	0.052%	-0.033%	0.000***
6	0.015%	0.056%	-0.041%	0.000***
7	0.011%	0.048%	-0.037%	0.000***
8	0.020%	0.054%	-0.034%	0.000***
9	0.017%	0.029%	-0.013%	0.006***
10	0.015%	0.060%	-0.044%	0.000***

	Aggregate Short Sales [0, +3]			
Decile of SUE score	**Low ATS Trading**	**High ATS Trading**	**Diff**	**p-value**
1	0.452%	1.762%	-1.310%	0.000***
2	0.515%	1.768%	-1.253%	0.000***
3	0.566%	1.511%	-0.944%	0.000***
4	0.506%	1.572%	-1.066%	0.000***
5	0.412%	1.426%	-1.013%	0.000***
6	0.549%	1.383%	-0.833%	0.000***
7	0.510%	1.423%	-0.913%	0.000***
8	0.523%	1.435%	-0.912%	0.000***
9	0.510%	1.349%	-0.839%	0.000***
10	0.433%	1.688%	-1.254%	0.000***

Table 15 – Regression analysis of Fail-to-deliver, Short Sales and Alternative Trading System (ATS) Trading Volume

This table presents the differences of outstanding fail-to-deliver and short sales between stocks with high and low trading volume in Alternative Trading System (ATS) for stocks listed in NYSE and NASDAQ for the period from January 1st, 2015 to August 31st, 2015. The regressions are the same of the ones in Table 4, Table 5 and Table 7. Aggregate Fail-to-Deliver is the aggregate outstanding fail-to-deliver in [T, T+3] divided by total share outstanding, in which T is the date of earnings announcement. Aggregate Short Sales is the aggregate outstanding short sales in [T, T+3] divided by total share outstanding. We create the portfolio based on 10 deciles of SUE score. Decile 1 of SUE score represents the most negative earnings surprise. Decile 10 of SUE score represents the most positive earnings surprise. The ATS trading volume data is from The Financial Industry Regulatory Authority (FINRA). ATS Trading Volume is based on the latest reporting date before the earnings announcement date. It is the total ATS trading volume during the reporting period divided by total share outstanding. Low ATS Trading is the ones which have ATS trading lower than the median level of whole sample, High ATS Trading is the ones which have ATS trading higher than the median level of whole sample. Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively. In the table, t-statistics appear in brackets and are based on robust standard errors clustered by firm.

Panel A:

Both Exchange	**Low ATS Trading**		**High ATS Trading**	
	Aggregate Short Sales	**Aggregate Fail-to-Deliver**	**Aggregate Short Sales**	**Aggregate Fail-to-Deliver**
SUE Score	-0.3611		-0.5005	
	[-1.126]		[-0.478]	
Return 1-day lagged	-0.0076		0.0115	
	[-0.498]		[0.556]	
Return 2-days lagged	0.0297***		0.0176	
	[2.612]		[0.694]	
Aggregate Short Sales		0.0525***		0.0561***
		[5.165]		[9.884]
Illiquidity 1-day lagged		0.1270		-0.0109
		[1.224]		[-0.006]
Illiquidity 2-days lagged		0.1613		1.3580
		[1.416]		[0.857]
Month Fixed Effect	yes	yes	yes	yes
Observations	1,380	1,375	1,379	1,378
Adjusted R2	0.035	0.222	0.015	0.260

Panel B:

Both Exchange	Dependent variable: Abnormal Return							
	Low ATS Trading				High ATS Trading			
	t = 0	t+1	t+2	t+3	t = 0	t+1	t+2	t+3
Pos x abs(ΔFTD)	10.4723	11.9945	3.0479	10.2332	-2.1762	-13.6154	3.7547	-11.7266**
	[0.837]	[0.745]	[0.275]	[1.284]	[-0.198]	[-1.152]	[0.607]	[-2.257]
Pos	0.3606*	0.1960	0.2276*	0.2954**	0.2185	0.6578*	-0.2118	0.1527
	[1.674]	[0.687]	[1.658]	[2.547]	[0.876]	[1.919]	[-1.370]	[1.197]
abs(ΔFTD)	-2.4861	-7.2445	-7.9851	-9.0057*	2.9019	2.4472	-6.6022	3.7939
	[-0.342]	[-0.482]	[-0.808]	[-1.813]	[0.305]	[0.253]	[-1.328]	[0.888]
Month Fixed Effect	yes	yes	yes	yes	yes	yes	yes	yes
Observations	1,378	1,378	1,378	1,378	1,379	1,379	1,379	1,378
R-squared	0.001	-0.002	0.001	0.007	-0.001	0.011	0.004	0.005

Figure 1. Short Interest of stocks listed in NYSE and NASDAQ (2005 – 2015)

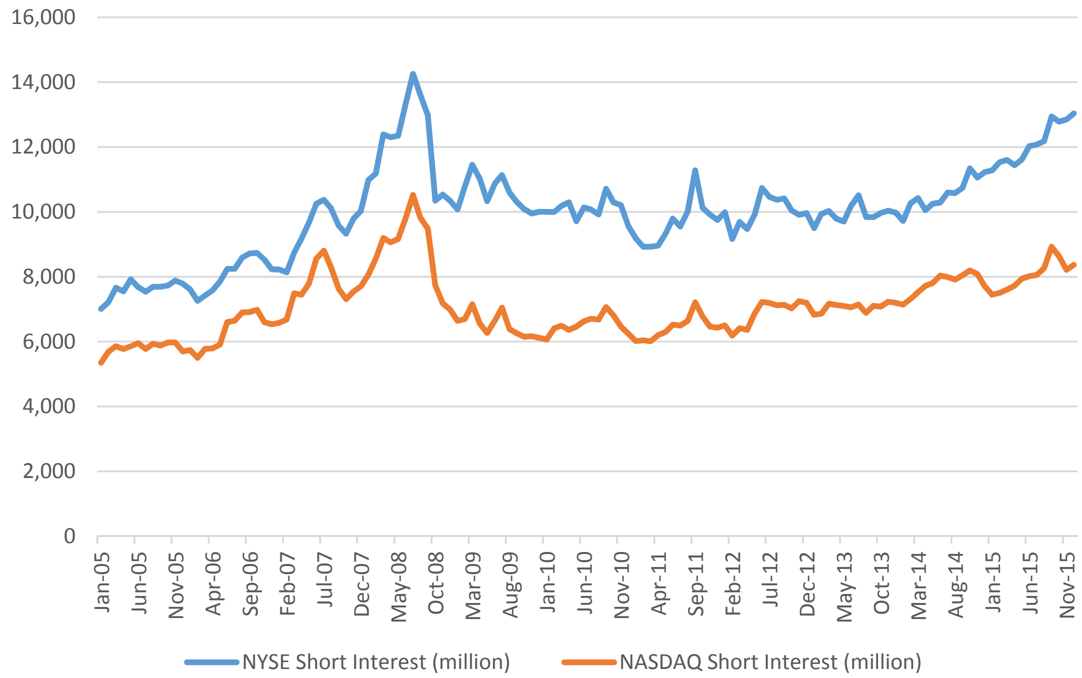


Figure 2. Short Interest Ratio and S&P 500 Index (2005 – 2015)

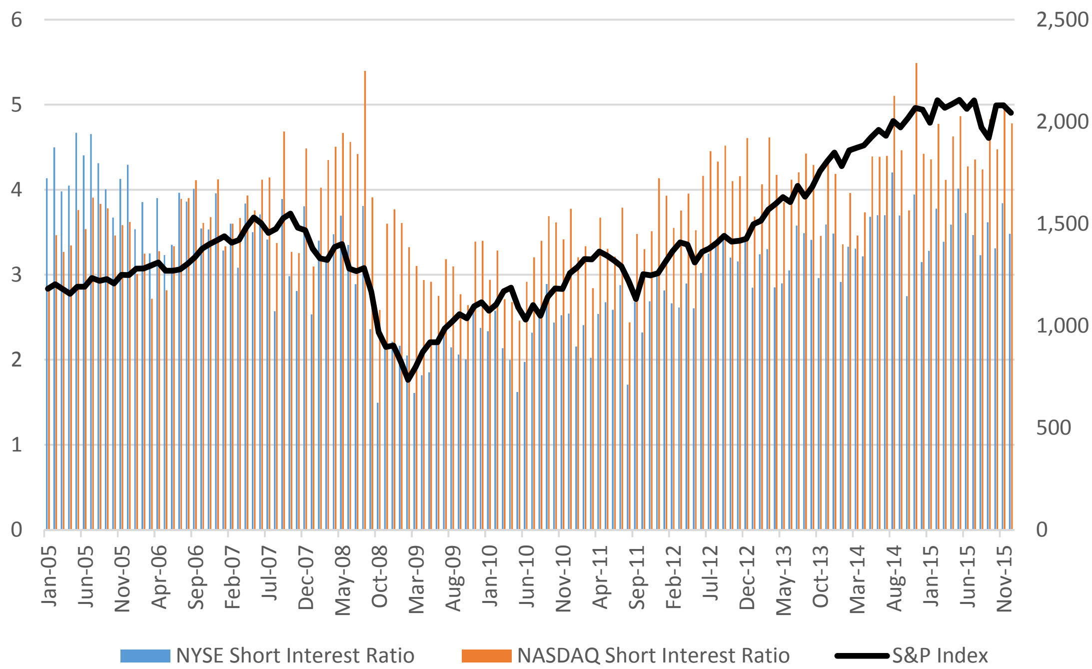


Figure 3. Short Sales Volume of stocks listed in NYSE and NASDAQ (2009 Oct – 2015 Dec)

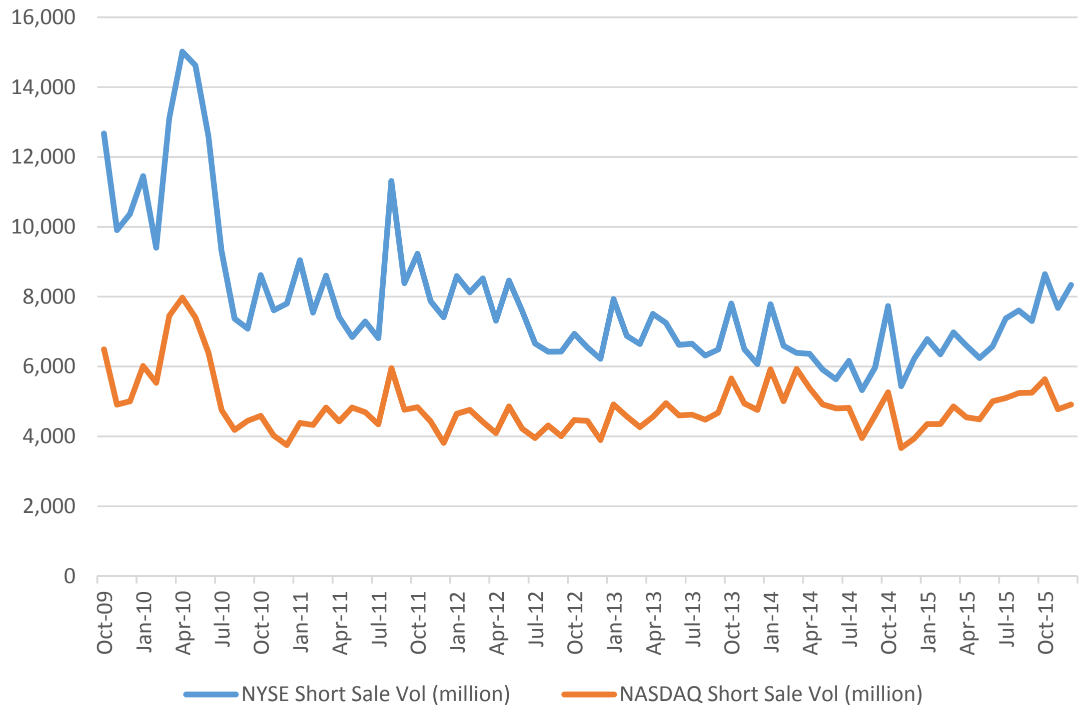


Figure 4. (Short Sales Volume / Total Volume) Ratio and S&P 500 Index (2009 Oct – 2015 Dec)

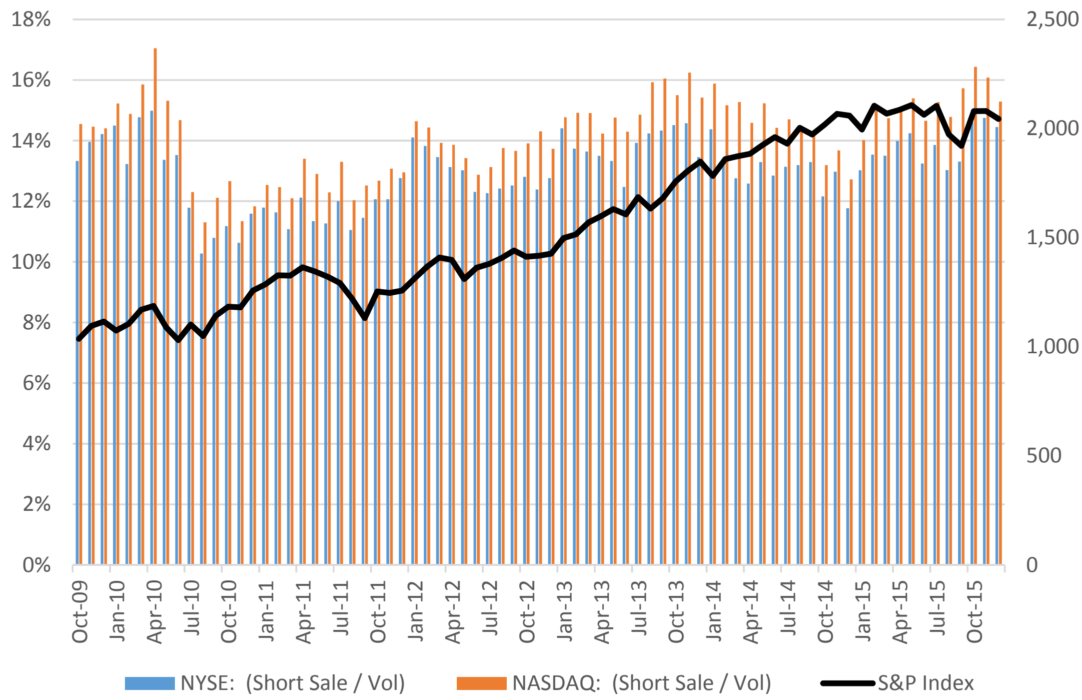


Figure 5. Fail-to-Deliver (FTD) Volume in NYSE and NASDAQ (2009 Oct – 2015 Dec)

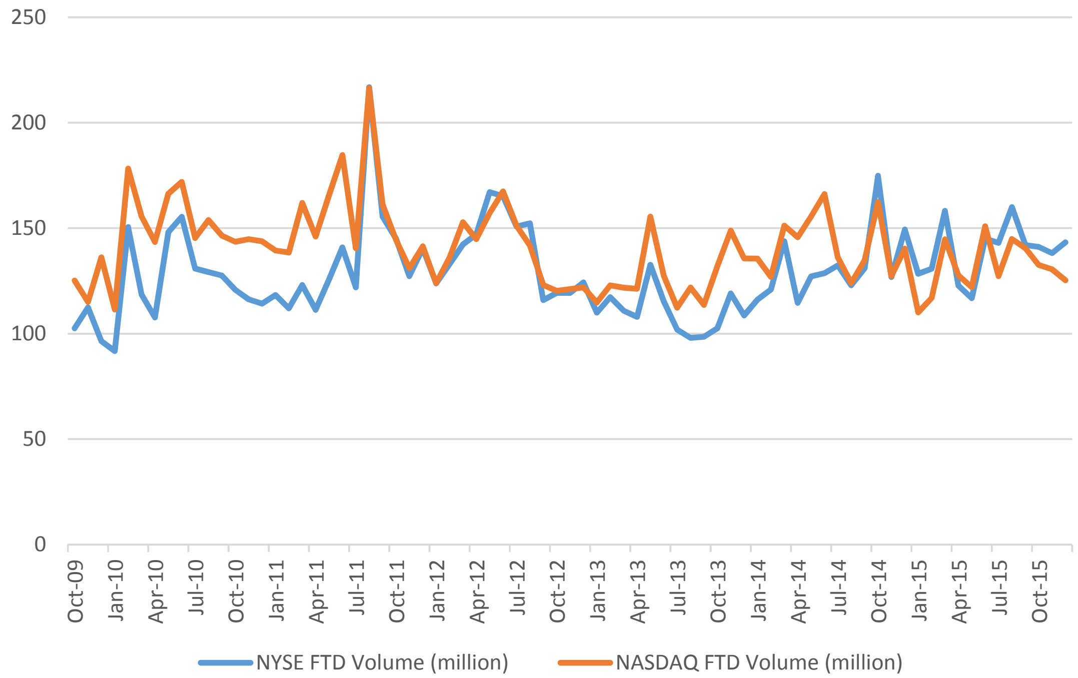


Figure 6. Fail-to-Deliver (FTD) Monetary Value in NYSE and NASDAQ (2009 Oct – 2015 Dec)

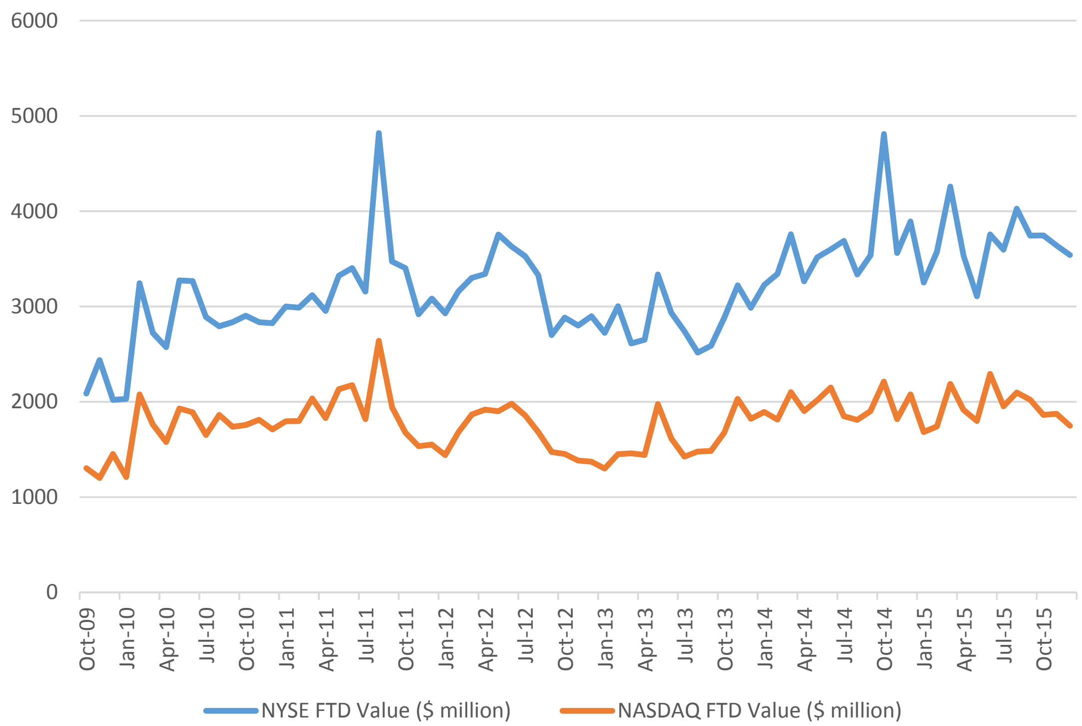


Figure 7. FTD Volume/Total Volume ratio in NYSE and NASDAQ (2009 Oct – 2015 Dec).

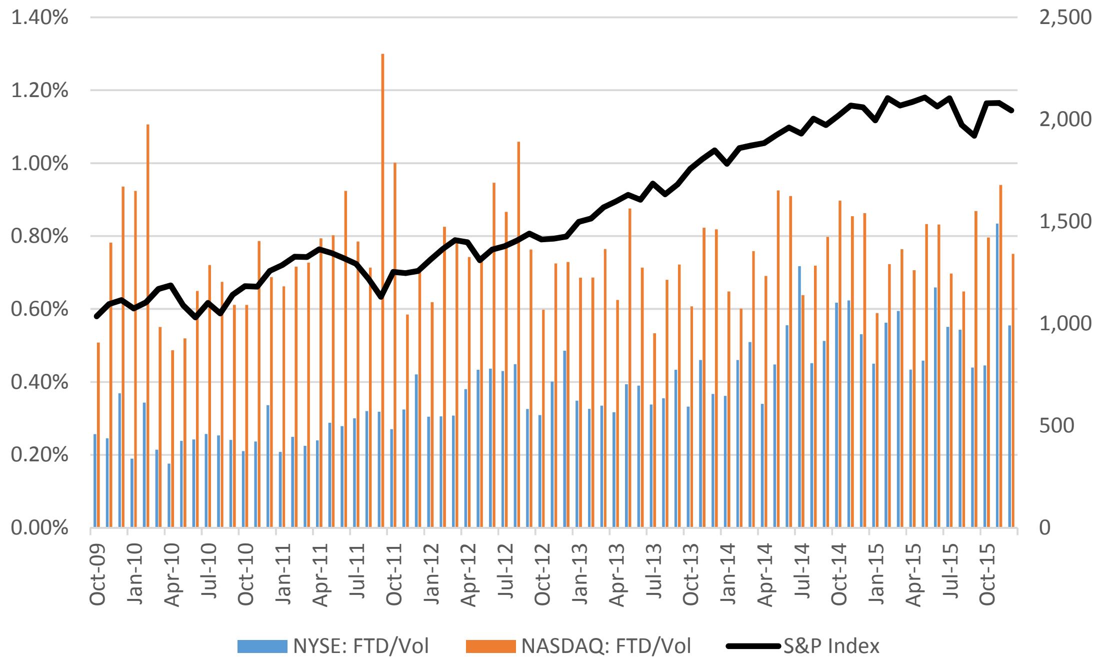


Figure 8. FTD Volume /Short Sales Volume ratio in NYSE and NASDAQ (2009 Oct – 2015 Dec)

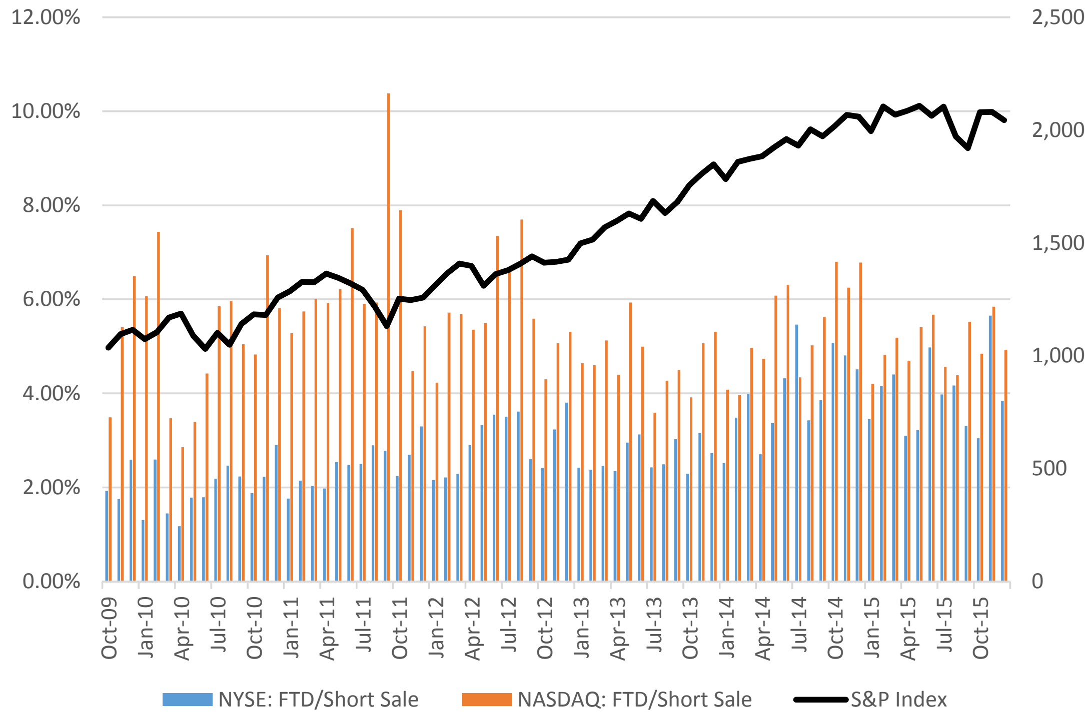
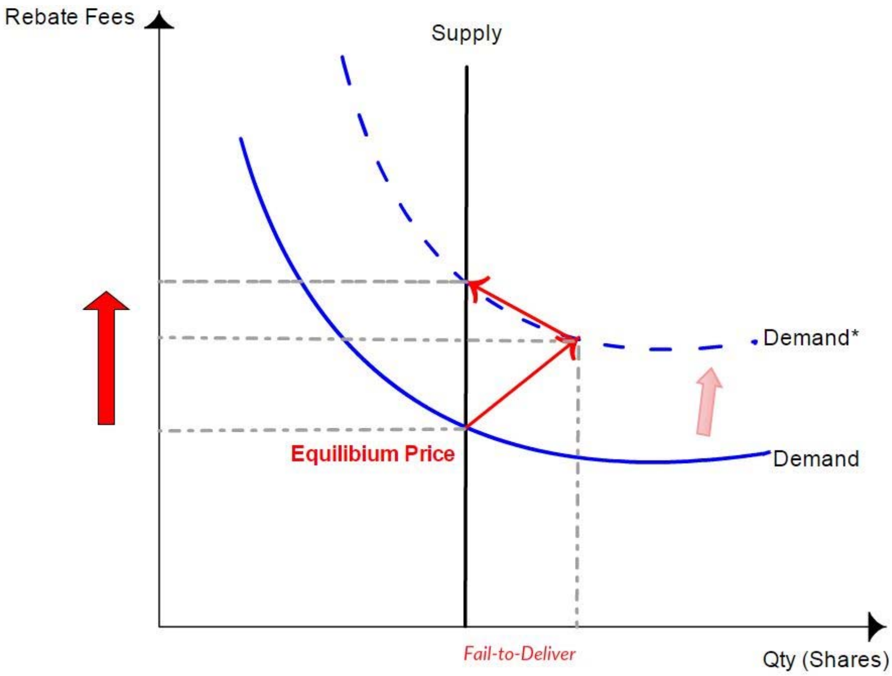


Figure 9. Effect from Security Lending Market to Stock Market

Panel A. Stock Lending Market



Panel B. Stock Trading Market

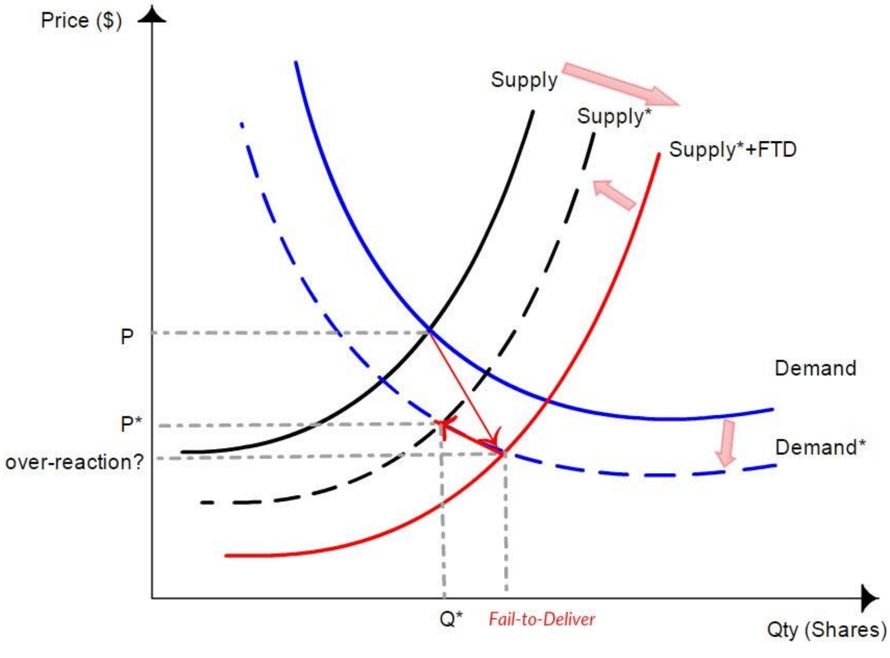


Figure 10. CAR around Earnings Announcement with Negative Surprise

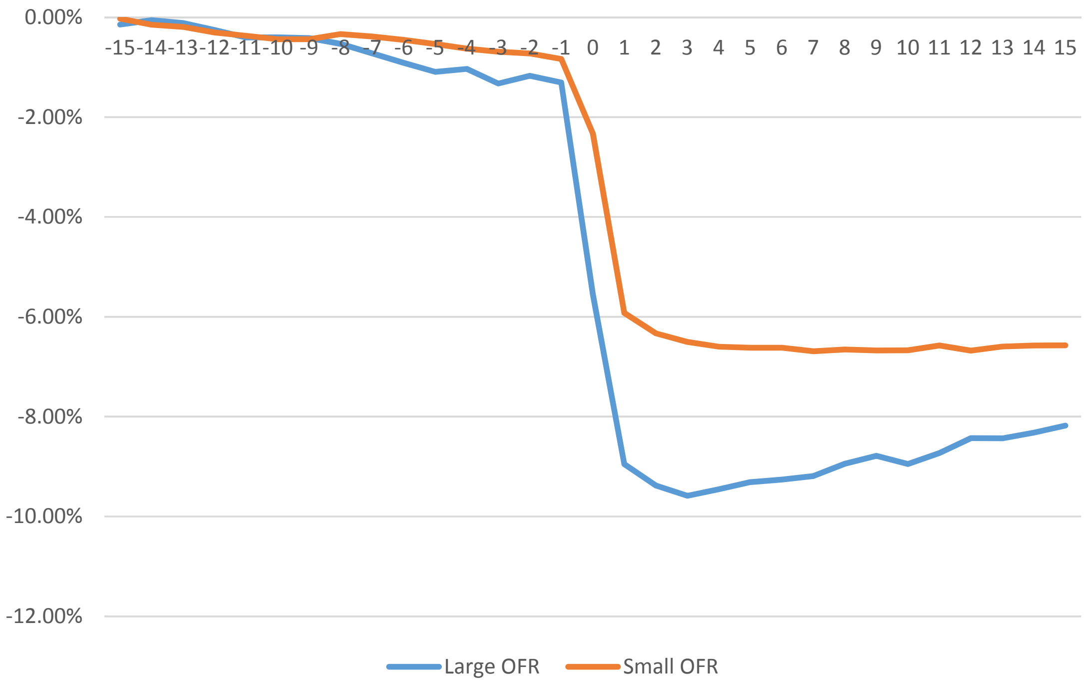
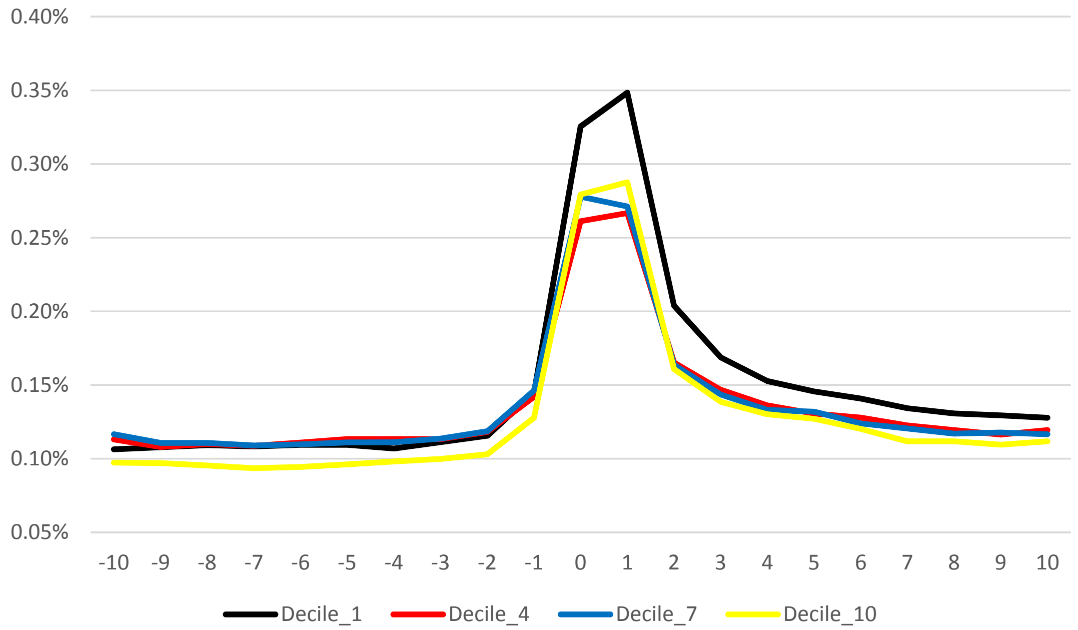


Figure 11. OSR around Earnings Announcement categorized by surprise level

Panel A. stocks listed in NYSE



Panel B. stocks listed in NASDAQ

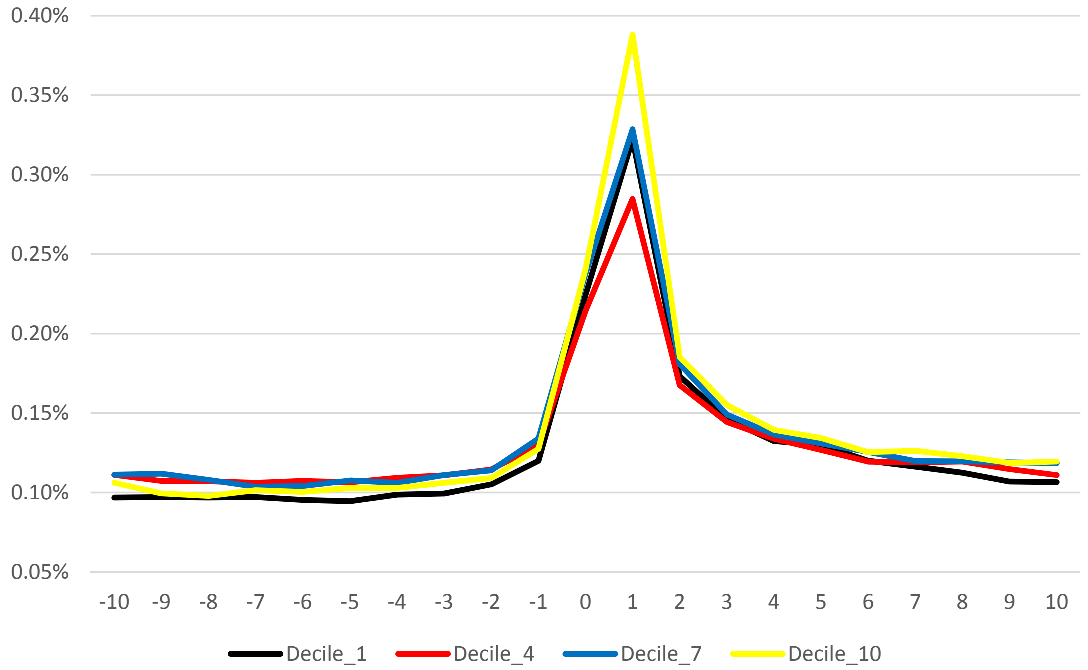
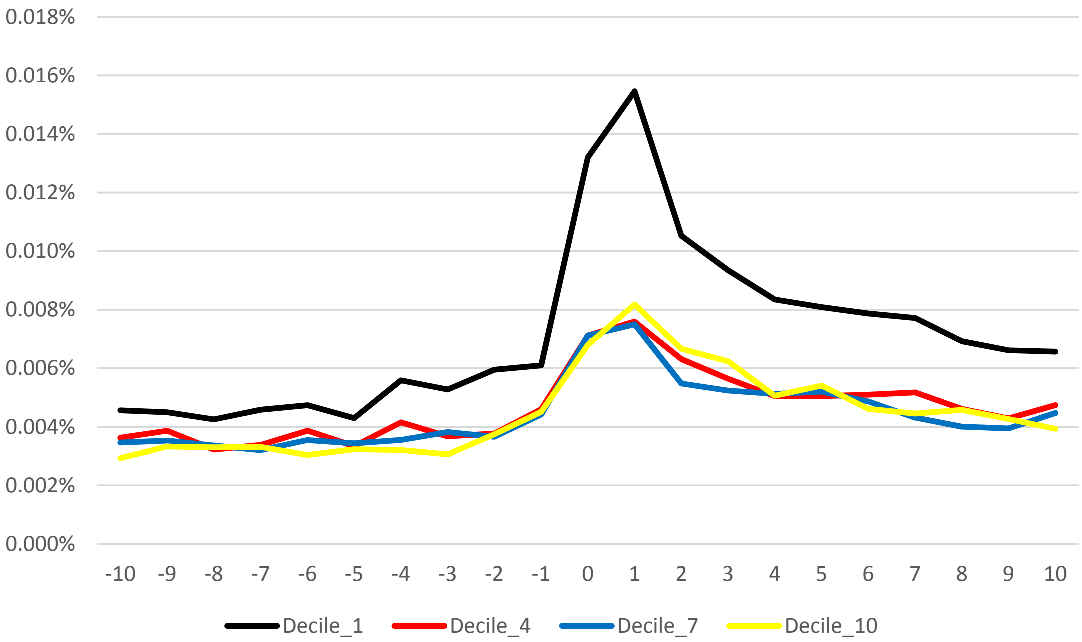


Figure 12. OFR around Earnings Announcement categorized by surprise level

Panel A. stocks listed in NYSE



Panel B. stocks listed in NASDAQ

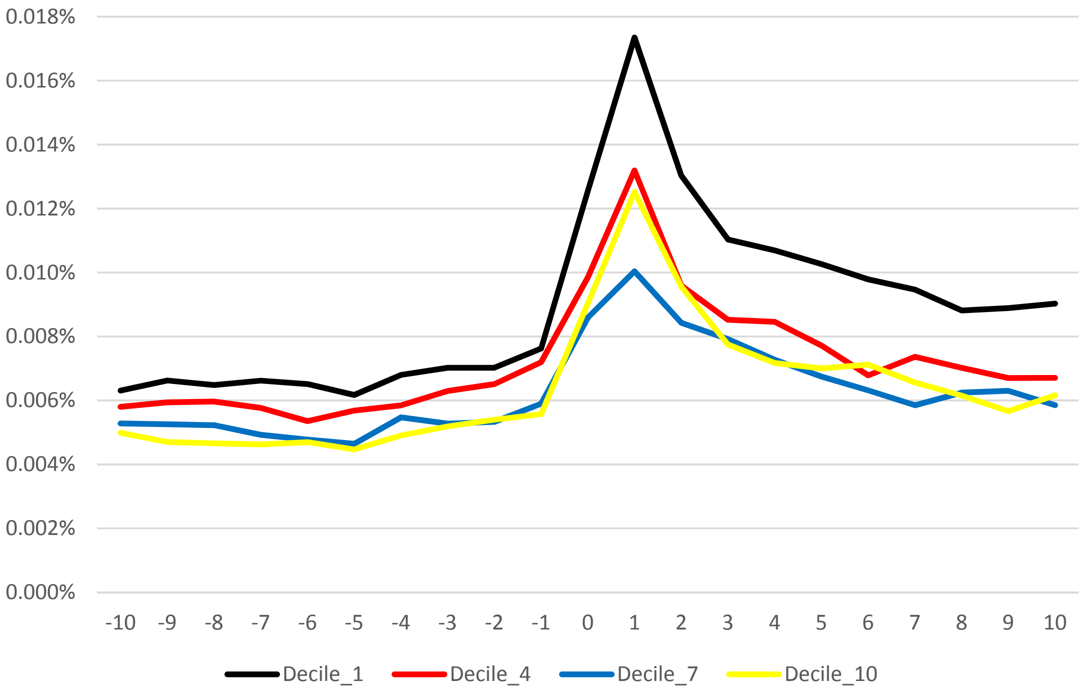
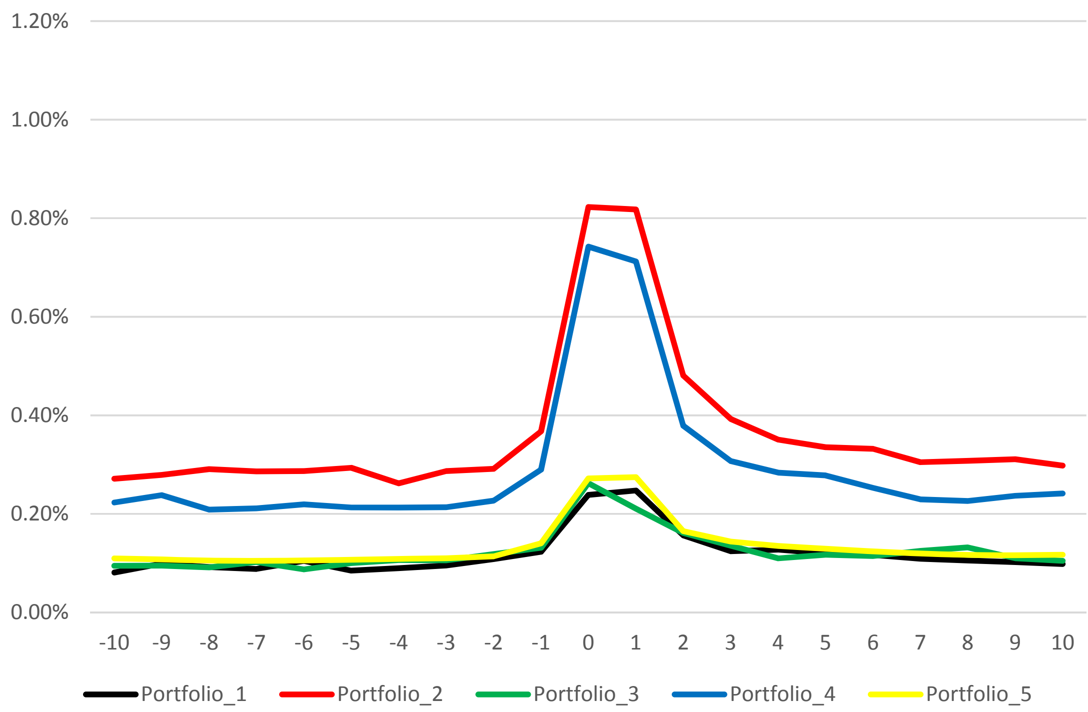


Figure 13. OSR around Earnings Announcement categorized by portfolio

Panel A. stocks listed in NYSE



Panel B. stocks listed in NASDAQ

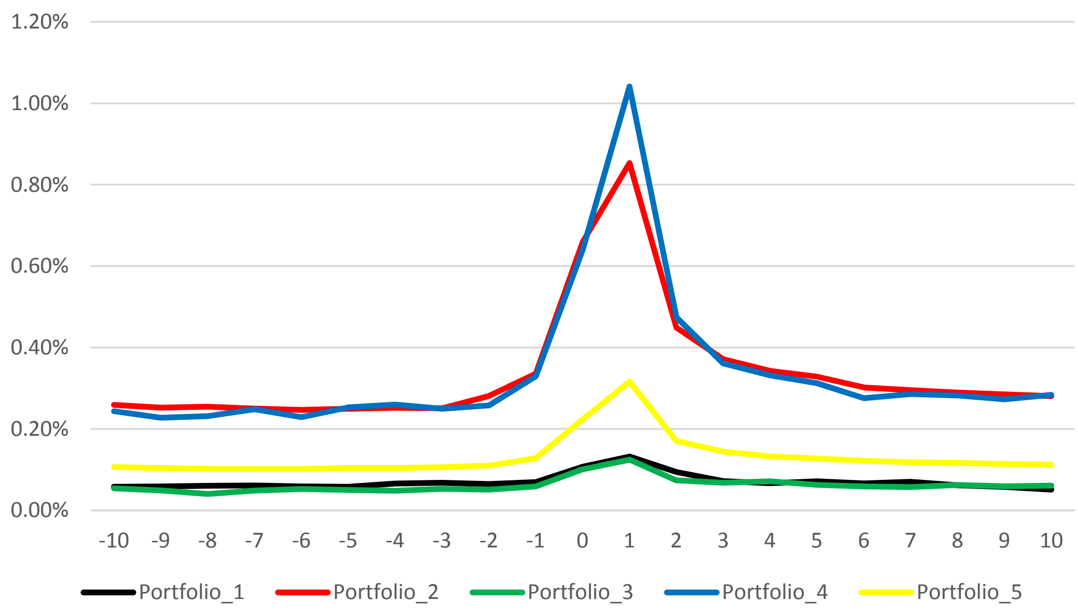
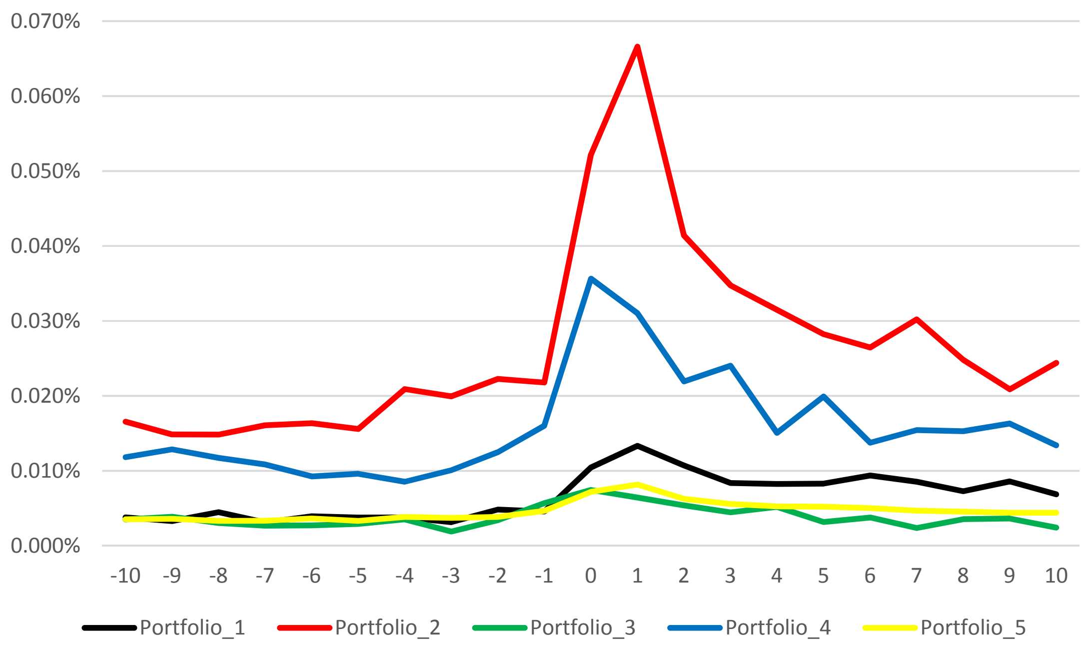
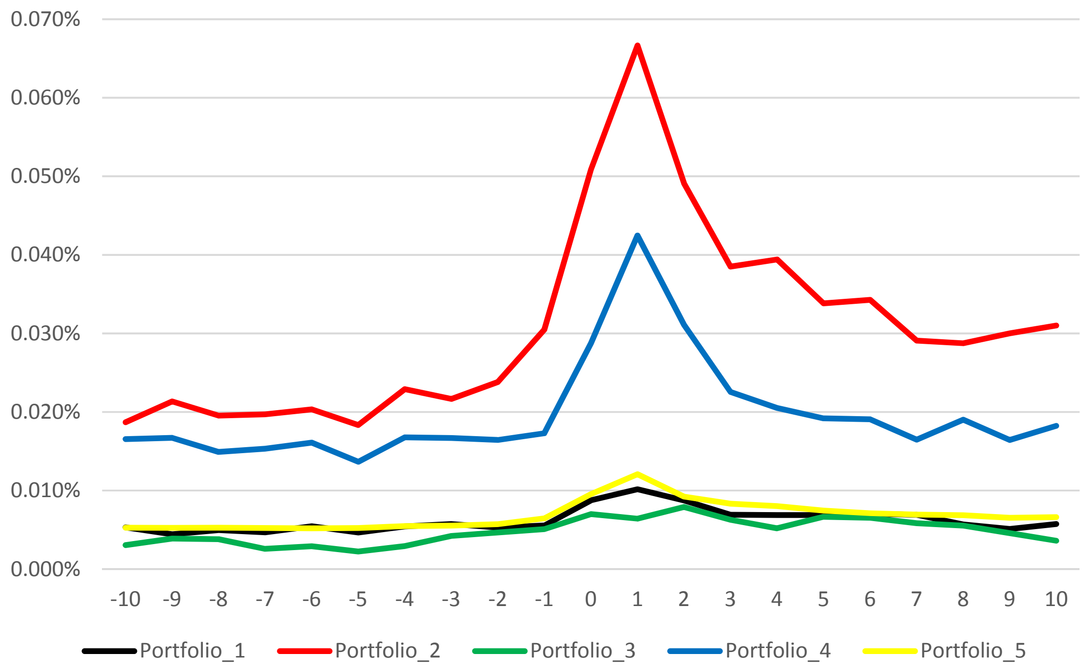


Figure 14. OFR around Earnings Announcement categorized by portfolio

Panel A. stocks listed in NYSE



Panel B. stocks listed in NASDAQ



Chapter 2. Hedge Fund Activism and Long-Term Firm Value

(joint work with K.J. Martijn Cremers,[*] Erasmo Giambona,[◊] Simone M. Sepe[+])

1. Introduction

Hedge fund activism has turned into a permanent force of corporate governance. Activist campaigns targeting publicly traded firms have steadily increased in the past ten years (Coffee and Palia, 2015). The governance changes sought by activists range from modest proposals, such as separating the positions of CEO and Board Chairman, to more radical interventions, such as firing the CEO or selling major assets or the firm to an acquirer. This increased activism has changed the U.S. corporate landscape, further undermining Berle and Means' canonical account of corporate governance (Gilson and Gordon, 2013). Under that account, shareholders in large public firms are portrayed as widely dispersed and, consequently, face collective action problems whose only remedy against managerial underperformance is the "Wall Street Rule" (i.e., the "exit" option to sell shares). In today's corporate environment, however, increased institutional shareholder concentration and hedge fund activism have empowered shareholders with the ability to exercise influential "voice" over the corporate affairs.

In this paper, we revisit the results of prior empirical studies (Brav, Jiang, Partnoy and Thomas, 2008a; Bebchuk, Brav and Jiang, 2015) suggesting that hedge fund activism is beneficial to shareholder interests in *both* the short-term *and* the long-term. Our starting point is the result in Brav et al. (2008a) that activist hedge funds resemble value investors, as they show that activist hedge funds tend to target firms that have been relatively poorly performing prior to the activists' interventions. Accordingly, funds targeted by activist hedge funds are not randomly selected, and the evaluation of their subsequent performance should take this selection effects carefully into consideration, which we do in this paper by comparing the long-term financial value of targeted firms to that of non-targeted firms whose prior performance was similar to that of the targeted firms.

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[*] Mendoza College of Business, University of Notre Dame. *Email address*: mcremers@nd.edu.
[◊] Syracuse University. *Email address*: egiambon@syr.edu.
[+] University of Arizona and Institute for Advanced Study in Toulouse (IAST) – Fondation Jean-Jacques Laffont – Toulouse School of Economics. *Email address*: sms234@email.arizona.edu.

Prior studies (such as Brav, Jiang, Ma, and Tian, 2014; Cheng, Huang, and Stanfield, 2012; and Cheng, Huang, and Li, 2015) have reported related results using matched samples, but they studies did not focus on long-term financial performance.

Our main contribution is to show that the positive long-term association of hedge fund activism and firm value documented in the prior literature seems endogenous and on average unlikely to be caused directly by the activist campaigns. We do so through constructing several different matched samples, where each firm targeted by an activist hedge fund is matched to a control firm with similar characteristics (especially with similar performance prior to the start of the activist hedge fund campaign). In particular, firms targeted by activist hedge funds improve less in value subsequent to the start of an activist hedge fund campaign than *ex-ante* similarly poorly performing control firms that are not subject to hedge fund activism. In other words, our different matched-samples consistently suggest that hedge fund activism (and especially the activism of hedge funds using hostile tactics) is associated with *lower* increases in firm value in the long-term *relative* to non-targeted control firms with similar characteristics as the targeted firms. This indicates that interventions – initiated by the board of directors, top executives, the market for corporate controls, etc. – other than activist hedge fund campaigns seem on average more successful than the typical activist hedge fund campaign in turning these relatively poorly performing firms around.

We first revisit the results in Bebchuk et al. (2015), using the same database of activist hedge fund campaigns from the (updated) Brav et al. (2008a) dataset, which Bebchuk et al. (2015) also employs.[1] This dataset covers the period 1995-2011 and identifies hedge fund interventions through Schedule 13D filings, which the 1934 Security Exchange Act requires investors acquiring more than 5% percent of any class of security of public companies to file with the Security and Exchange Commission (SEC) within 10 days of doing so. We closely replicate the results of Bebchuk et al. (2015), including their result that in the five years after the start of the activist hedge fund campaign, the *Q* (the widely used proxy for the firm's market-to-book value of assets) of targeted firms progressively increases, and more strongly so when the hedge fund campaign is

[1] We thank Alon Brav for making the data available to us.

classified as hostile in the Brav et al. (2008a) dataset. Similarly, portfolios formed to capture the stock market performance of targeted firms exhibit positive abnormal returns in the 1-year and 5-year period following the start of the activist hedge fund campaign.

Next, we confirm the selection result in Brav et al. (2008a) that firms targeted by hedge funds are substantially different from other firms by predicting hedge fund activism through logit and Cox proportional hazard models. Similar to Brav et al. (2008a), we find that firms are much more likely to become the target of hedge fund activism if they have been performing relatively poorly in the past one to five years—that is, hedge funds seem to primarily target firms with relatively low values. This selection result, in turn, raises the possibility that the increase in the value of targeted firms might be attributable to market mechanisms other than the intervention by activist hedge funds. Indeed, in competitive markets, many different actors can intervene to turn things around at a relatively poorly performing company, including key employees, top executive management, directors, long-term shareholders, as well as other stakeholders like large customers or suppliers. We consider this possibility by creating a variety of matched samples. In each matched sample, for each "target" firm that is targeted by an activist hedge fund we assign a "control" firm that has similar characteristics (using characteristics that we document matter for being targeted) as the target firm in the year before the start of the target firm's activist hedge fund campaign.

Using such matched samples, we consistently find that the long-term financial performance of targeted firms improves less than the long-term financial performance of the control firms, using both changes in Tobin's Q and abnormal stock returns as proxies for changes in financial value. In our baseline results, we use a matched sample where the control firms are matched using the Abadie-Imbens (2006) nearest-neighbor matching estimator to identify the control firms with the same 1-digit SIC industry code that is the closest match based on Tobin's Q (lag 1 to 5), the natural logarithm of market value of equity (lag 1), leverage (lag 1), ROA (lag 1), and fiscal year. While we discuss the results for this particular matched sample below, these results are similar across several other alternative ways to assign control firms that we conducted as robustness checks. We also report and briefly discuss the results for these alternative matched samples.

Using the baseline matched sample, the long-term increase in *Q* of the targeted firms in the years after the start of an activist hedge fund campaign is on average significantly below the increase in *Q* of the control firms, where the differences are statistically strong and economically meaningful. Specifically, starting with the target and controls firms having a similar value in the year before the start of the activist campaign, the firm value of the target firms tends to be, on average, 7.5% lower than the firm value of control firms in the three years after the activist hedge funds start their campaign, and about 13.3% lower in the period thereafter.[2]

Similarly, stocks of targeted firms have positive abnormal returns with respect to the four-factor Fama-French-Carhart model in the 3-year and 5-year period following the start of the activist hedge fund campaign, consistent with Bebchuk et al. (2015) and suggesting that activist hedge funds on average seem to have significant stock picking skills. However, we find that their control firms have even more positive abnormal stock returns, where the relative outperformance of the control firms is statistically significant for equal-weighted portfolios. In particular, we form a calendar-time long-short portfolio that, after the start of the activist hedge fund campaign, buys the stocks of targeted firms and sells the stocks of the control firms. If the long and short portfolios are equal-weighted, the long-short portfolio has a monthly four-factor alpha of -0.25% (t-statistic of 2.50) in the 3-year period following the activist campaign and of -0.20% (t-statistic of 2.41) over the subsequent 60 month period. Large control firms do not outperform large targeted firms, as the analogous results using value-weighted portfolios are statistically insignificant: a four-factor alpha of 0.11% per month (t-statistic of 0.35) and of 0.13% per month (t-statistic of 0.42) when holdings stocks 3 and 5 years, respectively, after being targeted.

In the remainder of our paper, we explore our main finding that the financial performance of firms targeted by activist hedge funds improves but on average less so than of similarly underperforming firms. First, we consider whether any changes in corporate policies following activist campaigns, on average, contribute to improved financial performance relative to the control

[2] This striking result is robust across many different matching procedures—including nearest neighbor matching and propensity score matching—but also to adding different fixed effects (including year, industry, firm, and higher dimensional effects such as year times industry fixed effects), or to incorporating the acquisition premium after firms are taken over (or more generally to incorporating the delisting price). Further, Brav et al. (2008a) also document that the ROA of targeted firms increases after the start of activist hedge fund campaigns, but we find that such increase is not statistically different from the increase in the ROA of the control firms over the same period.

firms. We show that targeted firms tend to increase leverage, increase stock buybacks and decrease capital expenditures in the 3 year period following the start of activist hedge fund campaigns, relative to the sample of control firms, and decrease R&D expenditures in the period after that. This suggests that, after being targeted, firms become riskier and less focused on long-term investments.

Next, we consider how the ability shareholders, especially activist hedge funds, to help change corporate policies or firm control in the short-term may complicate both managerial-decision making and the extent to which other stakeholders want to invest in their relationship with the firm. In anticipation of such potential short-term policy and control changes, managers may develop myopic incentives (Stein, 1988, 1989; Karpoff and Rice, 1989; Bradenburger and Polak, 1996) and important stakeholders might be discouraged to invest optimally in the firm (Shleifer and Summers, 1988; Johnson, Karpoff and Yi, 2015). This motivates our hypothesis that by enhancing shareholders' ability to pressure directors and managers, hedge fund activism could lead to a reduced focus on particularly long-term and firm-specific investments by managers and (non-shareholder) stakeholders, possibly resulting in reduced long-term firm value.

We test this hypothesis in the last part of our paper, exploring whether our main finding might be explained by hedge funds influencing a firm's investment policy and other operational decisions to the detriment of long-term investments and strong stakeholder relationships. Under our hypothesis, we should find that the relative long-term underperformance of targeted versus non-targeted firms is more pronounced for firms whose investments have a longer-term horizon or where stakeholders are more important. We first focus on firms that are more engaged in innovation, whose investments naturally tend to have a longer-term horizon. Using different proxies to identify more innovative firms (e.g., high R&D expenses, high intangible assets, and high patent citations), we document that when the target of hedge fund activism is an innovative firm, the decline in Q in the three years following the activist intervention tends to be more severe, at economically and statistically significant levels. Second, we consider firms with stronger relationships with other stakeholders such as employees, suppliers, and unsecure creditors. Consistent with our hypothesis, we find that when these stakeholder relationships matter more, targeted firms experience on average a more severe decline in Q in the three years after the intervention, relative to the firm value of the matched control firms.

The rest of the paper is organized as follows. Section 2 describes our data sets and provides a definition of our main variables. Section 3 presents our main findings on the relation between hedge fund activism and long-term firm value. The results on the effects of hedge fund activism on firm value for firms facing limited commitment problems are discussed in Section 4. Section 5 concludes.

2. Data and Variable Definitions

To assess the association of hedge fund activism with long-term firm value, we combine data from several data sources. The hedge fund data is from Brav et al. (2008), and covers the period 1995 – 2011. The procedure to obtain the hedge fund data is explained in Brav et al. (2008), who use Schedule 13D filings as their main source. The 1934 Security Exchange Act requires that investors file a 13D form with the Security and Exchange Commission within 10 days of acquiring 5% of any class of securities of a publicly listed firm if the reason for such acquisition is to influence the management of the target firms. The authors use information on the filer type available in Item 2 of Schedule 13D to limit the sample to only hedge funds, filtering out other filers such as banks, brokerage companies, corporations, insurance companies, individuals, pension funds, and trusts. Brav et al. further rely on web-searches, newswires, 13F holdings reports and direct phone calls to help identify additional events where an activist hedge fund acquires less than 5% of equity and thus does not file a 13D. Their various screenings generate a sample of 480 hedge funds and 2,684 events.[3] Using newswires and other sources, Brav et al. categorize 604 events (by 210 hedge funds) out of the 2,684 events as hostile hedge fund interventions, i.e., where the intervention "includes a threatened or actual proxy contest, takeover, lawsuit, or public campaign that is openly confrontational."

In order to identify firms where stakeholders are particularly important or where the limited commitment problem is particularly relevant, we use three different proxies: *Patent Citation* counts, *Contract Specificity*, and *Labor Productivity*. We obtain data on patent citation counts at the firm level from the NBER U.S. Patent Citations data file. Our *Contract Specificity* proxy is the fraction

[3] We refer the reader to Brav et al. (2008) for additional details on the construction of the hedge fund sample.

of inputs in an industry that are not sold in an organized exchange or reference priced in a trade publication, as made available for 1997 only in the Nunn (2007) data file. Our *Labor Productivity* proxy, the output per hour of labor in the firm's industry, comes from the Bureau of Labor of the U.S. Department of Labor.

We combine the hedge funds data and the other data sources with firm-level accounting data from COMPUSTAT and return data and delisting information from the Center for Research in Security Prices (CRSP). We use the return data to assess the market reaction for target and control firms around the hedge fund targeting dates and restrict our sample to non-financial firms (excluding firms with SIC codes 6000 – 6999).

Our proxy for financial value is *TobinQ*, measured as the ratio of the market value of total assets (COMPUSTAT's items *at – ceq + prcc_f×csho*) to the book value of total assets (*at*). Our set of basic control variables includes the following measures. *LnSize* is the natural logarithm of the book value of total assets (COMPUSTAT's item *at*). *Leverage* is defined as the ratio of total debt (COMPUSTAT's items *dltt + dlc*) to the book value of total assets. *CAPX* is the ratio of capital expenditures (COMPUSTAT's item *capx*) to the book value of total assets. *Intangibility* is one minus the ratio of property, plant, & equipment (COMPUSTAT's item *ppent*) to the book value of total assets. *ROA* is the ratio of operating income before depreciation (COMPUSTAT's item *oibdp*) to the book value of total assets. Ln Market Value of Equity is the natural logarithm of market value of equity (COMPUSTAT's items *prcc_f × csho*). To avoid undue influence of outliers, we winsorize all continuous variables at the 5th and 95th percentiles of their full sample distributions, though we have confirmed that our results are robust to winsorizing Tobin's Q at the 1st and 99th percentiles as well.

Panel A of Table 1 provides a brief description of all of the variables used in our study, while Panel B of Table 1 reports the basic descriptive statistics. The average *TobinQ* in our sample is 2.664, and its 25th and 75th percentiles are respectively 1.130 and 2.805, which suggests that there is significant heterogeneity in firm value in the sample.

3. Hedge Funds and Firm Value

3.1. Replicating the Results in Bebchuk et al. (2015)

Our focus is on the association of hedge fund activism with long-term firm value. We start by replicating Table 4 in Bebchuk et al. (2015), regressing the firm's annual Tobin's Q at the end of the fiscal year on time dummies, the log of market value (COMPUSTAT's *prcc_f* × *csho*), the log of firm age (measured as the number of years since the firm first appeared in COMPUSTAT) at the end of the fiscal year, and either industry (3-digit SIC) and year fixed effects, or firm and year fixed effects. The time dummies are defined as follows:

- *"t: Event year"* is an indicator equal to one for firms first targeted by an activist hedge fund sometime during the fiscal year, and zero for every other year before or after that year. The "t: Event year"-dummy is always equal to zero for firms not targeted by an activist hedge fund during our sample period;
- "*t+1*" is an indicator variable equal to 1 for firms first targeted by an activist hedge fund in the previous fiscal year, and zero otherwise;
- *"t+2"* to *"t+5"* dummies are defined similarly to the "t+1" indicator, capturing the fiscal years 2 to 5 years after the year the firm was initially targeted.
- *"(t to t+3)"* is an indicator variable equal to 1 for firms targeted by a hedge fund in that fiscal year or one of the previous 3 fiscal years, and zero otherwise.
- *"Post t+3"* dummy is equal to 1 for firms targeted by a hedge fund at least 4 (or more) fiscal years later, and zero otherwise.

Table 2 reports results from these estimations, which generally replicate the results in Bebchuk et al. (2015). Columns (1) – (4) show the results considering all activist hedge funds, while columns (5) – (8) only consider firms targeted in a hostile manner by an activist hedge fund. The results in column (1) shows that firms targeted by an activist hedge fund tend to have a substantially lower value than other firms in the industry at the end of the year in which they are first targeted (i.e., the event year), but that this value discount has disappeared five years after the event year. Indeed, column (2) shows that in the period starting at least three years after the event year, firm value tends to be significantly higher compared to other firms in the same industry (as shown by the coefficient of 0.191 (t-statistic of 4.01) of the "Post t+3" variable).

In columns (3) and (4), we add firm fixed effects rather than industry fixed effects, effectively comparing how firm value changes over time before versus after a firm is targeted by an activist hedge fund. Similar to the results in Bebchuk et al. (2015), we find that firm value increases in the years after a firm is targeted by an activist hedge fund. Economically, being targeted by an activist hedge fund is associated with an increase in firm value by about 9% three years afterwards (i.e., the coefficient of 0.265 of “Post t+3” in column 4, divided by the sample mean of 2.939). Finally, columns (5) – (8) indicate that the increase in firm value is more pronounced for firms targeted in a hostile campaign of an activist hedge fund.

3.2. The Ex-Ante Probability of Becoming a Hedge Fund Target

The results in Table 2 show that firm value tends to improve in the years after a firm is targeted by an activist hedge fund. However, this result needs to be interpreted with great caution, as the decision to target a particular firm at a particular time is an entirely discretionary choice by the activist hedge fund. Hence, firms being targeted by hedge funds could potentially be substantially different from other firms. Because of this possibility, it seems important to understand what type of firms tend to be targeted by activist hedge funds, and then to compare the performance of the firms being targeted (the “target” firms) to other firms that have similar characteristics but have not (yet) been targeted by an activist hedge fund (the “control” firms).

We consider what firm characteristics are associated with becoming a target in the next fiscal year by estimating a logit model (see Panel A of Table 3) and a Cox proportional hazard model (see Panel B of Table 3). These estimations allow us to assess which lagged variables help predict the probability that a firm will be targeted by a hedge fund in the next fiscal year, or how close a firm is to becoming a hedge fund target. In our discussion, we will focus on the logit model results shown in Panel A of Table 3, although we obtain similar results for the analogous Cox proportional hazard model (shown in Panel B of Table 3).

As shown in Panel A of Table 3, firms that are targeted by activist hedge funds tend to have relatively low valuations before they are being targeted. They also tend to be larger in size and, depending on the specification, tend to be more likely to be involved in research and development or have more intangible assets. Column (1) shows that the coefficient of Tobin’s Q at t-5 is negative and statistically significant at the 1% level, suggesting that firms with a high valuation in their industry are significantly less likely to become a target. We find similar effects for lagging the Tobin’s Q variables by four to one years in columns (2) to (5). If we include all of the *Q* variables together in column (6), all of the coefficients on the lags of *Q* become smaller in absolute value, while several coefficients also become statistically insignificant. This suggests that although it is primarily the most recent lagged *Q* that matters for future hedge fund interventions, the longer history of lagged valuations is also helpful for predicting which firms are more likely to become a target. LnSize is statistically significant across all specifications, while the other control variables are statistically significant in certain specifications but not in others. For example, this is the case for ROA, which is positively significant in column (1), but insignificant in the other specifications in Panel A.

3.3. Matched Sample

In order to control for firm heterogeneity, we create different matched samples of the 'target' firms that are targeted by an activist hedge funds and their "control" firms, which have similar characteristics as the target firms in the year before the first activist hedge funds files a 13D. In our 'Main Matched Sample', we match target firms to control firms using the nearest-neighbor Abadie-Imbens (2006) matching estimator, where control firms are a subset of the non-target firms selected as the closest match based on firm characteristics. To guide our selection of the matching variables, we consider the variables in our logit or Cox model estimations that showed up as statistically significant in at least one of the specifications in Panels A and B of Table 3. We drop (add) the statistically significant (insignificant) variables from (to) the set of matching variables if including (excluding) them leads to statistical differences between target and control firms after the matching has taken place.

Our final Matched Sample is based on the following matching variables for firms with the same one-digit SIC code: Tobin's Q (lags 1 to 5), leverage (lag 1), ROA (lag 1), the log of market value (lag 1), and the fiscal year. Table 4 shows that target firms (with the results for all hedge fund targets in Panel A and for hostile hedge fund targets only in Panel B) and control firms are similar both in terms of the matching variables and other important firm characteristics that were not included in the matching. For example, the Tobin's Q is not statistically different for target and control firms in the five to one years prior to the targeting year. Similarly, target and control firms are very similar in terms of their market value, leverage, ROA, log of market capitalization, CAPX, R&D, and Intangibility in the year prior to the targeting event. The similarity in these characteristics means that using the matched sample, differences between the two groups of firms in the years prior to the targeting event are unlikely to be the reason for any divergence in the value of the target and control firms in the years following a hedge fund targeting event.

For robustness, we also use several other matching procedures, obtaining additional, alternative matched samples. For the Alternative Matched Sample 1, we use the nearest-neighbor Abadie-Imbens (2006) matching estimator where we select a control firm with the same one-digit SIC code as the target firm based on the following alternative set of matching variables: industry median-adjusted Tobin's Q (lags 1 to 5, adjusted for the median Q in the firm's 4-digit SIC industry group), log market value (lag 1), leverage (lag 1), and ROA (lag 1). In the Alternative Matched Sample 2, we use the matching procedure based on firm's propensity score (rather than using the nearest-neighbor Abadie-Imbens matching estimator), including as covariates variables that we document matter for being targeted in our logit proportional hazard model (see section 3.2. above): Tobin's Q (lags 1 to 5), log Size (lag 1), leverage (lag 1), and ROA (lag 1). The reason we decided to use the Final Matched Sample over these alternative matching samples is because the Final Matched Sample minimizes the economic and statistical differences between the covariates of the treated and control groups before the hedge fund intervention (as shown in Table 4) and, therefore, yields a set of control firms whose characteristics are statistically closer to those of the targeted firms.

We use our Final Matched Sample throughout our following analysis. In our main tests, we regress the Tobin's Q on time dummies, the interaction of these time dummies with the *HF_Target* indicator (which equals one for firms targeted by a hedge fund, and zero for their matched control pairs), control variables, and various combinations of industry (4-digit SIC), firm, and year fixed effects.

The time dummies are defined as follows:

- "*t*" is an indicator equal to one for the fiscal year in which a firm is targeted by a hedge fund, and zero for every other year before or after the targeting event year. This indicator is also equal to one for the matched control firm;

- "*t-5*" is an indicator equal to one (for both target and control firms) five years before a firm is targeted by a hedge fund, and zero for every year before t-5 or after t-5;

- *“t-4”* to *“t+5”* are defined similarly to “t-5”;

- *“Post t+5”* is an indicator equal to one (for both target and control firms) in the years from t+6 onwards, and zero for every year before t+6.; and

- *“Post t+3”* is defined similarly to “Post t+5”.

- *“HF_Target* × *t”* is an indicator equal to one for firms targeted by a hedge fund in the year of the targeting event, and zero for every year before or after the targeting event year. *“HF_Target* × *t”* is always equal to zero for the matched-control pairs (i.e., firms not targeted by a hedge fund);

- *“HF_Target* × *t+1”* is an indicator equal to one for firms targeted by a hedge fund one year after the event year t, and zero for every year before t+1 or after t+1. *“HF_Target* × *t+1”* is always equal to zero for the matched-control pairs (firms not targeted by a hedge fund).

- *“HF_Target* × *t+2”* to “HF_Target × t+5” dummies are defined similarly to the “HF_Target × t+1” indicator.

- *“(t-4 to t-1)”* is an indicator equal to one (for both target and control firms) for the period from four years to one year before a firm is targeted by a hedge fund, and zero for every year before t-4 or after t-1.

- *“(t to t+3)”* is defined similarly to *“(t-4 to t-1)”*.

- *“HF_Target* × *t-4 to t-1”* is an indicator equal to one for firms targeted by a hedge fund for the period from four years to one year before the targeting event, and zero for every year before t-4 or after t-1. *“HF_Target* × *t-4 to t-1”* is always equal to zero for the matched-control pairs (firms not targeted by a hedge fund); and finally

- *“HF_Target* × *t to t+3”* and *“HF_Target* × *Post t+3”* are defined similarly to *“HF_Target* × *t-4 to t-1”*.

3.4. The Long-term Association between Firm Value and Becoming a Hedge Fund Target in a Matched Sample

In Table 5, we use our matched sample to reconsider the evidence in Bebchuk et al. (2015) that firms targeted by activist hedge funds tend to increase in value. The interactions of our event-time dummies (“t-5” through “t+5”) with “*HF_Target*” indicate whether the firm value of targeted firms is different from those of non-targeted firms, while the event-time dummies without the interaction consider whether there is a more general pattern in the firm value that is shared by both target and control firms.

The specification in column (1) of Table 5 includes 4-digit SIC industry fixed effects as well as year fixed effects, plus a set of firm characteristics as controls, while column (2) uses firm rather than industry fixed effects. The results indicate that once one incorporates firm heterogeneity, firms targeted by activist hedge funds seem to have very different characteristics than the population of publicly traded firms. This significantly changes the results from Table 2. In particular, the time dummies that are not interacted with "*HF_Target*" and thus capture changes in firm value that are common to target and control firms, exhibit the general pattern of increasing in value over time. For example, the coefficient on "*t*" in column (2) equals -0.134 (with a t-statistic of 2.46) and the coefficient on "*t+3*" equals -0.008 (with a t-statistic of 0.11), suggesting that both target and control firms significantly increase in firm value in the three years after the target firm is first targeted by an activist hedge fund. Results in column (1) with industry fixed effects are similar.

In contrast, the coefficients on the interactions of the event-time dummies with "*HF_Target*", which capture how the firm value of target firms differs from the firm value of control firms, indicate that the target firms tend to decrease in firm value after being targeted, relative to the firm value of the control firms. Note that the control firm sample is constructed—as shown in Table 4—to have a very similar firm value to the target firm sample in the year before the target firms are targeted. This allows a straightforward interpretation of the "*HF_Target*" interactions. For example, the coefficient of "*HF_Target* × t" in column (2) equals -0.140 (with a t-statistic of 2.67), and the coefficient of "*HF_Target* × t+5" equals -0.187 (with a t-statistic of 1.91). Economically, this means that the firm value of the target firms tend to be 7.95% (=-0.140 divided by the average Q in the target firm sample of 1.761) lower than the firm value of control firms at the end of the fiscal year in which the activist hedge funds start their campaign, and about 10.6% (=-0.187/1.761) lower five years thereafter.

These results do not appear to be driven by the particular matching procedure we selected. Indeed, when we use the three alternative matched samples based on the different matching procedures described in section 3.3 above, we obtain the same qualitative result, namely that target firms tend to increase less in value after being targeted, relative to the control firms. Using the Alternative Matched Sample 1, the statistical and economic results we obtain are on average stronger than the results we obtain using the Final Matched Sample (see Appendix Table A.1). Similarly, using the Alternative Matched Samples 2 and 3 (based on propensity score matching), the estimate coefficients always remain negative and statistically significant until the fourth year after the hedge fund intervention (see Appendix Table A.2, and A3, respectively).

Our findings suggest that the main result in Brav et al. (2015)—that firm value tends to go up after activist hedge funds commence their campaign—cannot be ascribed to the activist hedge fund campaign itself. The control firms, which are not targeted by an activist hedge fund, tend to increase in firm value around the same time, and controlling for that, the target firms that are targeted by activist hedge funds actually perform worse. These findings thus seem to suggest that sample selection drives the results in Bebchuk et al. (2015). Activist hedge funds tend to target firms that have been relatively poorly performing in the past one to five years, as we documented in Table 3. However, in generally competitive markets, many different actors can intervene to turn things around at a relatively poorly performing company, including management, directors, long-term shareholders, other stakeholders like large customers or suppliers, and also, naturally, activist hedge funds. The increases in firm value of the firms in our control sample suggest that mechanisms other than activist hedge funds have been on average *more* successful than the typical activist hedge fund campaign in turning these relatively poorly performing firms around.

Our results are also robust to verifying that the firm value of target and control firms is similar before the activist campaign. To this end, in columns (3) and (4) of Table 5, we first group together the four years leading up to the target event ("*t-4 to t-1*"), the four years following the target event ("*t to t+3*") as well as the period after that ("*Post t+3*"). The results in columns (3) and (4) are quite similar to the results in columns (1) and (2), respectively. Next, in columns (5) and (6), we add the interaction between "*HF_Target*" and "*t-4 to t-1*" in order to verify that target firms tend indeed to have a similar firm value to the control firms in the period leading up to the activist hedge fund campaign. This is a basic robustness check of our comparison between target and control firms in our matched sample as done in Table 4. We find that this interaction is statistically insignificant in both column (5) with industry fixed effects and in column (6) with firm fixed effects, indicating that our matching procedure has successfully matched target firms with similarly (typically poorly) performing control firms, while adding this interaction does not change any of the other results.

Panel A of Figure 1 plots the annual averages of the Tobin's Q for both the target and control firms in the five years before to five years after the target firm is targeted by an activist hedge fund. The figure further confirms that target and control firms have very similar firm values before the target firm is targeted during fiscal year t, that the firm value of the target firms declines substantially from the end of the fiscal year t-1 to the end of fiscal year t (during which year the firm is targeted), and then tends to increase in the three years after that in a similar way as the control firms.

Table 6 shows results analogous to those in Table 5, but then only including target firms and their controls for target firms involved in hostile activist hedge fund campaigns. Though these results confirm that the increase in firm value subsequent to the start of a (hostile) activist hedge fund campaign tends to be lower for the targeted firms than for the control firms, results for this sample are statistically weaker than for Table 5. If we include industry fixed effects, the differences are generally statistically insignificant. If we include firm fixed effects, the interaction between being targeted by in a hostile hedge fund campaign ("*HHF_Target*") and the three-year period after the event ("*t to t+3*") has a coefficient of -0.211 with a t-statistic of 2.35 in Column (4), though is statistically insignificant for the period thereafter. The results using hostile hedge fund campaigns using the alternative matching samples are reported in Appendix Tables A.4 and A.5, which again confirm that the control sample approach gives no evidence that targets have outperformed the controls.

Panel B of Figure 1 plots the annual averages of the Tobin's Q for both the target and control firms in the five years before to five years after the target firm is targeted by an activist hedge fund, but now only considering hostile campaigns. The figure shows that target and control firms have very similar firm values before the target firm is targeted during fiscal year t, that the firm value of the target firms remains relatively low during the year the firm is targeted and hardly changes thereafter, while the value of the control firms tend to increase in the three years after the targeting year.

3.4. Incorporating Delisting Prices

So far, we have exclusively used firm information at the end of the fiscal year, as is typical in the literature that uses annual Compustat information to analyze variation in Tobin's Q. However, using fiscal year-end information means not incorporating changes in firm value happening during years in which the firm delists from a stock exchange. This is especially relevant for firms targeted by activist hedge funds, which are fairly likely to delist in our sample. In particular, we find that these firms are frequently taken over. As a result, the generally large takeover premia received by target shareholders have not been incorporated in our analysis so far, which could potentially change our inference (see Greenwood and Schor, 2009). In this subsection, we incorporate the delisting price information into the Tobin's Q at the end of the fiscal year before the firm delists, and show that our main results are robust to doing so.

Appendix Table A.7 shows the number of firms delisting in our matched sample. In Panel A, we consider all hedge fund targets. Out of the 2,648 events (which constitute 2,009 unique firms), 397 target firms delist because they are taken over, 187 delist due to the firm violating a stock exchange requirement (e.g., the stock price fell below the exchange acceptable level, the firm has insufficient equity, or is delinquent in the payment of the listing fee), 13 firms delist but effectively retain securities that are traded is a different stock market that is not included in CRSP, and 1 firm delists due to liquidation. Compared to the 352 (227+125) firms in the control sample, target firms seem more likely to delist due to M&A and exchange requirement violations.

In Panel B, we consider the sample targeted in hostile hedge fund campaigns only, where the relative propensity of target firms to become a takeover target is even larger relative to the propensity of control firms. Specifically, 121 out of 313 target firms in this sample are taken over, as compared to only 64 out of 302 control firms. This is consistent with the evidence in Boyson, Gantchev and Shivdasani (2015) that activist campaigns make targeted firms more likely to be taken over.

In Appendix Tables A.8 and A.9, we adjust the Tobin's Q at the end of the firm's last fiscal year before delisting for the delisting price that is reported in CRSP. Appendix Table A.8 shows the results using the delisted-price adjusted Tobin's Q for the sample of all activist hedge fund campaigns, analogous to the results in Table 5. Appendix Table A.9 shows the results using the delisted-price adjusted Tobin's Q for the sample of only hostile activist hedge fund campaigns, analogous to the results in Table 6. In both cases, we find that our main result, namely that the value of firms targeted by activist hedge funds tends to decrease afterwards relative to control firms, is robust to incorporating the delisting price. For example in column (4) of Table A.8 with firm fixed effects, we find that the coefficient on the interaction "*HF_Target × t to t+3*" equals -0.116 (with a t-statistic of 2.21) and the coefficient on the interaction "*HF_Target × Post t+3*" equals -0.216 (with a t-statistic of 2.72), which are close to the analogous coefficients in column (4) of Table 5.

3.5. Adding time-varying industry fixed effects

Another important robustness check is to control for time-varying industry effects. For example, we do not match target and control firms by industry. Also, it is possible that activist hedge fund campaigns targeting a firm in a particular industry may have an effect on other firms in that industry, especially if the activist hedge fund is perceived as likely to target other firms operating in the same industry as prior firms it has targeted, as documented by Gantchev, Gredil and Jotiskasthira (2015).

We verify the robustness of our main result by adding 3-digit SIC industry fixed effects that change every year to our pooled panel specifications. Given that the 1,932 firms in our matched sample of all hedge fund targets come from 346 different industries and our sample consists of 17 years, that means adding about 5,882 annual industry dummies. The results with time-varying industry fixed effects for the matched sample of all hedge fund campaigns are reported in Appendix Table A.10, and for the sample of hostile hedge fund campaigns only in Appendix Table A.11. In both cases, we find that our results remain robust to adding these industry fixed effects.

3.6 Abnormal Stock Returns for Targeted and Control Firms

Brav et al. (2008a) and Bebchuk, Brav and Jiang (2015) also show that firms targeted by activist hedge funds exhibit positive abnormal stock returns in the period following the start of the activist campaign. In this section, we compare the abnormal stock returns of the targeted firms to those of their control firms. In our empirical design, we closely follow Bebchuk, Brav and Jiang (2015), and for example use the four-factor Fama-French-Carhart model to calculate abnormal returns (i.e., alphas). Our main extension is to consider the abnormal returns of the firms targeted by the hedge funds, but also the abnormal returns of their controls. The abnormal returns of the stocks of the control firms indicate whether the activist hedge funds on average have skills for picking stocks based on characteristics that indicate good future performance.

We first consider the target and control firm samples separately. In Table 7, we report the results for calendar-time portfolios that invest in stocks of the target or control firms in a particular window around the date that an activist hedge fund files its first 13D for a target firm. We consider three different periods: (i) the 36 months preceding the start of the activist hedge fund campaign, ending one month before the first 13D filing, [-36, -1], (ii) the 36 months following the start of the campaign, starting one month after the 13D filing by the activist hedge fund, [+1, +36], and (iii) the 60 months following the start of the campaign, [+1, +60]. For each period, we create equal- and value-weighted portfolios for all stocks that were target or control firms in that period. When we calculate abnormal returns, we follow Bebchuk, Brav and Jiang (2015) and employ weighted-least squares regressions, weighting the monthly excess portfolio returns by the number of stocks included in the portfolio. We further only consider the returns in calendar months where the portfolio includes at least 10 firms.

Panel A-1 of Table 7 replicates the results in Bebchuk, Brav and Jiang (2015), finding positive abnormal returns for targeted firms in the three and five years after the start of the activist hedge fund campaign using equal-weighted portfolios. We also find that the portfolio of target firms has a relatively low market beta, strongly positive exposure to the size and book-to-market factors and negative exposure to momentum. However, the abnormal returns of the control firms, reported in Panel A-2 of Table 7, are also positive, with a larger economic magnitude and with similar exposures to market risk, size, book-to-market and momentum. Panels B-1 and B-2 report the results for value-weighted portfolios for the target and control firms, respectively, where all alphas are insignificant, again similar to Bebchuk, Brav and Jiang (2015). This indicates that the positive abnormal returns were driven by the smaller targeted (and thus control) firms, possibly because small firms have a less efficient information environment.

In Table 8, we report the results for long-short calendar-time portfolios where we buy the portfolio of target firms and sell the portfolio of control firms, again in the three different time periods around the time the activist hedge funds file their first 13D for a stock. Using equal-weighted portfolios in Panel A, we find that the long-short portfolios have a negative and statistically significant four-factor alpha in three and five year period after the start of the activist hedge fund campaign. For example, over the 36 month period, [+1, +36], the long-short portfolio has a monthly four-factor alpha of -0.25% (t-statistic of 2.50), and over the 60 month period, [+1, +60], the alpha equals -0.20% per month (t-statistic of 2.41). Using value-weighted portfolios in Panel B, the abnormal returns are statistically insignificant result for both the 36 and the 60 month period after the start of the activist hedge fund campaign. As an aside, the long-short portfolio abnormal returns in Table 8 cannot be directly inferred from the abnormal returns of the separate long and short portfolios in Table 7, primarily because, in Table 8, the factor exposures are constrained to be identical across the long and short portfolios (as is typical in the literature when calculating abnormal returns for long-short portfolios).

The long-short portfolio results indicate that the stocks of the control firms outperformed the stocks of the target firms in the period around the start of the activist hedge fund campaigns. Therefore, while both targets and control firms on average had positive abnormal stock returns, investors would have been better off buying the stocks of the control firms rather than the stocks of the target firms. In turn, this suggests that while activist hedge funds appear to have stock picking skills on average, it seems unlikely that the positive abnormal stock returns can be attributed to the activist hedge funds themselves.

4. Activist Hedge Funds and the Limited Commitment Problem

In this section, we consider whether the association between becoming a target in an activist hedge fund campaign and long-term firm value is different for firms where it is more important that shareholders have a strong commitment to longer-term value creation and strong stakeholder relationships. A lack of such shareholder commitment may create what Cremers et al. (2015) label a 'limited commitment problem,' which arises out of the separation of shareholder ownership and managerial control in the context of asymmetric information and limited contracting or where stakeholder investments have a long-term, firm-specific nature. In these cases, the strong exit rights of shareholders, combined with asset pricing inefficiencies, may make shareholders unable to commit to the longer-term horizon. This is because upon observing a disappointing short-term firm outcome, shareholders will generally be unable to tell whether such an outcome is the result of managerial opportunism or an investment whose value will only materialize in the long term. As a result, fearing that managerial opportunism might be the source of such an outcome and in the attempt to protect their interests, shareholders may rationally ask the board to change top management and corporate policies, or decide to sell their shares in a takeover attempt or agree to change the board of directors in a proxy contest, all of which may lead to changes in the corporate strategy. Anticipating these circumstances, directors and managers may develop myopic incentives to appease shareholders (Stein, 1988, 1989; Karpoff and Rice, 1988, Bradenburger and Polak, 1996) and other stakeholders may become less willing (or demand higher compensation) to offer their cooperation for longer-term investments (Shleifer and Summers, 1988; Johnson et al., 2015). In both cases, the result is a reduction in long-term firm value.

Activist hedge funds are naturally more empowered than other shareholder to challenge the board of directors to change corporate policies or even corporate strategy, promoting the adoption of decisions to fire the existing management, increase leverage, reduce cash, or sell the firm to a prospective acquirer. All of such interventions—or even just their threat—may increase costs to incumbents, who risk losing their jobs, as well as to other stakeholders, especially those who are required to make longer-term, firm-specific investments in their relationships with the firm. This suggests that hedge fund activism may exacerbate a firms' limited commitment problem, with potentially detrimental effects on long-term firm value creation.

If our explanation about the possible transmission channel through which hedge fund activism is negatively associated to firm value is correct, we would expect to find that activist hedge fund campaigns—and *hostile* campaigns especially—are more negatively related to longer-term firm value for firms where the limited commitment problem is more relevant. In order to test this hypothesis, we will consider two different ways to identify such firms. We first focus on firms whose corporate strategy seems to intrinsically make the limited commitment problem more prominent, namely firms involved in longer-term research and development projects or firms with significant intangible assets that may be harder for outside shareholders to value. Second, we consider different proxies for firms where specific stakeholders have to make more firm-specific and long-term investments in their relationship with the firm.

4.1. Innovative Firms

To identify firms whose corporate strategies are likely to make the limited commitment problem particularly relevant, we employ three different proxies. First, we focus on firms that have high research and development expenses, as measured by a ratio of R&D expenses to sales that is above the 75th percentile in the sample (setting missing R&D expenses to zero). Second, we consider firms where intangible assets—such as goodwill, patents and trademarks—are relatively important, as measured by the ratio of book value of the firm's intangible assets over the book value of total assets being above the 75th percentile in the sample (setting missing intangible asset values to zero). Third and finally, we identify firms with significant patents directly using the NBER U.S. Patent Citations data file, focusing on firms with a patent citation count above the overall sample's 75th percentile.

In firm sharing the above characteristics it seems more likely that currently observable firm outcomes may not be fully informative about managerial performance (especially towards long-term value creation). This is because investments in R&D and intangible assets (including patents) naturally tend to be affected by a higher level of asymmetric information (Mizik and Jacobson, 2007, Edmans, 2011; Popadak, 2015). On the one hand, information about these investments is typically "soft" or non-verifiable. Moreover, these long-term investments tend to require large capital expenditures up-front, which is a kind of hard information that current market prices can more easily incorporate. As a result, shareholders are more likely to rationally interpret poor observed short-term outcomes that tend to accompany these investments as evidence of poor managerial performance (Eberhart, Maxwell, and Siddique, 2004).

In Table 9, we add the interaction of the above three limited commitment proxies to the specification in column (4) of Table 6, namely the specification with firm fixed effects that groups together event-times "*t-4 to t-1*," "*t to t+3*," and "*Post t+3*" dummies for the matched sample of hostile hedge fund campaigns. Our main interest is in the triple interaction of each limited commitment proxy, the dummy variable "*HHF_Target*" indicating that the firm was targeted by a hostile activist hedge fund, and finally the event-time dummies. We also include the double interactions of the limited commitment proxies and the event-time dummies to control for any time-variation in the valuation of these characteristics that changes over time similarly for target and control firms.

Consistent with our hypothesis, Table 9 documents that firms in which the limited commitment problem seems more relevant decrease more in value after hostile activist hedge fund campaigns than other targeted firms. The results are mixed and depend on the proxy used; while the economic signs on the triple interactions consistent confirm that the gap in firm value between targeted firms and controls firms is more negative for firms with more limited commitment, results are only statistically significant for half of the proxies we considered. For example, the triple interaction of the "*High* Intangibility" dummy with "*HHF_Target*" and "*t to t+3*" equals -0.552 with a t-statistic of 2.13, while the double interaction of "*HHF_Target*" with "*t to t+3*" equals -0.409 with a t-statistic of 2.46. These results suggest that firms with more intangible assets tend to decrease in value substantially more than other targeted firms (both relative to their respective control firms). Economically, the coefficients indicate that the group of firms with intangibility targeted in hostile activist hedge fund campaigns have declined in value by 31% relative to the control firms (=-0.552, divided by the average *Q* of all firms in this sample of 1.761) in the three years after first being targeted. However, the trips interactions for High R&D and High Patent Citation Count have negative coefficients but are statistically insignificant.

At the same time, we note that the triple interaction between the limited commitment proxies, "*HF_Target*" and "*t-4 to t-1*" are all statistically insignificant, except for High Intangibility. This indicates that our matching procedure has matched control firms with a similar value to the group of firms where the limited commitment problem is more severe, i.e., except there being a potential mismatch for firms with more intangible assets.

4.2. Firms with Important Stakeholder Relationships

In order to identify firms where specific stakeholders have to make more specific and longer-term investments in their relationship with the firm, we consider three different proxies, respectively capturing the importance of suppliers, employees, and unsecured borrowers. Our first proxy, *High Contract Specificity*, captures firms in industries where suppliers have to make more firm-specific investments in their relationship with the firms in that industry, as measured by *Contract Specificity*, which is the fraction of inputs in the industry that is not sold in an organized exchange (or reference priced in a trade publication). This variable comes from Nunn (2007), who makes this data available on his website. *High Contract Specificity* equals one for firms in industries where *Contract Specificity* is above its 75th percentile in the sample.

The second proxy is also at the industry level, focusing on firms in industries where the labor productivity is above the 75th percentile in the sample. Labor productivity data comes from the Bureau of Labor Statistics at the U.S. Department of Labor. The third and final proxy is at the firm-level rather than the industry-level, and captures firms with high unsecured borrowing, i.e., firms where the ratio of the book value of unsecured debt (COMPUSTAT's items *dlc+dltt-dm*) to the book value of total debt (*dlc+dltt*) and the ratio of total debt to assets are both above their sample medians (which is the case for about 25% of the sample, capturing firms that have relatively high debt, a relatively large fraction of which consists of unsecured loans).

As shown in Table 10 and similar to the case of more innovative firms, for each of these three proxies, we find that firm value tends to decrease more after an hostile activist hedge fund campaign for firms where stakeholder participation seems especially relevant relative to firms where stakeholder participation is not as important. For example, the coefficient on the triple interaction of "*High Labor Productivity*" with "*HHF_Target*" and "*t to t+3*" equals -0.394 with a t-statistic of 1.94, while the double interaction of "*HHF_Target*" and "*t to t+3*" has a coefficient of -0.692 with a t-statistic of 2.39. This suggests that the decline in firm value for firms where employees are relatively more productive is substantially larger in the years following a hostile hedge fund campaign, potentially because such campaign may disrupt the firm's relationship with these productive employees. For example, the most valuable employees will likely have the best outside options and may choose to leave the firm rather than experiencing a continued threat of further disruption to their work environment arising from the hostile activist hedge fund campaign.

5. Conclusion

This paper considers the role of hedge fund activism on firm value. Previous research has emphasized that hedge fund activism can increase firm value by more effectively monitoring corporate executives. As a matter of theory, however, hedge fund intervention may likewise exacerbate the limited commitment problem arising in publicly traded corporation, thereby undermining the ability of corporate managers to pursue value-increasing long-term investments and complicating (or making more costly) the cooperation of other stakeholders towards such long-term value creation.

To verify these conflicting theoretical hypotheses about the long-term association between hedge fund activism and firm value, we carefully match firms targeted by hedge fund activists to non-targeted control firms. Consistent with the limited commitment hypothesis, our findings reveals that in the years following the intervention of activist hedge funds, the firm value of hedge fund targets deteriorates (sizably) compared to control firms. These results are robust to accounting for the potentially higher premium that hedge fund targets receive in follow-up mergers and to incorporating time-varying industry fixed effects. Most importantly, we find the decrease in firm value for target firms (compared to control firms) to be particularly sizable for firms that are more likely to be affected by the limited commitment problems, such as firms that rely more intensively on R&D investments, intangible assets, and patents, or firms in industries characterized by high contract specificity, high labor productivity, and intensive use of unsecured debt.

Our paper contributes to the current academic and policy debate on the association between hedge fund intervention and firm value. Incorporating firm heterogeneity in a matching approach—and especially that firms targeted by activist hedge funds tend to have performed poorly in the period before they were targeted—we document a large decrease in firm value for target firms compared to control firms with a similarly poor ex-ante performance. Importantly, our study identifies the channel—namely the aggravation of the limited commitment problem—to help explain why firm value tends to decrease in the years after an activist hedge fund has started its campaign. Future research could consider additional channels through which hedge fund interventions can affect firm value, as well as investigate whether alternative governance solutions might be better suited at solving the trade-off between addressing both managerial moral hazard and the limited commitment problems that arise in the public corporation.

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Table 1 – Variable Definitions – Panel A

Table 1 provides a definition of the main variables used in the paper.

Dependent Variable:	
TobinQ	TobinQ is the ratio of market value of total assets (COMPUSTAT's items *at–ceq+prcc_f×csho*) to book value of assets (at)). Sample period 1995 – 2011.
Median Adjusted TobinQ	TobinQ is the industry-median adjusted Tobin's Q, calculated as the firm's Tobin's Q minus the 4-digit SIC-year median Tobin's Q (where Tobin's Q is the ratio of market value of total assets (COMPUSTAT's items *at–ceq+prcc_f×csho*) to book value of assets (at)). Sample period 1995 – 2011.
Control Variables:	
LnSize	LnSize is the natural logarithm of the book value of total assets (COMPUSTAT's item *at*). Sample period 1995 – 2011.
Leverage	Leverage is the ratio of the book value of total debt (COMPUSTAT's items *dltt + dlc*) to the book value of assets. Sample period 1995 – 2011.
CAPX	CAPX is the ratio of capital expenditures (COMPUSTAT's item *capx*) to the book value of total assets. Sample period 1995 – 2011.
R&D	R&D is the ratio of R&D expenses (COMPUSTAT's item *xrd*) to the book value of total assets. Sample period 1995 – 2011.
Intangibility	Intangibility is one minus the ratio of property, plants, & equipments (COMPUSTAT's item *ppent*) to the book value of total assets. Sample period 1995 – 2011.
ROA	ROA is the ratio of operating income before depreciation (COMPUSTAT's item *oibdp*) to the book value of total assets. Sample period 1995 – 2011.
Ln Market Value of Equity	LnSize is the natural logarithm of market value of equity (COMPUSTAT's items *prcc_f × csho*). Sample period 1995 – 2011.
Hedge Funds and Limited Commitment Variables	**Used in Matched-Sample Analysis Only: Table 4 and onward**
HF_Target	HF_Target is an indicator equal to 1 for firms targeted by an activist hedge fund during our sample. The hedge fund data is from Brav et al. (2008) (and subsequently updated by those authors) and cover the period 1995 – 2011.

HHF_Target	HHF_Target is an indicator equal to 1 for firms targeted by an activist hedge fund in a hostile campaign as coded by Brav et al. (2008) (and subsequently updated by those authors), covering the period 1995 – 2011.
High R&D	High R&D is an indicator equal to 1 if the ratio of R&D expenses (COMPUSTAT's item *xrd*) to total sales (*sale*) for the firm is above the overall sample 75th percentile on the year before the targeting event. Sample period 1995 – 2011.
High Intangibility	High Intangibility is an indicator equal to 1 if Intangibility (COMPUSTAT's items: *1 – ppent/at*) for the firm is above the overall sample 75th percentile on the year before the targeting event. Sample period 1995 – 2011.
High Patent Citation	High Patent Citation is an indicator equal to 1 for firms with a number of patent citation counts above the overall sample 75th percentile on the year before the targeting event. The patent citation count data is from the NBER U.S. Patent Citations data file. Sample period 1995 – 2010.
High Contract Specificity	High Contract Specificity is an indicator equal to 1 if the firm operates in an industry in which the percentage of inputs that are not sold in an organized exchange (or reference priced in a trade publication in the Nunn (2007) data file) is above the overall sample mean on the year before the targeting event. Data is available only for 1997.
High Labor Productivity	High Labor Productivity is an indicator equal to 1 if labor productivity (output per hour of labor) in the firm's industry is above the overall sample 75th percentile on the year before the targeting event. The labor productivity data are from Bureau of Labor Statistics (U.S. Department of Labor). Sample period 1995 – 2011.
High Unsecured Borrowing	High Unsecured Borrowing is an indicator equal to 1 for firms for which the ratio of unsecured debt (COMPUSTAT's items *dlc+dltt-dm*) to total debt (*dlc+dltt*) and the ratio of total debt to assets are both above their sample medians on the year before the targeting event. Sample period 1995 – 2011.

Table 1 - Descriptive Statistics – Panel B
This table reports descriptive statistics for the main variables used in the paper. The sample includes all non-financial firms from COMPUSTAT for the period 1995 – 2011. See Table 1, Panel A for detailed variable definitions.

Variables	Mean	St. Dev.	25^{th} PCTLE	75^{th} PCTLE	Obs.
Dependent Variable:					
TobinQ (5^{th}/95^{th} winsor)	2.664	2.664	1.130	2.805	91,466
Median Adjusted TobinQ (5^{th}/95^{th} winsor)	0.878	2.448	-0.365	0.860	91,466
Control Variables:					
LnSize	4.631	2.681	2.929	6.508	106,073
Leverage	0.369	0.741	0.021	0.414	105,689
CAPX	0.060	0.074	0.015	0.072	104,594
R&D	0.081	0.197	0.000	0.069	106,073
Intangibility	0.729	0.249	0.588	0.929	105,888
ROA	-0.235	1.284	-0.084	0.151	105,148
Ln Market Value of Equity	4.651	2.493	2.923	6.411	91,972

Table 1 - Descriptive Statistics – Panel C

This table reports descriptive statistics for the main variables used in the paper. The sample includes treated and control firms from COMPUSTAT for the period 1995 – 2011. See Table 1, Panel A for detailed variable definitions.

Variables	Mean	St. Dev.	25^{th} PCTLE	75^{th} PCTLE	Obs.
Dependent Variable:					
TobinQ (5^{th}/95^{th} winsor)	1.999	1.800	1.074	2.098	26,379
Median Adjusted TobinQ (5^{th}/95^{th} winsor)	0.303	1.607	-0.412	0.355	26,379
Control Variables:					
LnSize	5.274	2.047	3.886	6.698	27,225
Leverage	0.262	0.409	0.021	0.366	27,128
CAPX	0.055	0.063	0.017	0.067	26,956
R&D	0.061	0.151	0.000	0.055	27,225
Intangibility	0.735	0.232	0.618	0.917	27,188
ROA	0.007	0.599	0.028	0.159	27,135
Ln Market Value of Equity	5.146	2.055	3.733	6.626	26,403

Table 2 – Evolution of Tobin's Q over Time as in Table 4 of Bebchuk et al. (2015)

This table presents the coefficient estimates from OLS regressions where the dependent variable is Tobin's Q. The hedge fund data is from the (updated) dataset used in Brav. et al. (2008) and covers the period 1995 to 2011. The sample includes all non-financial firms from COMPUSTAT for the period 1995 – 2011. We follow Bebchuk et al. (2015) in the definition of variables and model specification. In particular, "*t: Event year*" is an indicator equal to one for firms targeted by a hedge fund in the year of the targeting event, and zero for every other year before or after the targeting event year. The *t*-dummy is always equal to zero for firms not targeted by a hedge fund. The other time dummies are defined similarly (see text for further details). See Table 1 for the description of all the variables. In all regressions, we include dummy variables representing the year of intervention as well as each of subsequent five years. In the table, *t*-statistics in brackets are based on robust standard errors clustered by firm.

	All Hedge Funds				Hostile Hedge Funds			
Dependent variable:	*(1)* *Tobin's Q*	*(2)* *Tobin's Q*	*(3)* *Tobin's Q*	*(4)* *Tobin's Q*	*(5)* *Tobin's Q*	*(6)* *Tobin's Q*	*(7)* *Tobin's Q*	*(8)* *Tobin's Q*
t: Event year	-0.361***		-0.155***		-0.617***		-0.323***	
	[-7.564]		[-3.063]		[-7.963]		[-4.029]	
t+1	-0.283***		-0.022		-0.425***		-0.073	
	[-5.887]		[-0.409]		[-4.828]		[-0.720]	
t+2	-0.254***		0.006		-0.348***		0.012	
	[-5.415]		[0.120]		[-3.996]		[0.114]	
t+3	-0.124**		0.128**		-0.228**		0.122	
	[-2.402]		[2.374]		[-2.184]		[1.053]	
(t to t+3)		-0.129**		0.052		-0.274***		-0.013
		[-2.475]		[0.973]		[-3.064]		[-0.141]
Post t+3		0.191***		0.265***		0.202***		0.411***
		[4.011]		[4.101]		[2.593]		[3.821]
t+4	-0.094*		0.164***		-0.129		0.225**	
	[-1.679]		[2.792]		[-1.174]		[2.018]	
t+5	-0.067		0.174***		-0.093		0.254**	
	[-1.053]		[2.800]		[-0.695]		[2.061]	
LnMV	0.228***	0.228***	0.733***	0.734***	0.228***	0.228***	0.733***	0.733***
	[38.133]	[38.185]	[63.870]	[63.877]	[38.138]	[38.143]	[63.862]	[63.866]
LnAge	-0.384***	-0.384***	-0.482***	-0.477***	-0.386***	-0.386***	-0.482***	-0.479***
	[-29.814]	[-29.823]	[-19.164]	[-18.938]	[-29.966]	[-29.949]	[-19.142]	[-19.007]
Year-Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SIC3-Fixed Effect	Yes	Yes			Yes	Yes		
Firm-Fixed Effect			Yes	Yes			Yes	Yes
Pre-event dummies	Yes	Yes	Yes	Yes	Yes		Yes	Yes
Obs.	131,163	131,163	131,163	131,163	131,163	131,163	131,163	131,163
R-2 (within)	0.077	0.077	0.170	0.170	0.077	0.077	0.170	0.170
F-Tests: [t+3] - t	17.03		25.11		11.09		13.55	
p - val	0.00%		0.00%		0.09%		0.02%	
[t+4] - t	16.58		23.01		21.80		26.13	
p - val	0.00%		0.00%		0.00%		0.00%	
[t+5] - t	16.44		20.44		12.92		16.46	
p - val	0.01%		0.00%		0.03%		0.00%	

Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Table 3 – The Ex-ante Probability of Becoming a Hedge Fund Target

This table presents the marginal effects estimates from logit (Panel A) and Cox proportional hazard model (Panel B) regressions. The hedge fund data is from the (updated) dataset used in Brav. et al. (2008) and covers the period 1995 to 2011. The sample includes all non-financial firms from COMPUSTAT for the period 1995 – 2011. In Panel A, the dependent variable is an indicator equal to 1 if the firm is targeted by a hedge fund in a given year, and zero otherwise. In Panel B, we categorize as "failure" an event year in which a firm is targeted by a hedge fund. To construct our sample, we use all firms that have not been targeted by a hedge fund in the past five years. If a firm is targeted by a hedge fund, we drop it from our sample. We allow the firm to re-enter the sample if it has not been targeted by a hedge fund for at least five years. See Table 1 for detailed variable definitions. In the table, *t*-statistics appear in brackets and are based on robust standard errors clustered by firm.

Panel A: Logit	**Hedge Fund Target Dummy**					
	(1)	(2)	(3)	(4)	(5)	(6)
TobinQ at t=-5	-0.002***					-0.000
	[-5.585]					[-0.487]
TobinQ at t=-4		-0.002***				-0.000
		[-6.953]				[-0.355]
TobinQ at t=-3			-0.003***			-0.001*
			[-8.363]			[-1.779]
TobinQ at t=-2				-0.003***		-0.001
				[-8.203]		[-1.088]
TobinQ at t=-1					-0.004***	-0.003***
					[-9.109]	[-4.109]
LnSize at t=-1	0.001***	0.001***	0.001***	0.001***	0.001***	0.000**
	[3.188]	[3.339]	[3.137]	[3.751]	[4.247]	[2.111]
Leverage at t=-1	-0.001	-0.000	0.000	0.000	0.001	0.002
	[-0.983]	[-0.020]	[0.124]	[0.073]	[1.013]	[0.918]
CAPX at t=-1	0.006	0.013	0.017*	0.017**	0.013*	0.020*
	[0.564]	[1.244]	[1.776]	[1.980]	[1.658]	[1.868]
R&D at t=-1	0.008**	0.007*	0.011***	0.011***	0.015***	0.017***
	[2.184]	[1.880]	[3.249]	[3.597]	[4.814]	[4.197]
Intangibility at t=-1	0.006**	0.006**	0.006**	0.007***	0.007***	0.010***
	[2.034]	[2.199]	[2.221]	[2.824]	[2.993]	[3.143]
ROA at t=-1	0.003**	0.002*	0.002*	0.002	0.001	0.001
	[1.962]	[1.821]	[1.896]	[1.413]	[0.706]	[0.374]
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	61,527	66,387	71,698	77,461	83,789	59,904
Pseudo-R2	0.032	0.031	0.034	0.036	0.041	0.038

Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Panel B: Cox model	**Failure Event: Hedge Fund Target Dummy**					
	(1)	(2)	(3)	(4)	(5)	(6)
TobinQ at t=-5	-0.148***					-0.045**
	[-4.339]					[-2.042]
TobinQ at t=-4		-0.169***				-0.009
		[-4.572]				[-0.316]
TobinQ at t=-3			-0.187***			-0.032
			[-4.745]			[-1.031]
TobinQ at t=-2				-0.233***		-0.018
				[-4.715]		[-0.519]
TobinQ at t=-1					-0.296***	-0.219***
					[-4.933]	[-3.712]
LnSize at t=-1	0.044**	0.047**	0.043**	0.041**	0.032**	0.028*
	[2.240]	[2.389]	[2.250]	[2.172]	[1.981]	[1.806]
Leverage at t=-1	-0.124	-0.059	-0.042	-0.011	0.113	0.060
	[-1.336]	[-0.624]	[-0.430]	[-0.102]	[1.016]	[0.556]
CAPX at t=-1	-0.230	-0.288	0.098	0.179	0.036	0.631
	[-0.335]	[-0.445]	[0.147]	[0.271]	[0.065]	[0.917]
R&D at t=-1	0.596**	0.643**	0.765***	0.888***	0.957***	0.962***
	[2.272]	[2.416]	[2.803]	[3.071]	[3.415]	[3.190]
Intangibility at t=-1	0.173	0.192	0.256	0.354	0.323	0.362
	[0.766]	[0.872]	[1.110]	[1.431]	[1.503]	[1.519]
ROA at t=-1	0.248*	0.277**	0.303**	0.288*	0.190	0.074
	[1.889]	[1.965]	[2.032]	[1.832]	[1.295]	[0.634]
Obs.	38,764	41,063	42,725	43,617	44,427	38,065
Wald-chi2	111.43***	125.57***	114.97***	103.61***	103.45***	111.21***

Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Table 4 – Hedge Funds Targets and Matched-Control Firms

This table reports mean difference tests of firm characteristics for firms targeted by hedge funds and control firms (Panel A) and for firms targeted by hostile hedge funds and control firms (Panel B). The hedge fund data is from the (updated) dataset used in Brav. et al. (2008) and covers the period 1995 to 2011. The sample includes non-financial firms from COMPUSTAT for the period 1995 – 2011. Control firms are a subsample of the non-targeted firms selected as the closest match based on Tobin's Q (lag 1 and 5), natural logarithm of market value of equity (lag 1), leverage (lag 1), ROA (lag 1), and exact matching on fiscal year and 1 digit SIC code . We use the Abadie-Imbens matching estimator to identify the control firms (Abadie and Imbens, 2006). Refer to Table 1 for detailed variable definitions.

Panel A: All Hedge Funds	**Target**	**Control**	**Difference**	**Difference *t*-test *p*-value**
TobinQ at t-1	1.761	1.752	0.009	0.886
TobinQ at t-2	1.829	1.831	-0.001	0.984
TobinQ at t-3	1.854	1.828	0.026	0.681
TobinQ at t-4	1.956	1.973	-0.017	0.811
TobinQ at t-5	2.017	1.989	0.028	0.698
Log Market Value at t-1	5.094	5.092	0.002	0.978
Leverage at t-1	0.253	0.239	0.013	0.295
ROA at t-1	0.017	0.019	-0.002	0.931
Log Size at t-1	5.439	5.330	0.109	0.175
CAPX at t-1	0.051	0.048	0.002	0.322
R&D at t-1	0.060	0.053	0.007	0.222
Intangibility at t-1	0.744	0.734	0.009	0.337
Panel B: Hostile Hedge Funds	**Treated**	**Control**	**Difference**	**Difference *t*-test *p*-value**
TobinQ at t-1	1.532	1.551	-0.019	0.800
TobinQ at t-2	1.632	1.642	-0.010	0.903
TobinQ at t-3	1.733	1.758	-0.025	0.818
TobinQ at t-4	1.868	1.908	-0.040	0.751
TobinQ at t-5	1.994	1.984	0.010	0.939
Log Market Value at t-1	5.311	5.315	-0.004	0.978
Leverage at t-1	0.231	0.217	0.013	0.529
ROA at t-1	0.072	0.077	-0.005	0.622
Log Size at t-1	5.736	5.553	0.183	0.173
CAPX at t-1	0.053	0.048	0.005	0.325
R&D at t-1	0.050	0.044	0.006	0.424
Intangibility at t-1	0.743	0.748	-0.006	0.768

Table 5 – Hedge Funds and Firm Value: All Hedge Funds

This table presents the coefficient estimates from OLS regressions. The dependent variable is *Tobin Q*. The hedge fund data is from the (updated) dataset used in Brav et al. (2008) and covers the period 1995 to 2011. Firm-level data are from COMPUSTAT for the period 1995 – 2011.The sample includes firms targeted by hedge funds and control firms (identified using the Abadie-Imbens matching estimator described in Table 4). "*t*" is an indicator equal to one for the year in which a firm is targeted by a hedge fund, and zero for every other year before or after the targeting event year. This indicator is also equal to one for the matched control firm. "*HF_Target* × *t*" is an indicator equal to one for firms targeted by a hedge fund in the year of the targeting event, and zero for every year before or after the targeting event year. "*HF_Target* × *t*" is always equal to zero for the matched-control pairs (firms not targeted by a hedge fund). The other time dummies are defined similarly (see text for further details). We restrict the sample to non-financial firms. Refer to Table 1 for detailed variable definitions. In the table, *t*-statistics appear in brackets and are based on robust standard errors clustered by firm.

Dep. Var.: *TobinQ*	(1)	(2)	(3)	(4)	(5)	(6)
t-5	-0.197***	-0.082**				
	[-5.274]	[-2.416]				
t-4	-0.208***	-0.063				
	[-5.254]	[-1.598]				
t-3	-0.323***	-0.156***				
	[-8.344]	[-3.792]				
t-2	-0.344***	-0.158***				
	[-8.639]	[-3.617]				
t-1	-0.408***	-0.219***				
	[-9.438]	[-4.571]				
t-4 to t-1			-0.317***	-0.164***	-0.294***	-0.148***
			[-8.937]	[-4.937]	[-7.591]	[-3.772]
t	-0.333***	-0.134**				
	[-7.088]	[-2.462]				
t+1	-0.302***	-0.087				
	[-5.840]	[-1.444]				
t+2	-0.260***	-0.039				
	[-4.737]	[-0.583]				
t+3	-0.223***	-0.008				
	[-3.597]	[-0.106]				
t to t+3			-0.300***	-0.111**	-0.304***	-0.107**
			[-6.019]	[-2.117]	[-6.109]	[-2.003]
Post t+3			-0.080	0.128	-0.083	0.131
			[-0.966]	[1.583]	[-0.994]	[1.630]
t+4	-0.174**	0.040				
	[-2.490]	[0.475]				
t+5	-0.124	0.130				
	[-1.330]	[1.231]				
Post t+5	-0.024	0.282**				
	[-0.241]	[2.514]				
HF_Target × t-4 to t-1					-0.052	-0.039
					[-1.130]	[-0.660]

HF_Target × t	-0.161***	-0.140***				
	[-2.900]	[-2.666]				
HF_Target × t+1	-0.147**	-0.138**				
	[-2.436]	[-2.312]				
HF_Target × t+2	-0.081	-0.087				
	[-1.216]	[-1.277]				
HF_Target × t+3	-0.141**	-0.124*				
	[-2.104]	[-1.754]				
HF_Target × t to t+3			-0.096*	-0.115**	-0.095*	-0.132**
			[-1.885]	[-2.199]	[-1.881]	[-1.979]
HF_Target × Post t+3			-0.172**	-0.216***	-0.170**	-0.235***
			[-2.185]	[-2.746]	[-2.171]	[-2.703]
HF_Target × t+4	-0.188**	-0.191**				
	[-2.505]	[-2.489]				
HF_Target × t+5	-0.197**	-0.187*				
	[-1.965]	[-1.909]				
HF_Target × Post t+5	-0.183*	-0.242**				
	[-1.806]	[-2.341]				
LnSize	-0.136***	-0.486***	-0.137***	-0.490***	-0.137***	-0.491***
	[-7.519]	[-14.185]	[-7.524]	[-14.265]	[-7.521]	[-14.269]
Leverage	0.894***	0.571***	0.896***	0.573***	0.897***	0.573***
	[11.539]	[8.114]	[11.573]	[8.152]	[11.567]	[8.147]
CAPX	4.045***	3.277***	4.066***	3.309***	4.069***	3.311***
	[12.008]	[10.112]	[12.052]	[10.175]	[12.061]	[10.184]
R&D	3.534***	1.606***	3.530***	1.594***	3.533***	1.595***
	[13.457]	[5.930]	[13.440]	[5.885]	[13.453]	[5.889]
Intangibility	1.150***	1.364***	1.151***	1.373***	1.153***	1.374***
	[8.163]	[6.256]	[8.146]	[6.292]	[8.155]	[6.292]
4-digit SIC Industry-FE	Yes	No	Yes	No	Yes	No
Firm-FE	No	Yes	No	Yes	No	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	25,795	25,795	25,795	25,795	25,795	25,795
R-2	0.228	0.180	0.226	0.178	0.226	0.178

Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Table 6 – Hedge Funds and Firm Value: Hostile Hedge Funds

This table presents the coefficient estimates from OLS regressions. The dependent variable is *TobinQ*. The hedge fund data is from the (updated) dataset used in Brav et al. (2008) and covers the period 1995 to 2011. Firm-level data are from COMPUSTAT for the period 1995 – 2011. The sample includes firms targeted by hostile hedge funds and control firms (identified using the Abadie-Imbens matching estimator described in Table 4). "*t*" is an indicator equal to one for the year in which a firm is targeted by a hedge fund, and zero for every other year before or after the targeting event year. This indicator is also equal to one for the matched control firm. "*HHF_Target* × *t*" is an indicator equal to one for firms targeted by a hostile hedge fund in the year of the targeting event, and zero for every year before or after the targeting event year. "*HHF_Target* × *t*" is always equal to zero for the matched-control pairs (firms not targeted by a hostile hedge fund). The other time dummies are defined similarly (see text for further details). We restrict the sample to non-financial firms. Refer to Table 1 for detailed variable definitions. In the table, *t*-statistics appear in brackets and are based on robust standard errors clustered by firm.

Dep. Var.: *TobinQ*	(1)	(2)	(3)	(4)	(5)	(6)
t-5	-0.121	-0.052				
	[-1.641]	[-0.747]				
t-4	-0.153*	-0.070				
	[-1.919]	[-0.832]				
t-3	-0.291***	-0.177*				
	[-3.368]	[-1.912]				
t-2	-0.414***	-0.275***				
	[-4.918]	[-2.727]				
t-1	-0.520***	-0.376***				
	[-5.530]	[-3.302]				
t-4 to t-1			-0.303***	-0.216***	-0.265***	-0.157*
			[-4.306]	[-3.168]	[-3.187]	[-1.812]
t	-0.391***	-0.205				
	[-3.368]	[-1.478]				
t+1	-0.323***	-0.107				
	[-2.605]	[-0.726]				
t+2	-0.416***	-0.155				
	[-3.466]	[-0.944]				
t+3	-0.231*	-0.002				
	[-1.790]	[-0.011]				
t to t+3			-0.301***	-0.124	-0.301***	-0.102
			[-2.853]	[-1.090]	[-2.852]	[-0.852]
Post t+3			0.037	0.188	0.036	0.211
			[0.222]	[1.130]	[0.218]	[1.232]
t+4	-0.261*	0.006				
	[-1.933]	[0.029]				
t+5	-0.132	0.117				
	[-0.780]	[0.504]				
Post t+5	0.166	0.342				
	[0.693]	[1.352]				
HHF_Target × t-4 to t-1					-0.082	-0.127
					[-1.025]	[-1.269]

HHF_Target × t	-0.171**	-0.239***				
	[-2.079]	[-2.648]				
HHF_Target × t+1	-0.120	-0.246**				
	[-1.345]	[-2.423]				
HHF_Target × t+2	0.079	-0.124				
	[0.895]	[-1.171]				
HHF_Target × t+3	-0.083	-0.242*				
	[-0.716]	[-1.796]				
HHF_Target × t to t+3			-0.071	-0.211**	-0.079	-0.269**
			[-0.980]	[-2.353]	[-1.032]	[-2.236]
HHF_Target × Post t+3			-0.131	-0.170	-0.138	-0.234
			[-0.838]	[-1.107]	[-0.874]	[-1.355]
HHF_Target × t+4	-0.005	-0.196				
	[-0.043]	[-1.420]				
HHF_Target × t+5	-0.164	-0.245				
	[-0.981]	[-1.271]				
HHF_Target × Post t+5	-0.221	-0.121				
	[-0.897]	[-0.593]				
LnSize	-0.114***	-0.457***	-0.114***	-0.457***	-0.113***	-0.460***
	[-3.492]	[-7.376]	[-3.477]	[-7.348]	[-3.455]	[-7.377]
Leverage	0.752***	0.631***	0.749***	0.632***	0.751***	0.632***
	[4.762]	[3.840]	[4.725]	[3.859]	[4.740]	[3.869]
CAPX	3.434***	3.295***	3.434***	3.297***	3.444***	3.305***
	[6.181]	[5.800]	[6.134]	[5.769]	[6.149]	[5.791]
R&D	2.623***	0.648	2.636***	0.649	2.634***	0.645
	[5.651]	[1.260]	[5.691]	[1.253]	[5.698]	[1.248]
Intangibility	0.971***	1.333***	0.972***	1.329***	0.976***	1.324***
	[4.449]	[3.296]	[4.460]	[3.286]	[4.460]	[3.282]
4-digit SIC Industry-FE	Yes	No	Yes	No	Yes	No
Firm-FE	No	Yes	No	Yes	No	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	8,265	8,265	8,265	8,265	8,265	8,265
R-2	0.141	0.170	0.135	0.164	0.135	0.164

Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Table 7 – Abnormal Returns from Calendar Time Portfolio Regressions for Firms Targeted by Hedge Funds and Control Firms

This table reports abnormal returns from calendar time portfolio regressions (equal weighted in Panel A and value weighted in Panel B) for firms targeted by hedge funds and their controls. Regression coefficients are estimated using weighted least squares regression and the number of firm-event in a given month as weights. We restrict our analysis to the calendar months with at least 10 target firms. See text for further details on the methodology. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Panel A: Equal-weighted Four-Factor Model

Panel A-1: Target Firms

Window	Alpha	Beta	SMB	HML	MOM	N	R-squared
[-36, -1]	-0.186	0.933***	0.857***	0.293***	-0.240***	200	85.84%
	(-0.99)	(17.80)	(12.07)	(3.86)	(-4.37)		
[+1, +36]	0.479**	0.867***	0.905***	0.250***	-0.206***	229	83.83%
	(2.34)	(15.00)	(15.73)	(2.96)	(-3.03)		
[+1,+60]	0.374**	0.925***	0.825***	0.281***	-0.222***	233	87.11%
	(2.14)	(17.30)	(15.50)	(3.77)	(-4.31)		

Panel A-2: Control Firms

Window	Alpha	Beta	SMB	HML	MOM	N	R-squared
[-36, -1]	-0.038	1.078***	1.012***	0.417***	-0.197***	200	91.88%
	(-0.19)	(18.77)	(12.44)	(5.20)	(-4.38)		
[+1, +36]	0.513***	0.959***	0.800***	0.328***	-0.214***	229	87.84%
	(3.09)	(19.07)	(11.46)	(4.53)	(-5.24)		
[+1,+60]	0.504***	0.945***	0.783***	0.291***	-0.215***	233	89.64%
	(3.22)	(19.43)	(12.73)	(3.92)	(-5.30)		

Panel B: Value-weighted Four-Factor Model

Panel B-1: Target Firms

Window	Alpha	Beta	SMB	HML	MOM	N	R-squared
[-36, -1]	-0.873***	0.950***	0.445***	0.217***	-0.138***	200	81.51%
	(-5.32)	(18.79)	(5.49)	(2.83)	(-3.85)		
[+1, +36]	-0.053	1.047***	0.507***	0.046	-0.046	229	84.81%
	(-0.21)	(12.80)	(7.14)	(0.39)	(-1.24)		
[+1, +60]	0.192	1.189***	0.360***	-0.262*	-0.042	233	91.21%
	(0.51)	(12.66)	(2.83)	(-1.79)	(-0.97)		

Panel B-2: Control Firms

Window	Alpha	Beta	SMB	HML	MOM	N	R-squared
[-36, -1]	-0.275	1.018***	0.401***	0.208**	-0.060	200	86.82%
	(-1.61)	(18.67)	(5.07)	(2.30)	(-1.65)		
[+1, +36]	0.110	1.000***	0.279***	-0.087	-0.006	229	85.39%
	(0.73)	(26.47)	(5.12)	(-1.31)	(-0.17)		
[+1, +60]	0.187	0.985***	0.207***	-0.082	0.019	233	91.29%
	(1.42)	(30.42)	(3.59)	(-1.43)	(0.76)		

Table 8 – Abnormal Returns from Calendar Time Portfolio Regressions for Long-Short Stock Portfolios Being Long Target Firms and Short Control Firms

This table reports abnormal returns from calendar time portfolio regressions (equal-weighted and value-weighted using the four factor model) for a portfolio consisting of being long stocks of firms targeted by hedge funds and short stocks of control firms over the period 1995 – 2011. Regression coefficients are estimated using weighted least squares regression and the number of firm-event in a given month as weights. We restrict our analysis to the calendar months with at least 10 target firms. See text for further details on the methodology. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Panel A: Long Target and Short Control: All Hedge Funds

Panel A-1: Equal-weight Four Factor Model

Window	Alpha	Beta	SMB	HML	MOM	N	R-squared
[-36, -1]	-0.03	0.13	0.03	0.37***	-0.31***	200	67.40%
	(-0.12)	(1.31)	(0.55)	(3.70)	(-4.59)		
[+1, +36]	-0.25**	-0.02	0.05	-0.04	-0.02	229	6.20%
	(-2.50)	(-0.89)	(1.54)	(-0.86)	(-1.22)		
[+1,+60]	-0.20**	0.03	0.04	-0.04*	-0.03*	233	30.34%
	(-2.41)	(1.45)	(1.01)	(-1.69)	(-1.96)		

Panel A-2: Value-weight Four Factor Model

Window	Alpha	Beta	SMB	HML	MOM	N	R-squared
[-36, -1]	-1.04***	1.95***	0.89***	0.47***	-0.21***	200	87.83%
	(-4.00)	(23.04)	(6.75)	(3.58)	(-3.58)		
[+1, +36]	0.11	1.98***	0.85***	0.07	-0.05	229	88.97%
	(0.35)	(23.38)	(10.37)	(0.49)	(-0.89)		
[+1,+60]	0.13	2.00***	0.60***	0.01	-0.03	233	91.53%
	(0.42)	(20.31)	(6.29)	(0.07)	(-0.60)		

Panel A-1: Long Target and Short Control: All Hedge Funds, *Equal-weighted* Four-Factor Model

Window	Alpha	Beta	SMB	HML	MOM	N	R-squared
[-36, -1]	-0.03	0.13	0.03	0.37***	-0.31***	200	67.40%
	(-0.12)	(1.31)	(0.55)	(3.70)	(-4.59)		
[+1, +36]	-0.25**	-0.02	0.05	-0.04	-0.02	229	6.20%
	(-2.50)	(-0.89)	(1.54)	(-0.86)	(-1.22)		
[+1,+60]	-0.20**	0.03	0.04	-0.04*	-0.03*	233	30.34%
	(-2.41)	(1.45)	(1.01)	(-1.69)	(-1.96)		

Panel A-2: Long Target and Short Control: All Hedge Funds, *Value-weighted* Four-Factor Model

Window	Alpha	Beta	SMB	HML	MOM	N	R-squared
[-36, -1]	-1.04***	1.95***	0.89***	0.47***	-0.21***	200	87.83%
	(-4.00)	(23.04)	(6.75)	(3.58)	(-3.58)		
[+1, +36]	0.11	1.98***	0.85***	0.07	-0.05	229	88.97%
	(0.35)	(23.38)	(10.37)	(0.49)	(-0.89)		
[+1,+60]	0.13	2.00***	0.60***	0.01	-0.03	233	91.53%
	(0.42)	(20.31)	(6.29)	(0.07)	(-0.60)		

Table 9 – The Limited Commitment Channel in Innovative Firms

This table presents the coefficient estimates from OLS regressions. The dependent variable is *TobinQ*. The hedge fund data is from the (updated) dataset used in Brav et al. (2008) and covers the period 1995 to 2011. Firm-level data are from COMPUSTAT for the period 1995 – 2011. The sample includes firms targeted by hostile hedge funds and control firms (identified using the Abadie-Imbens matching estimator described in Table 4). "*LimitedCommitment*" is an indicator for firms facing limited commitment problems. We use three limited commitment proxies, "*High R&D,*" "*High Intangibility,*" and "*High Patent Citation*". The patent citation count data is from the NBER U.S. Patent Citations data file. "*HHF_Target*" is an indicator equal to 1 for firms targeted by a hostile hedge fund. "*(t to t+3)*" is an indicator for the years t (the year in which a firms is targeted by a hostile hedge fund) to t+3. "*(t-4 to t-1)*" is an indicator for the years from four years prior to one year prior the firm is targeted by a hostile hedge fund. We restrict the sample to non-financial firms. Refer to Table 1 for detailed variable definitions. In the table, *t*-statistics appear in brackets and are based on robust standard errors clustered by firm.

Limited Commitment Proxy:	**High R&D (1)**	**High Intangibility (2)**	**High Patent Citation (3)**
LimitedCommitment × HHF_Target × (t to t+3)	-0.170	-0.552**	-0.381
	[-0.405]	[-2.134]	[-0.657]
LimitedCommitment × HHF_Target × (t-4 to t-1)	-0.504	-0.430*	-0.283
	[-1.371]	[-1.873]	[-0.489]
LimitedCommitment × (t-4 to t-1)	-0.381	-0.241	-0.275
	[-1.489]	[-1.605]	[-0.535]
LimitedCommitment × (t to t+3)	-1.127***	-0.409**	-0.241
	[-3.750]	[-2.456]	[-0.480]
HHF_Target × (t to t+3)	-0.169*	-0.137	-0.171*
	[-1.877]	[-1.383]	[-1.757]
(t to t+3)	-0.077	-0.080	-0.160**
	[-1.176]	[-1.095]	[-2.315]
HHF_Target × (t-4 to t-1)	-0.012	-0.006	-0.049
	[-0.145]	[-0.065]	[-0.568]
(t-4 to t-1)	-0.157**	-0.141*	-0.192***
	[-2.392]	[-1.951]	[-2.966]
LnSize	-0.456***	-0.462***	-0.455***
	[-7.439]	[-7.528]	[-7.353]
Leverage	0.660***	0.634***	0.640***
	[4.099]	[3.957]	[3.878]
CAPX	3.259***	3.392***	3.255***
	[5.712]	[6.058]	[5.746]
R&D	0.722	0.610	0.644
	[1.397]	[1.194]	[1.267]
Intangibility	1.338***	1.392***	1.347***
	[3.293]	[3.555]	[3.315]
Firm-FE	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes
Obs.	8,265	8,265	8,265
R-2	0.180	0.174	0.166

Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Table 10 – The Limited Commitment Channel in Firms with Important Stakeholders

This table presents the coefficient estimates from OLS regressions. The dependent variable is *TobinQ*. The hedge fund data is from the (updated) dataset used in Brav et al. (2008) and covers the period 1995 to 2011. Firm-level data are from COMPUSTAT for the period 1995. The sample includes firms targeted by hostile hedge funds and control firms (identified using the Abadie-Imbens matching estimator described in Table 4). *StakeholderProxy* is an indicator for firms with important stakeholders. We use three proxies for important stakeholders: "*High Contract Specificity*," "*High Labor Productivity*," and "*High Unsecured Borrowing*". "*HHF_Target*" is an indicator equal to 1 for firms targeted by a hostile hedge fund. "*(t to t+3)*" is an indicator for the years t (the year in which a firms is targeted by a hostile hedge fund) to t+3. "*(t-4 to t-1)*" is an indicator for the years from four years prior to one year prior the firm is targeted by a hostile hedge fund. We restrict the sample to non-financial firms. Refer to Table 1 for detailed variable definitions. In the table, *t*-statistics appear in brackets and are based on robust standard errors clustered by firm.

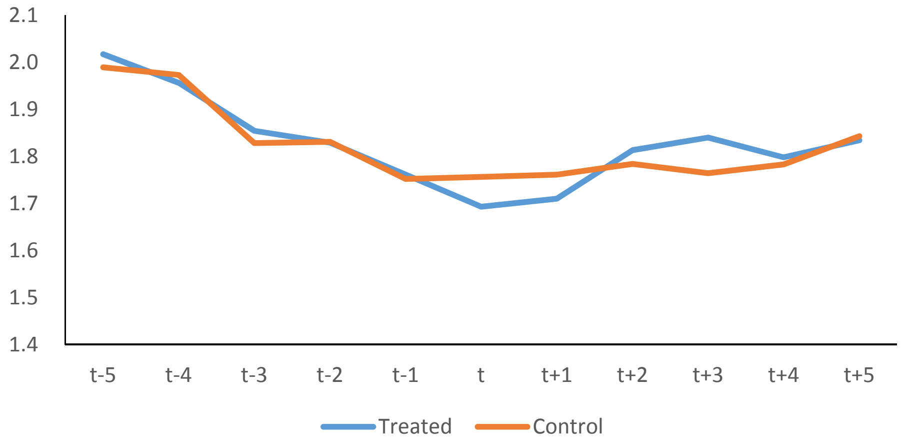
Important Stakeholder Proxy:	**High Contract Specificity (1)**	**High Labor Productivity (2)**	**High Unsecured Borrowing (3)**
StakeholderProxy × HHF_Target × (t to t+3)	-0.279	-0.394*	-0.692**
	[-0.884]	[-1.939]	[-2.391]
StakeholderProxy × HHF_Target × (t-4 to t-1)	-0.148	-0.313*	-0.704**
	[-0.397]	[-1.692]	[-2.520]
StakeholderProxy × (t-4 to t-1)	-0.349	0.315**	0.448***
	[-1.265]	[2.461]	[2.799]
StakeholderProxy × (t to t+3)	-0.277	0.517***	0.370***
	[-1.199]	[4.257]	[2.917]
HHF_Target × (t to t+3)	-0.069	-0.089	-0.121
	[-0.308]	[-0.795]	[-1.156]
(t to t+3)	0.017	-0.295***	-0.228***
	[0.085]	[-3.725]	[-2.921]
HHF_Target × (t-4 to t-1)	0.037	0.031	0.013
	[0.129]	[0.293]	[0.140]
(t-4 to t-1)	-0.042	-0.317***	-0.270***
	[-0.174]	[-3.924]	[-3.828]
LnSize	-0.351*	-0.414***	-0.458***
	[-1.911]	[-6.686]	[-7.408]
Leverage	-0.620*	0.474**	0.634***
	[-1.700]	[2.181]	[3.865]
CAPX	5.851***	3.323***	3.286***
	[3.230]	[5.667]	[5.754]
R&D	1.887	0.625	0.658
	[1.215]	[1.188]	[1.305]
Intangibility	2.598**	1.392***	1.329***
	[2.430]	[3.302]	[3.287]
Firm-FE	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes
Obs.	2,095	7,924	8,265
R-2	0.170	0.154	0.166

Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

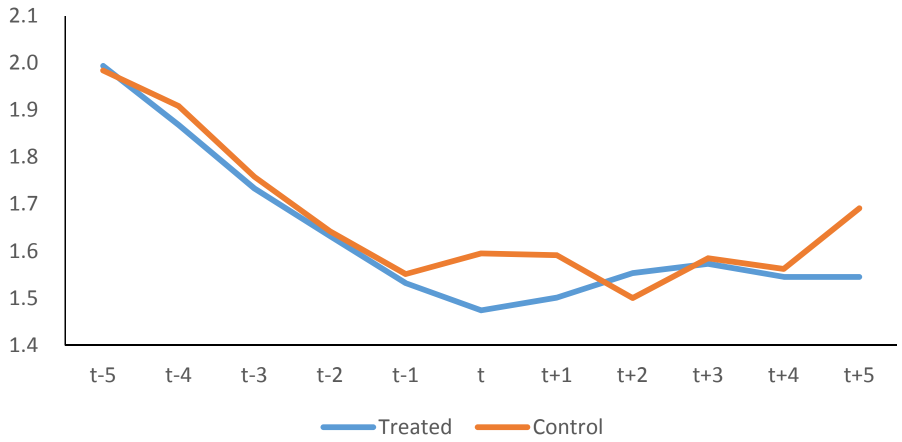
Figure 1 – Tobin's Q for Hedge Funds Targets and their Matched-Control Firms in the Period from 5 Years before the Targeting Event to 5 Years After

This figure reports Tobin's Q for firms targeted by hedge funds (Panel A), firms targeted by hostile hedge funds (Panel B), and their matched control firms (identified using the Abadie-Imbens matching estimator described in Table 4) in the period from t-5 to t+5 (where t is the targeting year). The hedge fund data is from the (updated) dataset used in Brav. et al. (2008) and covers the period 1995 to 2011. Firm-level data are from COMPUSTAT for the period 1995. Refer to Table 1 for detailed variable definitions.

Panel A: Tobin's Q around the start of activist hedge fund campaigns (sample of all hedge funds campaigns)



Panel B: Tobin's Q around the start of hostile activist hedge fund campaigns (sample of hostile hedge fund campaigns only)



APPENDIX TO

HEDGE FUNDS AND LONG-TERM FIRM VALUE

Table A.1 – Hedge Funds and Firm Value for All Hedge Funds (Alternative Matched Sample 1: NNMATCH using different matching variables)

This table presents the coefficient estimates from OLS regressions. The dependent variable is *MedianAdjTobinQ*, the industry median-adjusted Tobin's Q. The hedge fund data is from the (updated) dataset used in Brav et al. (2008) and covers the period 1995 to 2011. Firm-level data are from COMPUSTAT for the period 1995 – 2011. The sample includes firms targeted by hedge funds and control firms (identified using the Abadie-Imbens matching estimator with the following matching variables MedianAdjTobin's Q (lag 1 and 5), log of market value (lag 1), leverage (lag 1), ROA (lag 1) and exact matching on fiscal year and 1 digit SIC code. "*t*" is an indicator equal to one for the year in which a firm is targeted by a hedge fund, and zero for every other year before or after the targeting event year. This indicator is also equal to one for the matched control firm. "*HF_Target* × *t*" is an indicator equal to one for firms targeted by a hedge fund in the year of the targeting event, and zero for every year before or after the targeting event year. "*HF_Target* × *t*" is always equal to zero for the matched-control pairs (firms not targeted by a hedge fund). The other time dummies are defined similarly (see text for further details). We restrict the sample to non-financial firms. Refer to Table 1 for detailed variable definitions. In the table, *t*-statistics appear in brackets and are based on robust standard errors clustered by firm.

Dep.Var.: MedianAdjTobinQ	(1)	(2)	(3)	(4)	(5)	(6)
t-5	-0.148***	-0.077**				
	[-4.282]	[-2.266]				
t-4	-0.173***	-0.083**				
	[-4.713]	[-2.052]				
t-3	-0.239***	-0.133***				
	[-6.658]	[-3.173]				
t-2	-0.293***	-0.180***				
	[-7.838]	[-3.937]				
t-1	-0.358***	-0.246***				
	[-8.586]	[-4.889]				
t-4 to t-1			-0.272***	-0.147***	-0.251***	-0.102**
			[-7.945]	[-4.216]	[-6.593]	[-2.524]
t	-0.272***	-0.157***				
	[-5.814]	[-2.712]				
t+1	-0.312***	-0.192***				
	[-6.316]	[-3.088]				
t+2	-0.252***	-0.137*				
	[-4.618]	[-1.938]				
t+3	-0.193***	-0.081				
	[-3.227]	[-1.045]				
t to t+3			-0.269***	-0.119**	-0.273***	-0.108**
			[-5.485]	[-2.242]	[-5.578]	[-2.008]
Post t+3			-0.007	0.141*	-0.009	0.150*
			[-0.081]	[1.707]	[-0.116]	[1.821]
t+4	-0.065	0.057				
	[-0.860]	[0.606]				
t+5	-0.041	0.102				
	[-0.497]	[1.047]				

Post t+5	0.005	0.114				
	[0.054]	[1.007]				
HF_Target × t-4 to t-1					-0.048	-0.109*
					[-1.081]	[-1.935]
HF_Target × t	-0.139***	-0.144***				
	[-2.601]	[-2.738]				
HF_Target × t+1	-0.077	-0.086				
	[-1.365]	[-1.519]				
HF_Target × t+2	-0.019	-0.035				
	[-0.296]	[-0.523]				
HF_Target × t+3	-0.133**	-0.137**				
	[-2.121]	[-1.991]				
HF_Target × t to t+3			-0.065	-0.096*	-0.065	-0.145**
			[-1.360]	[-1.883]	[-1.352]	[-2.258]
HF_Target × Post t+3			-0.180**	-0.224***	-0.179**	-0.276***
			[-2.271]	[-2.752]	[-2.257]	[-3.117]
HF_Target × t+4	-0.218***	-0.264***				
	[-2.717]	[-3.069]				
HF_Target × t+5	-0.206**	-0.234**				
	[-2.259]	[-2.522]				
HF_Target × Post t+5	-0.156	-0.192*				
	[-1.514]	[-1.820]				
LnSize	-0.143***	-0.447***	-0.144***	-0.448***	-0.144***	-0.449***
	[-7.951]	[-13.140]	[-7.988]	[-13.223]	[-7.983]	[-13.260]
Leverage	0.845***	0.559***	0.845***	0.561***	0.846***	0.561***
	[12.207]	[7.738]	[12.221]	[7.796]	[12.218]	[7.795]
CAPX	3.731***	2.829***	3.745***	2.848***	3.748***	2.855***
	[11.983]	[9.799]	[12.049]	[9.840]	[12.062]	[9.879]
R&D	3.593***	1.760***	3.591***	1.757***	3.595***	1.757***
	[13.370]	[6.726]	[13.368]	[6.718]	[13.381]	[6.720]
Intangibility	1.137***	1.140***	1.138***	1.147***	1.140***	1.148***
	[8.899]	[5.539]	[8.916]	[5.563]	[8.926]	[5.568]
4-digit SIC Industry-FE	Yes	No	Yes	No	Yes	No
Firm-FE	No	Yes	No	Yes	No	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	25,212	25,212	25,212	25,212	25,212	25,212
R-2	0.234	0.161	0.233	0.159	0.233	0.160

Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Table A.2 – Hedge Funds and Firm Value for All Hedge Funds (Alternative Matched Sample 2: PSMATCH using different matching variables)

This table presents the coefficient estimates from OLS regressions. The dependent variable is *TobinQ*. The hedge fund data is from the (updated) dataset used in Brav et al. (2008) and covers the period 1995 to 2011. Firm-level data are from COMPUSTAT for the period 1995 – 2011. The sample includes firms targeted by hedge funds and control firms (identified using propensity score matching with the following matching variables: *TobinQ* (lag 1 and 5), log of market value (lag 1), leverage (lag 1), ROA (lag 1), and exact matching on fiscal year and 1 digit SIC code. "*t*" is an indicator equal to one for the year in which a firm is targeted by a hedge fund, and zero for every other year before or after the targeting event year. This indicator is also equal to one for the matched control firm. "*HF_Target* × *t*" is an indicator equal to one for firms targeted by a hedge fund in the year of the targeting event, and zero for every year before or after the targeting event year. "*HF_Target* × *t*" is always equal to zero for the matched-control pairs (firms not targeted by a hedge fund). The other time dummies are defined similarly (see text for further details). We restrict the sample to non-financial firms. Refer to Table 1 for detailed variable definitions. In the table, *t*-statistics appear in brackets and are based on robust standard errors clustered by firm.

Dep. Var.: TobinQ	(1)	(2)	(3)	(4)	(5)	(6)
t-5	-0.004	-0.079				
	[-0.117]	[-0.985]				
t-4	-0.018	-0.084				
	[-0.611]	[-0.924]				
t-3	-0.101***	-0.149**				
	[-3.923]	[-2.235]				
t-2	-0.143***	-0.190**				
	[-5.525]	[-2.104]				
t-1	-0.180***	-0.239**				
	[-5.652]	[-2.252]				
t-4 to t-1			-0.183***	-0.150***	0.111	0.025
			[-4.813]	[-2.623]	[1.621]	[0.105]
t	0.195***	0.018				
	[4.652]	[0.110]				
t+1	0.020	-0.073				
	[0.519]	[-0.791]				
t+2	-0.210***	-0.311*				
	[-5.890]	[-1.769]				
t+3	-0.132***	-0.217				
	[-3.614]	[-1.238]				
t to t+3			0.062	-0.093	-0.046	-0.097
			[0.792]	[-0.406]	[-0.698]	[-0.425]
Post t+3			-0.133*	-0.302	-0.167**	-0.274
			[-1.846]	[-1.260]	[-2.194]	[-1.008]
t+4	0.104**	0.016				
	[2.478]	[0.152]				
t+5	0.050	-0.028				
	[1.169]	[-0.266]				
Post t+5	-0.080	-0.343				
	[-0.765]	[-1.329]				

HF_Target × t-4 to t-1					-0.420***	-0.247
					[-4.906]	[-0.819]
HF_Target × t	-0.534***	-0.379***				
	[-7.638]	[-3.147]				
HF_Target × t+1	-0.294***	-0.244***				
	[-4.207]	[-2.588]				
HF_Target × t+2	0.047	0.081				
	[0.685]	[0.549]				
HF_Target × t+3	-0.039	-0.022				
	[-0.565]	[-0.145]				
HF_Target × t to t+3			-0.342***	-0.147	-0.298***	-0.202
			[-3.702]	[-0.556]	[-3.543]	[-0.615]
HF_Target × Post t+3			0.034	0.200	-0.002	0.096
			[0.351]	[0.590]	[-0.015]	[0.214]
HF_Target × t+4	-0.263***	-0.273**				
	[-3.611]	[-2.472]				
HF_Target × t+5	-0.161**	-0.149				
	[-1.962]	[-1.388]				
HF_Target × Post t+5	0.079	0.226				
	[0.609]	[0.797]				
LnSize	-0.086***	-0.406***	-0.088***	-0.403***	-0.093***	-0.406***
	[-4.739]	[-7.411]	[-4.824]	[-7.026]	[-5.072]	[-6.836]
Leverage	0.700***	0.442***	0.697***	0.433***	0.695***	0.433***
	[5.621]	[3.405]	[5.598]	[3.134]	[5.621]	[3.104]
CAPX	2.561***	1.131	2.653***	1.337	2.693***	1.418
	[8.281]	[1.043]	[8.672]	[1.292]	[8.849]	[1.483]
R&D	3.956***	1.932***	3.975***	1.977***	3.975***	1.976***
	[11.531]	[3.958]	[11.607]	[3.836]	[11.734]	[3.791]
Intangibility	0.797***	0.773***	0.838***	0.809***	0.883***	0.814***
	[5.409]	[2.669]	[5.643]	[2.716]	[5.881]	[2.747]
4-digit SIC Industry-FE	Yes	No	Yes	No	Yes	No
Firm-FE	No	Yes	No	Yes	No	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	18,352	18,352	18,352	18,352	18,352	18,352
R-2	0.204	0.156	0.201	0.148	0.205	0.150

Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Table A.4 – Hedge Funds and Firm Value for Hostile Hedge Funds (Alternative Matched Sample 1: NNMATCH using different matching variables)

This table presents the coefficient estimates from OLS regressions. The dependent variable is *MeidanAdjTobinQ*, the industry median-adjusted Tobin's Q. The hedge fund data is from the (updated) dataset used in Brav et al. (2008) and covers the period 1995 to 2011. Firm-level data are from COMPUSTAT for the period 1995 – 2011. The sample includes firms targeted by hedge funds and control firms (identified using the Abadie-Imbens matching estimator with the following matching variables MedianAdjTobin's Q (lag 1 and 5), log of market value (lag 1), leverage (lag 1), ROA (lag 1) and exact matching on fiscal year and 1 digit SIC code. "*t*" is an indicator equal to one for the year in which a firm is targeted by a hostile hedge fund, and zero for every other year before or after the targeting event year. This indicator is also equal to one for the matched control firm. "*HHF_Target* × *t*" is an indicator equal to one for firms targeted by a hostile hedge fund in the year of the targeting event, and zero for every year before or after the targeting event year. "*HHF_Target* × *t*" is always equal to zero for the matched-control pairs (firms not targeted by a hostile hedge fund). The other time dummies are defined similarly (see text for further details). We restrict the sample to non-financial firms. Refer to Table 1 for detailed variable definitions. In the table, *t*-statistics appear in brackets and are based on robust standard errors clustered by firm.

Dep. Var.: MedianAdjTobinQ	(1)	(2)	(3)	(4)	(5)	(6)
t-5	-0.079	-0.002				
	[-1.091]	[-0.034]				
t-4	-0.189***	-0.089				
	[-2.706]	[-1.197]				
t-3	-0.266***	-0.139*				
	[-3.514]	[-1.690]				
t-2	-0.351***	-0.205**				
	[-4.658]	[-2.209]				
t-1	-0.452***	-0.289***				
	[-5.393]	[-2.693]				
t-4 to t-1			-0.310***	-0.235***	-0.307***	-0.159**
			[-5.084]	[-3.774]	[-4.299]	[-2.103]
t	-0.334***	-0.089				
	[-3.245]	[-0.695]				
t+1	-0.357***	-0.091				
	[-3.520]	[-0.673]				
t+2	-0.340***	-0.049				
	[-2.972]	[-0.314]				
t+3	-0.094	0.187				
	[-0.668]	[1.027]				
t to t+3			-0.288***	-0.103	-0.288***	-0.076
			[-3.047]	[-0.991]	[-3.040]	[-0.705]
Post t+3			0.159	0.335**	0.159	0.362**
			[0.864]	[2.184]	[0.864]	[2.320]
t+4	-0.027	0.342*				
	[-0.171]	[1.751]				
t+5	0.030	0.431**				
	[0.166]	[2.002]				

Post t+5	0.336	0.552**				
	[1.277]	[2.324]				
HHF_Target × t-4 to t-1					-0.006	-0.167*
					[-0.086]	[-1.820]
HHF_Target × t	-0.138*	-0.275***				
	[-1.840]	[-3.168]				
HHF_Target × t+1	-0.051	-0.225**				
	[-0.655]	[-2.331]				
HHF_Target × t+2	0.057	-0.165				
	[0.675]	[-1.625]				
HHF_Target × t+3	-0.152	-0.352***				
	[-1.280]	[-2.633]				
HHF_Target × t to t+3			-0.063	-0.258***	-0.064	-0.334***
			[-0.920]	[-2.984]	[-0.890]	[-2.941]
HHF_Target × Post t+3			-0.271	-0.399***	-0.272	-0.483***
			[-1.609]	[-2.830]	[-1.599]	[-3.113]
HHF_Target × t+4	-0.221*	-0.485***				
	[-1.747]	[-3.415]				
HHF_Target × t+5	-0.273*	-0.503***				
	[-1.723]	[-2.963]				
HHF_Target × Post t+5	-0.348	-0.281				
	[-1.351]	[-1.477]				
LnSize	-0.108***	-0.362***	-0.107***	-0.362***	-0.107***	-0.364***
	[-3.375]	[-5.980]	[-3.350]	[-5.992]	[-3.347]	[-6.044]
Leverage	0.571***	0.414**	0.568***	0.414**	0.569***	0.414**
	[3.713]	[2.352]	[3.754]	[2.389]	[3.757]	[2.396]
CAPX	2.992***	2.126***	2.991***	2.118***	2.993***	2.149***
	[6.278]	[4.699]	[6.236]	[4.664]	[6.233]	[4.747]
R&D	3.070***	1.250***	3.054***	1.257***	3.054***	1.254***
	[7.354]	[2.765]	[7.342]	[2.767]	[7.345]	[2.764]
Intangibility	1.147***	1.131***	1.151***	1.133***	1.152***	1.147***
	[5.194]	[2.961]	[5.217]	[2.958]	[5.189]	[3.002]
4-digit SIC Industry-FE	Yes	No	Yes	No	Yes	No
Firm-FE	No	Yes	No	Yes	No	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	8,119	8,119	8,119	8,119	8,119	8,119
R-2	0.128	0.126	0.123	0.121	0.123	0.122

Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Table A.5 – Hedge Funds and Firm Value for Hostile Hedge Funds (Alternative Matched Sample 2: PSMATCH using different matching variables)

This table presents the coefficient estimates from OLS regressions. The dependent variable is *TobinQ*. The hedge fund data is from the (updated) dataset used in Brav et al. (2008) and covers the period 1995 to 2011. Firm-level data are from COMPUSTAT for the period 1995 – 2011. The sample includes firms targeted by hedge funds and control firms (identified using propensity score matching with the following matching variables: TobinQ (lag 1 and 5), log of market value (lag 1), leverage (lag 1), ROA (lag 1), and exact matching on fiscal year and 1 digit SIC code . "*t*" is an indicator equal to one for the year in which a firm is targeted by a hostile hedge fund, and zero for every other year before or after the targeting event year. This indicator is also equal to one for the matched control firm. "*HHF_Target* × *t*" is an indicator equal to one for firms targeted by a hostile hedge fund in the year of the targeting event, and zero for every year before or after the targeting event year. "*HHF_Target* × *t*" is always equal to zero for the matched-control pairs (firms not targeted by a hostile hedge fund). The other time dummies are defined similarly (see text for further details). We restrict the sample to non-financial firms. Refer to Table 1 for detailed variable definitions. In the table, *t*-statistics appear in brackets and are based on robust standard errors clustered by firm.

Dep. Var.: TobinQ	(1)	(2)	(3)	(4)	(5)	(6)
t-5	0.058	-0.007				
	[0.908]	[-0.089]				
t-4	0.043	-0.023				
	[0.835]	[-0.322]				
t-3	-0.075*	-0.122*				
	[-1.805]	[-1.805]				
t-2	-0.136***	-0.188**				
	[-3.260]	[-2.333]				
t-1	-0.167***	-0.216***				
	[-3.334]	[-3.013]				
t-4 to t-1			-0.202***	-0.219***	0.007	-0.071
			[-3.193]	[-3.228]	[0.083]	[-0.379]
t	0.075	-0.038				
	[1.394]	[-0.242]				
t+1	-0.017	-0.118				
	[-0.335]	[-0.779]				
t+2	0.052	-0.031				
	[1.090]	[-0.408]				
t+3	-0.069	-0.153				
	[-1.353]	[-1.139]				
t to t+3			-0.035	-0.216	-0.110	-0.226
			[-0.376]	[-0.816]	[-1.312]	[-0.889]
Post t+3			-0.051	-0.237	-0.084	-0.231
			[-0.454]	[-0.710]	[-0.750]	[-0.670]
t+4	0.038	-0.087				
	[0.583]	[-0.410]				
t+5	-0.104	-0.162				
	[-1.564]	[-1.004]				
Post t+5	0.019	0.010				
	[0.162]	[0.049]				

HF_Target × t-4 to t-1					- [illegible]	-0.215
					[-3.113]	[-0.847]
HF_Target × t	-0.396***	-0.301*				
	[-4.291]	[-1.822]				
HF_Target × t+1	-0.167*	-0.120				
	[-1.656]	[-0.691]				
HF_Target × t+2	-0.103	-0.108				
	[-1.116]	[-0.915]				
HF_Target × t+3	0.047	0.060				
	[0.449]	[0.385]				
HF_Target × t to t+3			-0.228**	-0.110	-0.221**	-0.152
			[-2.094]	[-0.367]	[-2.099]	[-0.445]
HF_Target × Post t+3			0.111	0.287	0.059	0.208
			[0.660]	[0.761]	[0.349]	[0.452]
HF_Target × t+4	-0.013	0.064				
	[-0.115]	[0.290]				
HF_Target × t+5	0.147	0.234				
	[1.054]	[1.155]				
HF_Target × Post t+5	0.290	0.426				
	[1.335]	[1.477]				
LnSize	-0.051	-0.326***	-0.051	-0.322***	-0.056	-0.325***
	[-1.441]	[-3.552]	[-1.451]	[-3.495]	[-1.586]	[-3.477]
Leverage	0.390	0.202	0.399*	0.199	0.402*	0.205
	[1.614]	[0.728]	[1.661]	[0.716]	[1.703]	[0.726]
CAPX	1.886***	0.206	1.936***	0.364	1.983***	0.449
	[3.759]	[0.178]	[3.918]	[0.328]	[4.083]	[0.427]
R&D	3.661***	1.186	3.641***	1.246	3.640***	1.237
	[4.935]	[1.265]	[4.898]	[1.296]	[4.909]	[1.279]
Intangibility	0.552**	0.319	0.570**	0.352	0.597**	0.346
	[2.147]	[0.698]	[2.241]	[0.764]	[2.359]	[0.755]
4-digit SIC Industry-FE	Yes	No	Yes	No	Yes	No
Firm-FE	No	Yes	No	Yes	No	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	5,535	5,535	5,535	5,535	5,535	5,535
R-2	0.133	0.152	0.131	0.145	0.133	0.146

Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Table A.7 – Delisting Reasons: Hedge Fund Targets and Matched-Control Firms

This table contains information on delisting reasons in the years after a firm has been targeted by a hedge fund, for either target or control firms. The delisting reasons are identified using the delisting code from CRSP. The hedge fund data is from the (updated) dataset used in Brav. et al. (2008) and covers the period 1995 to 2011.

Panel A: All Hedge Funds	Target: All Hedge Funds	Control	Combined
Delisting Reason:			
M&A	397	231	628
Security Exchanged for Security Trading in other Market	2	4	6
Firm's Liquidation	1	3	4
Delisted from NYSE, NYSE MKT, NASDAQ or Arca:			
Security Started Trading in Different Exchange	11	13	24
Security in Violation of Stock Exchange Requirement	187	111	298
Total	**598**	**362**	**960**

Panel B: Hostile Hedge Funds	Target: Hostile Hedge Funds	Control	Combined
Delisting Reason:			
M&A	121	77	198
Security Exchanged for Security Trading in other Market	1	0	1
Firm's Liquidation	0	1	1
Delisted from NYSE, NYSE MKT, NASDAQ or Arca:			
Security Started Trading in Different Exchange	2	4	6
Security in Violation of Stock Exchange Requirement	39	30	69
Total	**163**	**112**	**275**

Table A.8 – Controlling for Delisting Price (All Hedge Funds)

This table presents the coefficient estimates from OLS regressions. The dependent variable is *Tobin Q*. The hedge fund data is from the (updated) dataset used in Brav et al. (2008) and covers the period 1995 to 2011. Firm-level data are from COMPUSTAT for the period 1995 – 2011. The sample includes firms targeted by hedge funds and control firms (identified using the Abadie-Imbens matching estimator described in Table 4). In this table, if a target firm or its matched-control firm delist from the stock exchange after the event year, we estimate Tobin's Q for the delisting year using the delisting price from CRSP and accounting information from COMPUSTAT (previous fiscal year). "*t*" is an indicator equal to one for the year in which a firm is targeted by a hedge fund, and zero for every other year before or after the targeting event year. This indicator is also equal to one for the matched control firm. "*HF_Target* × *t*" is an indicator equal to one for firms targeted by a hedge fund in the year of the targeting event, and zero for every year before or after the targeting event year. "*HF_Target* × *t*" is always equal to zero for the matched-control pairs (firms not targeted by a hedge fund). The other time dummies are defined similarly (see text for further details). We restrict the sample to non-financial firms. Refer to Table 1 for detailed variable definitions. In the table, *t*-statistics appear in brackets and are based on robust standard errors clustered by firm.

Dep. Var.: *TobinQ*	(1)	(2)	(3)	(4)	(5)	(6)
t-5	-0.194***	-0.077**				
	[-5.209]	[-2.260]				
t-4	-0.204***	-0.055				
	[-5.142]	[-1.382]				
t-3	-0.320***	-0.148***				
	[-8.268]	[-3.589]				
t-2	-0.353***	-0.160***				
	[-8.778]	[-3.640]				
t-1	-0.415***	-0.216***				
	[-9.612]	[-4.488]				
t-4 to t-1			-0.323***	-0.162***	-0.298***	-0.145***
			[-9.082]	[-4.863]	[-7.680]	[-3.700]
t	-0.324***	-0.118**				
	[-6.829]	[-2.162]				
t+1	-0.303***	-0.081				
	[-5.830]	[-1.329]				
t+2	-0.267***	-0.039				
	[-4.809]	[-0.575]				
t+3	-0.229***	-0.007				
	[-3.673]	[-0.085]				
t to t+3			-0.304***	-0.107**	-0.308***	-0.102*
			[-6.070]	[-2.017]	[-6.164]	[-1.903]
Post t+3			-0.090	0.129	-0.093	0.133
			[-1.087]	[1.589]	[-1.118]	[1.637]
t+4	-0.176**	0.046				
	[-2.490]	[0.546]				
t+5	-0.126	0.138				
	[-1.342]	[1.306]				
Post t+5	-0.037	0.280**				
	[-0.373]	[2.500]				

HF_Target × t-4 to t-1					-0.056 [-1.225]	-0.040 [-0.680]
HF_Target × t	-0.179*** [-3.204]	-0.151*** [-2.804]				
HF_Target × t+1	-0.150** [-2.450]	-0.135** [-2.228]				
HF_Target × t+2	-0.080 [-1.194]	-0.079 [-1.154]				
HF_Target × t+3	-0.154** [-2.283]	-0.131* [-1.825]				
HF_Target × t to t+3			-0.103** [-2.019]	-0.116** [-2.211]	-0.102** [-2.015]	-0.134** [-1.999]
HF_Target × Post t+3			-0.172** [-2.187]	-0.216*** [-2.722]	-0.170** [-2.171]	-0.235*** [-2.688]
HF_Target × t+4	-0.199*** [-2.597]	-0.198** [-2.532]				
HF_Target × t+5	-0.219** [-2.184]	-0.204** [-2.078]				
HF_Target × Post t+5	-0.171* [-1.692]	-0.229** [-2.213]				
LnSize	-0.134*** [-7.435]	-0.485*** [-14.125]	-0.134*** [-7.440]	-0.488*** [-14.200]	-0.134*** [-7.436]	-0.489*** [-14.203]
Leverage	0.902*** [11.741]	0.590*** [8.593]	0.904*** [11.777]	0.592*** [8.635]	0.904*** [11.772]	0.591*** [8.630]
CAPX	4.018*** [11.907]	3.228*** [9.924]	4.039*** [11.951]	3.258*** [9.987]	4.043*** [11.961]	3.261*** [9.996]
R&D	3.399*** [12.916]	1.498*** [5.432]	3.395*** [12.902]	1.485*** [5.390]	3.398*** [12.918]	1.486*** [5.394]
Intangibility	1.141*** [8.067]	1.364*** [6.231]	1.141*** [8.048]	1.373*** [6.262]	1.144*** [8.059]	1.373*** [6.262]
4-digit SIC Industry-FE	Yes	No	Yes	No	Yes	No
Firm-FE	No	Yes	No	Yes	No	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	25,797	25,797	25,797	25,797	25,797	25,797
R-2	0.220	0.175	0.219	0.173	0.219	0.173

Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Table A.9 – Controlling for Delisting Price (Hostile Hedge Funds)

This table presents the coefficient estimates from OLS regressions. The dependent variable is *TobinQ*. The hedge fund data is from the (updated) dataset used in Brav et al. (2008) and covers the period 1995 to 2011. Firm-level data are from COMPUSTAT for the period 1995 – 2011. The sample includes firms targeted by hostile hedge funds and control firms (identified using the Abadie-Imbens matching estimator described in Table 4). In this table, if a target firm or its matched-control firm delist from the stock exchange after the event year, we estimate Tobin's Q for the delisting year using the delisting price from CRSP and accounting information from COMPUSTAT (previous fiscal year). "*t*" is an indicator equal to one for the year in which a firm is targeted by a hedge fund, and zero for every other year before or after the targeting event year. This indicator is also equal to one for the matched control firm. "*HHF_Target × t*" is an indicator equal to one for firms targeted by a hostile hedge fund in the year of the targeting event, and zero for every year before or after the targeting event year. "*HHF_Target × t*" is always equal to zero for the matched-control pairs (firms not targeted by a hostile hedge fund). The other time dummies are defined similarly (see text for further details). Refer to Table 1 for detailed variable definitions. In the table, *t*-statistics appear in brackets and are based on robust standard errors clustered by firm.

Dep. Var.: *TobinQ*	(1)	(2)	(3)	(4)	(5)	(6)
t-5	-0.122*	-0.053				
	[-1.664]	[-0.751]				
t-4	-0.155*	-0.070				
	[-1.944]	[-0.835]				
t-3	-0.296***	-0.179*				
	[-3.422]	[-1.939]				
t-2	-0.421***	-0.277***				
	[-5.000]	[-2.749]				
t-1	-0.529***	-0.380***				
	[-5.634]	[-3.339]				
t-4 to t-1			-0.308***	-0.217***	-0.269***	-0.158*
			[-4.383]	[-3.188]	[-3.240]	[-1.827]
t	-0.388***	-0.198				
	[-3.332]	[-1.425]				
t+1	-0.312**	-0.089				
	[-2.473]	[-0.601]				
t+2	-0.418***	-0.149				
	[-3.463]	[-0.909]				
t+3	-0.237*	-0.001				
	[-1.843]	[-0.007]				
t to t+3			-0.298***	-0.114	-0.297***	-0.092
			[-2.819]	[-0.998]	[-2.818]	[-0.765]
Post t+3			0.031	0.192	0.030	0.215
			[0.186]	[1.153]	[0.181]	[1.255]
t+4	-0.266**	0.007				
	[-1.972]	[0.037]				
t+5	-0.143	0.113				
	[-0.847]	[0.487]				
Post t+5	0.156	0.341				
	[0.650]	[1.351]				

HHF_Target × t-4 to t-1					-0.085	-0.127
					[-1.065]	[-1.271]
HHF_Target × t	-0.149*	-0.212**				
	[-1.797]	[-2.334]				
HHF_Target × t+1	-0.120	-0.240**				
	[-1.279]	[-2.260]				
HHF_Target × t+2	0.089	-0.105				
	[0.983]	[-0.983]				
HHF_Target × t+3	-0.110	-0.257*				
	[-0.980]	[-1.948]				
HHF_Target × t to t+3			-0.066	-0.198**	-0.075	-0.256**
			[-0.907]	[-2.196]	[-0.966]	[-2.118]
HHF_Target × Post t+3			-0.132	-0.167	-0.139	-0.231
			[-0.840]	[-1.086]	[-0.877]	[-1.337]
HHF_Target × t+4	-0.024	-0.207				
	[-0.224]	[-1.490]				
HHF_Target × t+5	-0.172	-0.247				
	[-1.026]	[-1.276]				
HHF_Target × Post t+5	-0.208	-0.109				
	[-0.847]	[-0.534]				
LnSize	-0.113***	-0.453***	-0.113***	-0.453***	-0.112***	-0.455***
	[-3.465]	[-7.286]	[-3.448]	[-7.248]	[-3.426]	[-7.277]
Leverage	0.746***	0.629***	0.743***	0.630***	0.745***	0.630***
	[4.699]	[3.803]	[4.657]	[3.819]	[4.672]	[3.829]
CAPX	3.436***	3.294***	3.438***	3.297***	3.448***	3.305***
	[6.195]	[5.814]	[6.149]	[5.784]	[6.164]	[5.806]
R&D	2.634***	0.642	2.646***	0.643	2.644***	0.639
	[5.601]	[1.236]	[5.641]	[1.229]	[5.646]	[1.224]
Intangibility	0.966***	1.349***	0.968***	1.344***	0.972***	1.340***
	[4.430]	[3.329]	[4.443]	[3.317]	[4.443]	[3.313]
4-digit SIC Industry-FE	Yes	No	Yes	No	Yes	No
Firm-FE	No	Yes	No	Yes	No	Yes
Year-FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	8,266	8,266	8,266	8,266	8,266	8,266
R-2	0.139	0.166	0.133	0.161	0.133	0.161

Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Table A.10 – Controlling for 2-digit SIC Industry × Year Fixed Effects (All Hedge Funds)

This table presents the coefficient estimates from OLS regressions. The dependent variable is *TobinQ*. The hedge fund data is from the (updated) dataset used in Brav et al. (2008) and covers the period 1995 to 2011. Firm-level data are from COMPUSTAT. The sample includes firms targeted by hedge funds and control firms (identified using the Abadie-Imbens matching estimator described in Table 4). "*t*" is an indicator equal to one for the year in which a firm is targeted by a hedge fund, and zero for every other year before or after the targeting event year. This indicator is also equal to one for the matched control firm. "*HF_Target* × *t*" is an indicator equal to one for firms targeted by a hedge fund in the year of the targeting event, and zero for every year before or after the targeting event year. "*HF_Target* × *t*" is always equal to zero for the matched-control pairs (firms not targeted by a hedge fund). The other time dummies are defined similarly (see text for further details). We restrict the sample to non-financial firms. Refer to Table 1 for detailed variable definitions. In the table, *t*-statistics appear in brackets and are based on robust standard errors clustered by firm.

Dep. Var.: *TobinQ*	(1)	(2)	(3)	(4)	(5)	(6)
t-5	-0.208***	-0.060*				
	[-3.998]	[-1.708]				
t-4	-0.219***	-0.035				
	[-5.149]	[-0.848]				
t-3	-0.348***	-0.127***				
	[-8.906]	[-2.930]				
t-2	-0.356***	-0.119**				
	[-8.522]	[-2.514]				
t-1	-0.420***	-0.178***				
	[-9.463]	[-3.417]				
t-4 to t-1			-0.332***	-0.133***	-0.316***	-0.129***
			[-10.866]	[-3.748]	[-9.772]	[-3.144]
t	-0.352***	-0.098*				
	[-8.405]	[-1.674]				
t+1	-0.324***	-0.049				
	[-6.688]	[-0.763]				
t+2	-0.278***	0.006				
	[-5.215]	[0.078]				
t+3	-0.241***	0.037				
	[-4.345]	[0.449]				
t to t+3			-0.318***	-0.074	-0.322***	-0.073
			[-9.661]	[-1.333]	[-9.542]	[-1.298]
Post t+3			-0.085**	0.160*	-0.088**	0.161*
			[-2.064]	[1.888]	[-2.111]	[1.900]
t+4	-0.198***	0.073				
	[-3.122]	[0.825]				
t+5	-0.179**	0.159				
	[-2.074]	[1.468]				
Post t+5	-0.018	0.317***				
	[-0.359]	[2.762]				
HF_Target × t-4 to t-1					-0.036	-0.009

					[-1.267]	[-0.148]
HF_Target × t	-0.133**	-0.106**				
	[-2.551]	[-2.040]				
HF_Target × t+1	-0.140**	-0.125**				
	[-2.334]	[-2.081]				
HF_Target × t+2	-0.069	-0.063				
	[-1.062]	[-0.912]				
HF_Target × t+3	-0.137*	-0.096				
	[-1.794]	[-1.363]				
HF_Target × t to t+3			-0.080***	-0.092*	-0.078**	-0.096
			[-2.608]	[-1.778]	[-2.515]	[-1.460]
HF_Target × Post t+3			-0.174***	-0.193**	-0.172***	-0.197**
			[-4.430]	[-2.362]	[-4.359]	[-2.222]
HF_Target × t+4	-0.179**	-0.147*				
	[-2.239]	[-1.868]				
HF_Target × t+5	-0.157	-0.134				
	[-1.616]	[-1.374]				
HF_Target × Post t+5	-0.198***	-0.236**				
	[-3.849]	[-2.192]				
LnSize	-0.129***	-0.515***	-0.129***	-0.519***	-0.129***	-0.519***
	[-14.128]	[-14.988]	[-14.133]	[-15.089]	[-14.094]	[-15.086]
Leverage	0.878***	0.599***	0.881***	0.602***	0.881***	0.601***
	[18.309]	[8.388]	[18.324]	[8.405]	[18.321]	[8.403]
CAPX	4.241***	3.188***	4.260***	3.216***	4.262***	3.217***
	[12.192]	[9.466]	[12.228]	[9.517]	[12.227]	[9.523]
R&D	4.126***	1.634***	4.125***	1.623***	4.127***	1.623***
	[19.500]	[5.998]	[19.355]	[5.958]	[19.345]	[5.959]
Intangibility	1.292***	1.505***	1.296***	1.516***	1.298***	1.516***
	[14.485]	[6.602]	[14.500]	[6.635]	[14.501]	[6.635]
2-digit SIC Industry × Year	Yes	Yes	Yes	Yes	Yes	Yes
Firm-FE	No	Yes	No	Yes	No	Yes
Obs.	25,795	25,795	25,795	25,795	25,795	25,795
R-2	0.257	0.235	0.255	0.233	0.255	0.233

Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Table A.11 –Controlling for 2-digit SIC Industry × Year Fixed Effects (Hostile Hedge Funds)

This table presents the coefficient estimates from OLS regressions. The dependent variable is *TobinQ*. The hedge fund data is from the (updated) dataset used in Brav et al. (2008) and covers the period 1995 to 2011. Firm-level data are from COMPUSTAT. The sample includes firms targeted by hostile hedge funds and control firms (identified using the Abadie-Imbens matching estimator described in Table 4). "*t*" is an indicator equal to one for the year in which a firm is targeted by a hedge fund, and zero for every other year before or after the targeting event year. This indicator is also equal to one for the matched control firm. "*HF_Target × t*" is an indicator equal to one for firms targeted by a hedge fund in the year of the targeting event, and zero for every year before or after the targeting event year. "*HF_Target × t*" is always equal to zero for the matched-control pairs (firms not targeted by a hedge fund). The other time dummies are defined similarly (see text for further details). We restrict the sample to non-financial firms. Refer to Table 1 for detailed variable definitions. In the table, *t*-statistics appear in brackets and are based on robust standard errors clustered by firm.

Dep. Var.: ***TobinQ***	(1)	(2)	(3)	(4)	(5)	(6)
t-5	-0.125	-0.023				
	[-1.448]	[-0.292]				
t-4	-0.141*	-0.009				
	[-1.762]	[-0.098]				
t-3	-0.314***	-0.144				
	[-4.651]	[-1.365]				
t-2	-0.422***	-0.213*				
	[-6.964]	[-1.810]				
t-1	-0.549***	-0.320**				
	[-9.504]	[-2.368]				
t-4 to t-1			-0.328***	-0.180**	-0.289***	-0.122
			[-6.967]	[-2.326]	[-5.676]	[-1.283]
t	-0.430***	-0.150				
	[-5.297]	[-0.924]				
t+1	-0.392***	-0.042				
	[-4.662]	[-0.244]				
t+2	-0.503***	-0.102				
	[-5.247]	[-0.532]				
t+3	-0.277***	0.059				
	[-2.596]	[0.274]				
t to t+3			-0.377***	-0.085	-0.379***	-0.064
			[-6.028]	[-0.675]	[-6.074]	[-0.482]
Post t+3			-0.072	0.220	-0.075	0.242
			[-0.846]	[1.177]	[-0.886]	[1.271]
t+4	-0.301***	0.086				
	[-3.037]	[0.377]				
t+5	-0.226	0.180				
	[-1.554]	[0.698]				
Post t+5	0.054	0.402				
	[0.428]	[1.418]				
HHF_Target × t-4 to t-1					-0.081**	-0.121

	(1)	(2)	(3)	(4)	(5)	(6)
					[-1.973]	[-1.149]
HHF_Target × t	-0.163**	-0.223**				
	[-2.076]	[-2.312]				
HHF_Target × t+1	-0.079	-0.227**				
	[-0.970]	[-2.125]				
HHF_Target × t+2	0.109	-0.093				
	[1.178]	[-0.849]				
HHF_Target × t+3	-0.084	-0.179				
	[-0.638]	[-1.307]				
HHF_Target × t to t+3			-0.052	-0.192**	-0.051	-0.248**
			[-1.159]	[-2.106]	[-1.134]	[-2.036]
HHF_Target × Post t+3			-0.089	-0.158	-0.087	-0.221
			[-1.019]	[-0.890]	[-1.000]	[-1.138]
HHF_Target × t+4	-0.019	-0.186				
	[-0.154]	[-1.252]				
HHF_Target × t+5	-0.168	-0.250				
	[-0.936]	[-1.266]				
HHF_Target × Post t+5	-0.136	-0.086				
	[-1.077]	[-0.351]				
LnSize	-0.078***	-0.478***	-0.078***	-0.479***	-0.077***	-0.482***
	[-4.739]	[-7.194]	[-4.699]	[-7.163]	[-4.652]	[-7.195]
Leverage	0.521***	0.532***	0.521***	0.537***	0.523***	0.537***
	[4.809]	[3.445]	[4.741]	[3.490]	[4.758]	[3.487]
CAPX	4.068***	3.176***	4.085***	3.186***	4.093***	3.201***
	[8.270]	[4.804]	[8.256]	[4.801]	[8.270]	[4.827]
R&D	3.615***	0.822	3.615***	0.812	3.616***	0.805
	[8.434]	[1.533]	[8.431]	[1.499]	[8.439]	[1.487]
Intangibility	1.209***	1.355***	1.215***	1.365***	1.218***	1.363***
	[9.293]	[2.863]	[9.303]	[2.893]	[9.306]	[2.896]
2-digit SIC Industry × Year FE	Yes	No	Yes	No	Yes	No
Firm-FE	No	Yes	No	Yes	No	Yes
Obs.	8,265	8,265	8,265	8,265	8,265	8,265
R-2	0.129	0.267	0.124	0.262	0.124	0.263

Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

CHAPTER 3. HEDGE FUND ACTIVISM AND LONG-TERM FIRM VALUE

(joint work with Miles Gietzmann*)

1. Introduction

FASB 142 (Goodwill and Other Intangible Asset, Issued 6/01) was designed to recognize that the substance of the economic transactions that lead to recognition of goodwill and other intangible assets did not result in "wasting assets" that should be amortized over an estimated useful life as had been assumed in APB Opinion 17. Instead it was recognized that business assets with indefinite useful life were being created and the value of those assets were critical for understanding business performance and hence should be tested for impairment on an annual basis. Typically goodwill is recognized because of an M&A transaction. The goodwill amount that is recognized is determined as the difference between the target company's book value (written up to fair market values) and the equity purchase price paid for the company. Some authors' refer to this as the excess purchase price required to take control. The magnitudes of goodwill and possibly subsequent impairment charges can be very large. In 2012 sixty percent of US firms recorded goodwill and for those firms fourteen percent made a goodwill impairment charge in the year. In total US firms recorded $51 billion of goodwill impairment charges, the highest level reported since the 2008 financial crisis[4].

It is sometimes argued that given the complexity involved in the recognition process, managers have the possibility to introduce self-interested bias because of the discretion in what they report. However since investors can rationally anticipate such behavior, they can re-price firms downward. Thus if managers could commit to not introducing discretion, some types of managers could be better off. The problem here is that rational investors may not believe management claims of non-bias because of non-verifiability. One potential response of management could be to decide to use an independent external expert to test for and certify goodwill impairments. That is, if experts are used to credibly provide certification earlier empirical research on impairment charges has a

* Bocconi University. *Email address*: miles.gietzmann@unibocconi.it

[4] Source Duff and Phelps (2013)

potential omitted variable problem, not least because earlier research asserting that management are strategic is contradicted if one finds that instead some management choose to disclose the use of credible certification that they did not introduce strategic bias.

Our main contribution is to present an analysis of the properties of impairment valuations when the use of an independent expert is disclosed. We are not aware of any prior research that reports on this. Before starting the analysis we give a brief overview of the formal problem and selectively cite some earlier research on goodwill impairment. In section 2 we review the literature on certification.

1.1. The Demand for Certification and Selected Prior Literature on Impairment Timeliness

A primary question is; what are the economic forces that explain why a firm uses a certifier to conduct impairment testing, and if they do, why make a public disclosure to the market of the use of such an expert. One argument could be that the regulator (here the SEC) either requires it or strongly "suggests" it. However in terms of fundamental underlying parametric forces we suggest the starting point for any attempt to answer this question needs to start with a clear characterization of the information environment. First, in terms of the nature of any proposed goodwill impairment disclosure it is we suggest appropriate to view it as unverifiable information[5] because it reflects valuation of the future success or failure of a typically highly idiosyncratic M&A transaction. For instance Ramana and Watts (2012) argue that the "The current fair value of goodwill is unverifiable because it depends in part on management's future actions (including managers' conceptualization and implementation of firm strategy" pp 749. In such a setting it is natural to ask whether an expert could help with the valuation of goodwill. It will be assumed the expert's assessment of impairment value is still publicly unverifiable (after certification) but that the expert certifier has some unique expertise in the area[6] which endows her with the ability to make a more precise forecast than investors. A unique institutional feature in this setting is that although the expert does not disclose a verifiable signal to the public, the expert is required to disclose the results of

[5] See Bolton et al (2007) who look more generally at the industry of financial services for more detail on this point.
[6] See for instance Causholli and Knechel (2015) for an application of this in the area of auditing when it is viewed as a credence good and Ottaviani & Sorensen (2006) on the information economics of the use of use professional advisers more generally. Valsecchi (2013) provides a review of the economics literature on the use of experts.

impairment tests and proposed disclosures if any, to the SEC on a confidential[7] basis. Given the SEC gets to see all tests and has its own experts this acts as a controlling device. That is the literature on the use of experts, such as the review paper by Valsecchi (2013), points out that the "quality" of an experts performance will depend critically upon the incentives (possibly reputational) and controls that an expert certifier faces. In this case of expert certification of goodwill impairments we are assuming the subsequent reporting to the SEC acts as an important incentive control. We shall henceforth assume that the certifier discloses valuations honestly given the SEC oversight[8] and our main focus will be on the reaction to publicly certified goodwill impairments. In this setting there are at least two reasons why financial markets may react to disclosure when an expert certifier has been used to determine the level of an impairment charge. First since the certifier is assumed endowed with more precise information on the valuation, the market can be seen to be reacting to a reduction in information uncertainty (risk) when an honest expert certifier is used. However a related interpretation in the language of adverse selection is that the certification is used to show that the firm wants to indicate it is not a lemon and that the disclosed impairment is credible. There is a large literature industrial organization on (quality) disclosure and certification reviewed[9] by Dranove and Jin (2010) that delineates the limits of such claims. To summarize on the basis of the above argument, the primary economic forces that explain the use of expert certifiers is in terms of the experts endowment with more precise information on the current valuation of goodwill and the experts preparedness to disclose this honestly.

Now, trying to relate this to the extant accounting literature we note that a central motivation for the design[10] of SFAS 142 was to promote timely recognition and disclosure of impairment losses for goodwill and other intangible assets. The advised methodology for measurement and recognition of impairment changed considerably[11]. In the related FASB 141, (Business

[7] This is because of potential subsequent legal liability for the certifier if the report was made public. In such a case the certifiers probably would add a significant (legal liability) risk premium to their fees.

[8] Given existing data restrictions, relaxing this assumption is a potentially challenging future extension.

[9] We review some of the applications and findings of their paper below.

[10] For a more detailed review of SFAS 142 see Chen et al. (2008).

[11] See FASB 2001 a,b.

Combinations) the pooling of interests method was eliminated so that goodwill would become identifiable with the economic substance of business transactions and new types of intangible assets became identifiable so that the disclosed goodwill item contained less heterogeneous items. Under SFAS 142 intangible assets were classified as either limited-life or indefinite-life intangibles, were limited-life assets were subsequently amortized in contrast to indefinite-life assets which were not and were instead impairment testing was to be applied. Goodwill was required to be evaluated for impairment annually using fair values to determine any write-off values. Thus in contrast to the prior (undiscounted) cash flow recovery test, goodwill was now required to be assessed on a two-step fair-value based test, applied at the reporting unit level. If fair value was assessed to be less than book value a second stage test needs to be conducted on the applied value of goodwill to determine the numerical goodwill impairment charge. Clearly determining the magnitude of such reported charges could be significant for market valuation and moreover is complicated and allows for discretion to be applied. Not surprisingly this discretion in the valuation process allows companies to be strategic in the way they choose to make impairment charges. Beatty and Weber (2006) argue that managers may choose to delay or accelerate impairment losses because of contracting and market incentives. They make the point that rational investors should anticipate the implications of such strategic behavior because if the initial disclosed impairment losses are understated, firms are more likely to experience future impairments and in contrast if the disclosed amounts are overstated firms are more likely to avoid future impairments. Thus the market when pricing the company's securities should form expectations about strategic behavior and price accordingly. On a related theme Bens et al. (2007) find that at cross-sectional differences in firm informational asymmetry and relative costliness of implementing impairment testing determine the magnitude of investor pricing responses to impairment disclosures. In summary multiple studies (Hayn & Hughes, 2006; Li & Sloan, 2014; Ramanna, 2008; Ramanna & Watts, 2012; Riedl, 2004; Watts, 2003) have proposed that managers bias financial reporting opportunistically by not always booking economic impairments in a timely manner[12] however those papers do not explain how managers choose a specific level of bias. In this respect a multi-country study by Knauer and Wohrmann (2015) show "that investors react more negatively when a country's level of legal

[12] See also the forthcoming paper by Paugam and Ramond (2015).

protection is low and allows more management discretion. We further report that the market reaction can be associated with managers explaining the write-down decision and their simultaneous use of independent expert certifiers for those explanations. Investors react more negatively when an unverifiable internal explanation is given and less negatively when a certified explanation is provided". That is the Knauer and Wohrmann (2015) study suggests the level of bias introduced depends upon on the supporting country specific legal regime[13]. Furthermore they argue that the market reacts less negatively to more verifiable impairment disclosures. This is the point of departure for our research from earlier research in the area. Rather than assume pervasive bias (perhaps differing cross-countries) this research explains why some managers would like to employ an independent valuation expert to certify impairment disclosures as being made without bias.

From a theoretical perspective the reason rational investors form an opinion that impairment disclosures may be made strategically is because they fear a classic Akerloff lemons (adverse selection) problem with hidden information. Just as the car sellers in the Akerloff model cannot convince potential purchasers that the car they are trying to sell is not a lemon and lowering prices only compounds the problem, rational investors observing goodwill impairment charges rationally anticipate that companies may be delaying full impairment recognition and so anticipate the worst that more impairment charges are likely[14]. In the theoretical literature work first developed by Viscusi (1978) the possibility of certification was introduced as a means to overcome the lemons problem. This links to what happened in practice post the introduction of SFAS 142. Increasingly companies have employed independent valuation experts in a manner consistent with wanting certification of impairment charges in order to address the lemons problem. In section 2 we comment how the literature review by Dranove and Zhe (2010) looks at the economics of certifiers from both a theoretical and empirical perspective (e.g. in HMO's and restaurants). The idea of certification has been applied in other areas and this research considers whether the results in those other settings can be viewed as analogous to financial goodwill impairment. As statutory auditors are potentially competitors for independent valuation we will first briefly provide an

[13] See also Amiraslani et al (2013) for an in depth study on related issues.

[14] Informal support for this was provided to us by a former senior City of London analyst who argued that he used the heuristic that "*bad news happens in three's*" – the first goodwill impairment could be interpreted as a precursor to on average two more impairments.

informal discussion on the performance of external auditors in impairment testing and the observed subsequent movement to increased use of experts in goodwill impairment testing.

While theory can explain the potential gain from using an expert independent certifier it is silent on exact identification of experts. Since firms already pay for another class of financial expert (external auditors) to analyze the firm it is natural to ask: why should the firm go to the additional cost of employing another independent financial valuation expert to assess goodwill? Historically firms have worked closely with their external auditor and relied upon the associated audited statements to credibly certify the need for and possible level of impairment charges. However one problem with this line of argument is that the PCAOB has repeatedly criticized auditors for being "soft" on clients when it comes to impairment charging. For instance the chief auditor of the PCAOB, Martin Baumann (2012) in a speech to the AICPA reported that "PCAOB inspectors *continue to observe* instances in which the circumstances suggest that auditors did not appropriately apply professional skepticism in their audits. As examples, audit deficiencies like the following, observed in our inspections, raise concerns that a lack of professional skepticism was at least a contributing factor: The engagement team did not evaluate the effects on the financial statements of management's determination not to test a significant portion of its property and equipment for impairment, despite indicators that the carrying amount may not have been recoverable. These indicators in this situation included operating losses for the relevant segment for the last three years, substantial charges for the impairment of goodwill and other intangible assets during the year, a projected loss for the segment for the upcoming year, and reduced and delayed customer orders". As a further example of these concerns a formal PCAOB inspection report released around the same time for one of the Big – 4 auditor Firms found that: "The issuer had prepared cash flow projections for one of the models used in its fair value determination for both interim and annual goodwill impairment analyses. In both analyses, the issuer forecasted significant growth rates in a new line of business. In evaluating these assumptions, the Firm inquired of management and considered the growth rates associated with another company's new product. The Firm, however, failed to assess whether the issuer would be able to achieve the significant growth it had projected. Also, during the year under audit, the issuer changed the weighting between the models it used in its fair value determination. The Firm, however, failed to perform procedures, beyond inquiry of management, to assess the appropriateness of the change in the weighting between these models. In addition, the issuer made both a five - year and an eight - year revenue projection as part of its annual goodwill impairment analysis and used lower discount rates in both projections than it had used in its interim analysis. There was no evidence in the audit documentation, and no persuasive other evidence, that the Firm had evaluated the appropriateness of the discount rates used in the issuer's annual analysis, even though the issuer would have failed step one of the goodwill impairment test had it used the same discount rates that it had used in its interim goodwill impairment analysis. Also, the Firm accepted the issuer's assumed terminal growth rate used in its five - year projection, without further evaluation, despite the view of the Firm's internal specialist that the growth rate appeared somewhat high".

In in similar vein a recent report by Acuitas summarizing the findings of a significant set of PCAOB reports, argues that the PCAOB has identified the general topic of Fair Value Measurement (FVM) which includes Goodwill Impairment as a major cause of deficiencies in audits. For instance commenting on PCAOB inspection in 2011, Acuitas (2012) observes[15] that "Of the 45 available inspection reports, 21 had FVM and impairment audit deficiencies."

These extracts above make clear the repeated difficulties that external auditors have with dealing with impairment decision making. Given such experiences relying on the external auditors to provide certification for impairments may not seem credible to investors. In order to provide certification, firms need to use independent valuation experts that are specialists at valuing the financial progress of M&A transactions rather than auditors whose core skill is to provide attestation of historically recorded financial transactions. In this respect a speech to the national AICPA conference; Hunsaker (2007) commented that "During the past year, the (SEC) staff, has seen an increase in the number of companies that have chosen to make reference to the use of an independent valuation firm or other expert in both periodic filings and registration documents …. (including) the use of an independent valuation firm to assist in the process of determining goodwill

[15] Recently Bens et al (2015) have shown that questionable FV practices (or lack of associated disclosure) in audited financial statements are a major source of comment letters sent by the SEC to companies.

impairment". In her speech she makes clear that disclosing the use of an expert is a discretionary choice and that "there is absolutely no requirement to make reference to an expert just because the expert was used and their findings were considered in the registrant's analysis. Rather, instead of naming the expert and obtaining the consent, the registrant could simply delete the reference to the expert." A priori this suggests that we would expect to see diversity in the disclosed use of experts and that underlying this is a two dimensional strategic choice by companies whether or not to (I) use an expert and (II) if used, whether or not to disclose it publicly.

2. Prior Research on the theory and empirics of the use of Certification

A major empirical finding of this research is that the management of some firms disclose the use of expert independent valuation consultants to produce certified estimates for goodwill impairment charges while at the same time others do not (existence of a separating equilibrium). In a major review of both the theoretical and empirical industrial organization literature on certification Dranove and Zhe (2010) provide a diverse list of industries in which certification is used. In their Table 1 on *Quality Assurance Mechanisms Used in Various Markets* they report a list of 8 markets in which empirical research has identified where external certifiers are used as a method to provide assurance. The markets identified include Hospitals, Restaurants and Airlines. In Table 3 *List of Cited Empirical papers by Industry* they provide a lengthy list of empirical papers that investigate quality disclosure with or without certification.

More directly in financial markets we note[16] that Muller and Reidel (2002) and Muller et al. (2011) show how certification has a role to play in valuing funds that invest in European Real estate and Kisgen et al. (2009) explain[17] and evaluate how M&A targets and acquirers use third party investment banks to provide (certified) fairness opinions.

Taken as a whole those empirical studies find diversity in the use of certifiers. This naturally leads

[16] We would like to thank Peter Pope for alerting us to these two papers.
[17] We would like to thank Mauro Bini for alerting us to this paper.

to the question; what theoretically explains this observed diversity. The first formal model of certification was developed by Viscusi (1978). In the classic unravelling models once the highest quality firm discloses (in order to distinguish itself from lower quality firms) this generates a cascade of incentives for firms just below to also disclose until in the limit all firms disclose. In contrast in his model Viscussi explains how the unravelling result may not apply if an independent expert certifier is used. Dranove and Jin (2010) explain that[18] the basic unravelling result is not born out in practice as typically voluntary disclosure is observed to be incomplete. They argue that "This is not surprising because the basic unravelling result requires several often strong assumptions" pp. 943 to hold. Rather than adopt the extreme position of requiring mandated disclosure by a government agency they argue the problem can be resolved if external certifiers provide precise and unbiased information about product quality. The theoretical literature in this area has focused upon how noise in data collection by certifiers can combine with the narrow self-interest of certifiers to give rise to potential conflicts of interest and Dranove and Jin comment upon the case of Enron were Arthur Anderson received large audit fees and at the same time was asked to provide independent consulting (certification) services which, following the enactment of SOX is now disallowed and thus provides a further rationale for why firms may not depend on their external auditors for specialist consulting (goodwill) valuation services. Thus if an external certifier is used it is natural to ask is there evidence that disclosure of a certifiers report improves the quality of the good or service in question? To date empirical research has found heterogeneous responses by sellers. In summary Dranove and Jin pp959 argue that "Research suggests that quality disclosure is a two-edged sword, with problems including measurement error, consumer misunderstanding and inspector bias."

The three papers that are closest to our research setting in focus are Muller and Reidel (2002), Muller et al. (2011) and Kisgen et al. (2009).

Muller and Reidel (2002) looked at 64, UK investment property firms over the period 1990 – 1999.

[18] Most of the cases they consider refer to a product or service that in principle can easily be inspected. When one considers more complicated problems like determination of goodwill valuations these issues are further attenuated. We will discuss this in the main text.

They found that market makers in the stocks setting bid ask spreads, set lower spreads for firms that employed external property portfolio valuation appraisers versus those using only internal appraisers which they concluded demonstrated the market interpreting valuations produced by external appraisers as being characterized by less informational asymmetry. More recently Muller, Reidel and Sellhorn (2015) compared a group of investment property firms that voluntarily provided fair values for long lived (tangible) property assets prior to adoption of IAS 40 (which mandated adoption of fair values) to a control group that did not voluntarily provide fair value information. Their main result was that asymmetries persist and so one should not assume fair value accounting removes all differential information asymmetry. However since their main concern is not the effects of external appraisers (certifiers) their research design does not give clear results on the use of certifiers for voluntary and subsequent mandatory adopters. They find that the effect of external appraisers on the reliability of fair values in their primary sample, is not statistically significantly different between the two classes of adopters. They explain this in terms of the mandatory adopters' use of external appraisers leading to significantly more reliable fair value measurements. Taken together these two papers provide interesting empirical support for certifiers being valued by the market in an investment property setting. However we should exercise caution before inferring that this applies in a general M&A goodwill setting for a number of reasons. First investment property pricing, databases exist to provide support for pricing estimates and secondly a history of recorded related (geographically) transactions is available. So while an external appraiser does need to apply professional judgment, relevant verifiable datasets exist comparing similar properties to assist in this task. This suggests to us that the task of external appraisers in investment property pricing is characterized by less informational asymmetries than in M&A goodwill valuation.

Another paper close in spirit to ours is Kisgen, Qian and Song (2009). They look at deal premiums when the acquirer or target in an M&A deal use an external appraiser to provide a *fairness opinion*. The external appraisers are often the investment bankers on the deal. They find that the deal premium is lower if an acquirer obtains a fairness opinion and is further reduced if multiple appraisers provide an opinion. However, the announcement period returns are lower for the acquirer when they have a fairness opinion especially in the case were they pay a higher premium. This provides further empirical support for the view that the use of certifiers has real capital markets affects.

The above discussion provides the motivation for our following two principal hypotheses.

H1 (Less under-recognition bias with credible certification): When firms disclose use of an independent financial valuation expert we expect the impairment charge to be higher than the market average when the use of experts is not disclosed.

H2 (Less delayed partial release of impairment bad news with credible certification): Firms that disclose the use of an expert are less likely to experience impairment charges in following years.

It is interesting to note that our theory of certifying experts provides a competing hypothesis to big bath earnings management models that claim impairment charges are higher in a given year were poor results are going to reported since the penalties are less if the “decks are cleared” for future years.

3. Data

To analyze the relation between the disclosure of valuation experts usage and goodwill impairment, we first download all 10-K filings from the SEC EDGAR (the Electronic Data Gathering, Analysis, and Retrieval system) database over the period (start) 2002 to (end) 2014. The total number of 10-K filings is 111,405. Then we merge this data set with COMPUSTAT annual industrial database and delete all unmatched observations. Next, following usual practice, we remove the financial firms (SIC code from 6000 to 6999) and the firms which are not incorporated in U.S. This leaves us with 66,948 firm-year observations.

In order to collect the information about the disclosure of valuation experts in goodwill impairment testing, we use the following algorithm to implement multi-keyword searches in each 10-K.

Paragraphs were required to satisfy the following criteria:

1. Contain the keyword "goodwill" and also contains at least one of the following words: "impairment" or "write-off" ("writeoff", "write off") or "write-down" ("writedown", "write down");

2. The paragraph also contains at least one of following word: "expert", "third party" ("third-party"), "independent", "valuation company", "consulting", "consultant", "outside", "external".

We found that 9,482 filings satisfied criteria 1 and 2. Having extracted all these paragraphs we then manually read them to verify the use of valuation expert by firms. The final sample[19] of filings which disclosed the usage of valuation expert in their goodwill impairment test is 1,272.

3.1. Control Variables

Next we collect data from COMPUSTAT the standard control variables used in the literature for our empirical tests.

- Goodwill (*Goodwill*); defined as the value of goodwill divided by total assets.
- Size (*logSize*); defined as the logarithm of the total assets at the fiscal year end. Size captures aspects of a firm's operating and business environment.
- Market to Book (*Market-to-Book*); defined as the market value of equity divided by the book value of equity at the fiscal year end. Heterogeneity in Market to Book captures fundamental differences in the growth options that firms face.
- Return of Asset (*ROA*); defined as operating income before depreciation divided by total assets
- Leverage (*Leverage*); defined as the sum of long- and short-term debt divided by total assets.
- Sales (*SALE*); defined as total quarterly sales divided by total assets.
- R&D expenses (*R&D*): defined as quarterly research and development expenses over quarterly total assets. As more R&D expenses may be consistent with business complexity we expect to see a positive coefficient.
- Tangibility (*Tangibility*): We include an additional measure of intangible assets which is defined Property Plant and Equipment over Total Asset for each firm-quarter. We predict the sign to be negative, since Tangibility partially reflect lower potential information asymmetry.

[19] The database will be available as an internet Appendix.

- Cash (*CASH*); defined as Cash and Cash Equivalents over total assets. Since higher cash reserves indicates assets values that do not need to be explained we expect to see a negative coefficient.
- Growth (*GROWTH*); defined as the growth of sales from previous fiscal year to current fiscal year divided by previous year sales to standardize it.
- Capital Expenditure (*CAPX*); defined as the value of capital expenditure divided by total assets.
- Dummy Loss (*DummyLoss*); a dummy variable equals to one for negative operating income before depreciation, zero elsewise.

Table 1 reports the descriptive statistics for these control variables for the sample in our empirical analysis. In Panel A, the summary statistics of variables for the whole sample in our analysis is listed. In order to compare the firms' characteristics between the firms who disclose expert use and the firms who do not, we also separately list the summary statistics by each group in Panel B. In Panel C, the distributions of disclosed use of experts by year and by the Fama-French 17 Industries classification are provided.

[Place Table 1 here]

4. Empirical Analysis

In this section, we implement our major empirical tests. First, we implement OLS regressions with fixed effects (firm and industry). We also implement the Logit model and Cox hazard ratio model estimation to investigate the relationship between disclosure of use of a valuation expert and goodwill impairment. One potential criticism of the OLS methodology is that, the results from the regression analysis can be only interpreted as the correlation between dependent variable and independent variable rather than a causality analysis. In order to address this concern we then use two other empirical methodologies to investigate causality: instrumental variables estimation and Abadie-Imbens matching estimator to identify the control firms (Abadie and Imbens, 2006).

4.1. OLS regression analysis

Ceteris paribus following hypothesis ***H1*** when a firm discloses use of an independent valuation expert the absolute magnitude of any contemporaneous goodwill impairment charge should be larger (more negative), consistent with less downward bias (under-recognition) being introduced strategically by management.

That is in specification (1) the critical variable we want to investigate is the coefficient β_0 since if it is statistically significantly negative this would support the hypothesis that the use of valuation experts increase the credibility of certified impairment reports being less downwardly biased.
Here:

$$GWI_{i,t} = \alpha + \beta_0 Expert_t + \gamma X_{i,t} + \nu_i + \tau_t + \varepsilon_{i,t} \qquad (1)$$

- $GWI_{i,t}$ is the goodwill impairment normalized by previous year total assets in year t for firm i.
- $Expert_t$ is a dummy variable which equals to one for the firm year in which valuation expert usage is disclosed to assist management in their goodwill impairment test.
- $X_{i,t}$ is the vector of control variables which we include in the regressions;
- ν_i is firm *i*-fixed effect (industry fixed effect in second group of tests);
- τ_t is year *i*-fixed effect.

The results are reported in Table 2 Panel A and Panel B which we will comment on below.

4.2. Logit Model Estimation and Cox Proportional Hazard Model

In this section, we focus on the direct relationship between goodwill impairment and disclosure of expert usage. Different from the OLS regression in the previous section, we consider here the dummy which equals to one if firm reports any goodwill impairment, zero otherwise as the dependent variable rather than the magnitude of impairment. The purpose of this group tests is to investigate whether the expert usage could increase the probability of goodwill impairment in subsequent years. Interpretation of results are discussed below in subsection 4.5.

In the first test, we use a Logit Model to estimate the effect of expert usage. The dependent variable is a dummy (*DummyGWI*) which is equal to one if the firm reports goodwill impairment in that fiscal year, zero otherwise. Here *X* is the vector of established control variables to control other factors which could influence the goodwill impairment, such as firm size, market-to-book, etc. The coefficients of variables can be interpreted as indicating the increase or decrease in the probability of reporting goodwill impairment caused by the variables. Here we will focus on the coefficient of *Expert*, since it reflects the change in probability following the disclosure of expert usage:

$$P(DummyGWI = 1 \mid X) = \frac{\exp(x'\beta)}{1+\exp(x'\beta)}$$

In the second group of tests, we use Cox Proportional Hazard Model to estimate the hazard ratio. The logic behind this test is similar to the Logit model. We categorize as a failure event a year in which a firm reports a goodwill impairment given the firm has not reported any goodwill impairment for past five years. The Cox Hazard Model estimates the determinants of the duration of a failure event, in our case that of goodwill impairment. Interpretation of results are discussed below in subsection 4.5.

Next we turn to reconsider the research design to test the causality of the disclosure of valuation expert usage and the reported value of the goodwill impairment.

4.3. Instrumental Variables Approach

One concern with estimation results from OLS regressions is that, the reverse causality between reporting impairment and use of experts may lead to biased estimation. To address this problem, we consider the Instrumental Variable (IV) approach as a tool to test for causal effects. Instrumental variable methods allow consistent estimation when the independent variables are correlated with the error terms of a regression relationship. Such correlation may occur when a reverse causality exists between dependent variable and independent variables.

To structure our IV model we use SEC Comment Letters that specifically reference goodwill concerns as our choice of the instrumental variable. We do this by searching through all SEC Comment Letters that include the term "goodwill". This sub-sample is helpful because when managers predict they will possibly receive a Comment Letter from the SEC about their goodwill impairment, in such a setting, contemporaneous disclosure of the use of a valuation expert to certify impairment charges should increase. This follows because it is assumed that in the minds of the SEC disclosure of use of an independent expert, should increase the credibility of the reported impairment number. On the other hand, because comment letters are received after managers' report goodwill impairments in the financial statements, the comment letters cannot directly influence the reporting of impairment charges with the effects only working through the disclosure of expert usage.

4.4. Nearest Neighborhood Matching

In order to address the potential issue that larger firms with larger goodwill impairments choose to disclose use of an independent valuation expert we use nearest neighborhood matching to create matched pairing. The treatment is defined by the disclosure of valuation expert usage. When estimating causal effects, we need randomized experiments by using all observations. However in the real world such randomized experimentation is hard to implement. Nearest Neighborhood Matching (Abadie and Imbens, 2006) provides a tool to test causal effects by using semi-random sampling. Based on the control variables we use (we match by using Size, Goodwill, Market-to-Book ratio, and two digits of SIC code), the neighborhood distance between the observations in treatment group (disclosure of use of an expert) and the observations in control group is calculated. Then for each observation in treatment group, the nearest neighborhood observation in control group (with the smallest value of distance) is selected. By using this method, we get a semi-randomized sample by finding a matching observation in the control group for each observation in the treatment group. Then we repeat the OLS regressions by controlling firm- and industry fixed-effects for the matched sample.

In Table 5, we report the regression results for our matched sample. Consistent with our OLS regressions using the whole sample, we still find support for our main hypothesis H1 when we use the matched control group, firms which disclose the usage of a valuation expert in the goodwill impairment tests, still have larger impairment charges than in matched control group.

4.5. Discussion of Results

In this section we report and discuss the results of the empirical test specifications identified above.

4.5.1. OLS regressions

[Place Table 2 Panel A and Panel B here]

In Table 2, we present an OLS analysis to show the relationship between the disclosure of valuation expert usage in goodwill impairment testing and the reported magnitude of the goodwill impairment. *Expert* is a dummy variable which equals one for the year which the firm discloses it uses a valuation expert in the goodwill impairment testing in a 10-K filing, zero elsewise. We use firm-fixed effects in the Table 2 Panel A regressions to capture the time series change of goodwill impairment for each firm in our sample. As a further control in the regressions we also take into account disclosure of use of an expert in the previous two years. Based on hypothesis H1 we do not expect a significantly negative coefficient for these two dummies for the previous two years disclosure of expert usage. The results are consistent with our predictions. Consistent with hypothesis H1 the regression results, on average, show that firms report significantly more goodwill impairment (a larger negative number) during the year in which they disclose the usage of a valuation experts in their goodwill impairment testing. This increase is both economical and statistical significant after applying standard control variables and incorporating firm year fixed effects which capture any latent factors.

We use industry-fixed effects in Table 2 Panel B to capture the cross-sectional comparison between firms. Different from the interpretation of coefficients in Table 2 Panel A, cross-sectional regressions compare the effects of the disclosure of expert usage for firms within the same industry. These results show that, comparing firms in the same industry, reported goodwill impairment is significantly higher (a more negative number) for firms which disclose the use of an expert. Combining the results from Table 2 Panel A, for both time series and cross sectional (variation) regressions, firms consistently report higher levels of goodwill impairment for the year they disclose that they use a valuation expert in their goodwill impairment tests.

Column (5) in both Table 2 Panel A and Panel B, provide support for hypothesis H2. After controlling for use of a disclosed expert in current year, the disclosure of expert use in previous year significantly lower the goodwill impairment in the subsequent year. This confirms our previous discussion that the disclosure of expert usage provides a supporting evidence to the market about the credibility of certified impairments. Based on this information, the firms which disclose use of a valuation expert could be separated from lemons to prevent a negative overreaction from the market when investors observe a goodwill impairment being publicly disclosed.

4.5.2. Determinants of the Probability of Goodwill Impairment

In this sub-section, we discuss the use of Logit and Cox Proportional Hazard Models to estimate the determinants of the goodwill impairment. Different from the OLS regressions in the previous sub-section, in this group of tests, we consider whether the firm reports a goodwill impairment or not, rather than the magnitude of impairment. What we want to investigate in this section is whether disclosure of expert usage increases the *probability* of reporting a goodwill impairment. The results are provided in Table 3 and Table 4.

[Place Tables 3 and 4 here]

In Table 3, we show the results from Logit estimation. The interpretation of the coefficients β of each variable is the increase of probability of reporting a goodwill impairment in the contemporaneous fiscal year. From the estimation results, the disclosure of expert usage in the current year has strong positive effect on the probability of reporting a goodwill impairment in the contemporaneous year. Just considering disclosure of use of an expert in the previous two years also increases the probability of an impairment charge in the current period, but when we also include disclosure of expert usage in the current year they become insignificant, implying the current years disclosure of expert usage is the major determinant of the probability of an impairment charge.

In Table 4, we use a Cox Proportional Hazard Model to estimate the determinants of the duration of goodwill impairments. We categorize as a failure event, the year in which a firm reports a goodwill impairment. To construct our sample, we use all firms that haven't disclosed goodwill impairment in the last 5 years. Once a firm discloses an impairment, the firms is dropped out of the sample. We allow a firm to re-enter the sample after the firm has not reported any impairment for at least 5 years. Consistent with the results in Table 3, we also find that firms that disclosed use of an expert have a higher likelihood to report goodwill impairment, and when we include current year and previous two years expert usage dummies, only the current year dummy has a significantly positive coefficient, which is consistent with hypothesis H1 and what we found in the Logit Model estimations.

4.5.3. Instrumental Variable estimation

In order to mitigate the concern of reverse causality problem in the OLS regressions, we apply several different econometric tools to estimate the effect of disclosure of use of an independent valuation expert on the magnitude of any reported goodwill impairment. In the first test, we use SEC Comment Letters as an instrumental variable. The basic idea is that, when firms want to report a significant level of goodwill impairment then given this puts them under increased SEC scrutiny, they expect it to be more likely they will receive a (follow-up) Comment Letter from the SEC. Thus firms have a higher motivation to disclose a number that will stand up to SEC scrutiny and so may want to employ an independent valuation expert to increase the credibility of the reporting number (provide certification). However this increased likelihood of receiving a Comment Letter does not directly influence the reported goodwill impairment. So if we find any evidence that, in our instrumental variable estimation, disclosure of valuation expert usage is associated with larger goodwill impairments, then the results should also hold when the expectation of receiving a comment letter has no impact on the reported impairment.

[Place Table 5 here]

In Table 5, we use two-stage least squares estimation by using the STATA command "ivegress" and select four instrumental variables in our tests: the Comment Letter relating to current year goodwill impairment; the Comment Letter relating to previous year goodwill impairment; the Comment Letter relating to any current year corporate governance variable; and the Comment Letter relating to any previous year corporate governance. These first two instrumental variables are dummy variable which are equal to one if the firm receives any comment letter related to goodwill impairment from the SEC, zero otherwise; the last two instrumental variables are dummy variable which equal to one if the firm receives comment letter regarding to any aspect of corporate governance, zero elsewise. We focus on the two instrumental variables relating to the current year, the reason being that, they are more relevant to the decision of disclosure of expert usage. The results are similar to the evidence we found in the OLS regressions: the disclosure of expert usage increases the magnitude of reported goodwill impairments.

4.5.4. Regressions by using matched sample

Another way to estimate the causal effects is to use a matched sample. We use the nearest neighborhood matching to create a matched pair for each firm year observation when the firm discloses the usage of a valuation expert in a 10-K filing. The matching procedure is based on firm size, goodwill, market-to-book ratio and two digit SIC code to find the closest firm characteristics between treatment group and control group. After constructing the matched sample, we then repeat the regressions in Table 2 Panel A and Panel B by controlling firm- and industry-fixed effects. For the matched sample, Table 6 Panel A, provides the coefficients of the *Expert* variable which are all statistically and economically significant with sign consistent with our earlier predictions. These results provide additional evidence that the disclosure of expert usage has a causal effect on the magnitude of the reported goodwill impairment. The results are robust when we use different controls and firm- and industry-fixed effects. We also report that for the ATE (Average Treatment Effects) estimation in Panel B, the results still consistently show that compared to the matched control group, firms which disclose expert certification report higher magnitudes of goodwill impairment.

[Place Table 6 here]

4.6. Debt and Valuation Expert

One concern of the effect of valuation expert use is that the firm may use the expert before any debt raising in the following years to evaluate the goodwill for preparing the public and private credit agreements. In order to test whether debt raising has explanatory power we use several measures which relate to debt raising in our tests to investigate the effect of valuation experts. In Table 7, we use total debt, long-term debt, short-term debt, unsecured debt and convertible debt as dependent variables and use current and the previous year's expert usage as an independent variable to test the potential effects. According to the empirical results, there is not any significantly positive relationship between debt and disclosure of expert certification in the regressions.

[Place Table 7 here]

5. Conclusion

Empirical models that do not allow for equilibrium responses of agents to informational asymmetries risk reporting spurious results if an agent's full-range of rational responses are not considered. In the empirical area of disclosed goodwill impairments it is often assumed a priori that because discretion can be introduced this necessarily leads to all managers strategically disclosing biased downward impairments. However in equilibrium investors can rationally anticipate such behavior and (downgrade) price the assets of firms accordingly. That is strategic disclosure of goodwill impairment reporting is priced by the market and so it is not self-evident that firms should always want to report with downward bias if they could in some way credibly commit to not reporting strategically they would not be subject to rational equilibrium downgrading. Thus rational firm management should conduct a cost benefit analysis of strategic reporting which results in downgrading versus non-biased (non-strategic) reporting which may not lead to downgrading. At issue though is what "device" could they use to show credible commitment to not-bias impairment recognition?

In this research we introduce the possibility that firms employ an independent valuation expert to certify goodwill impairment disclosures. In our large sample of firms we observe that a proportion of firms choose to disclose the use of an independent valuation expert to certify impairment charges. Our hypotheses predict that the reason for this is disclosure is that firms want to influence investors' beliefs that they are not reporting strategically and introducing downward bias into impairment disclosures. This is in contrast to most of the existing empirical research in the area that unquestioningly assumes firms are always introducing bias strategically. Accessing the public site SEC Edgar, we create a database of firms that disclose in the notes to financial statements the use of independent valuation experts. Consistent with theory we find that the firms that disclose use of experts do so in order to promote credibility of impairment recognition by reporting the use of independent valuation experts who certify goodwill impairment recognition. Consistent with investors being rational to interpret certified oversight as credibly reducing recognition bias, we find that firms that disclose use of an expert report consistently higher impairment charges in the contemporaneous year and that they are then less likely to make additional impairment charges in subsequent years.

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Table 1 - Descriptive Statistics

This table reports descriptive statistics for firms in the sample. All firm level data are from the COMPUSTAT industrial database. The expert usage disclosure data are gathered from 10-K filings from SEC Edgar database for the period from 2002 to 2013. The sample excludes financial firms (SICs from 6000 to 6999). Expert is a dummy for valuation expert usage disclosure which equals to one if the firm report its expert usage in 10-K filing, zero elsewise. Goodwill Impairment is after-tax goodwill impairment (gdwlia) devide by lagged total assets. Goodwill is goodwill (gdwl) divided by total assets. LnSize is the natural logarithm of total assets. Market-to-Book is the ratio of market value of equity to book value of equity. ROA is defined as operating income before depreciation (oibdp) divided by total assets. Dummy Loss is equal to one if operation income is negative, zero elsewise. Growth is defined as the change rate of sales from previous year to current year. Leverage is defined as total long- and short-term debt (dltt+dlc) devided by total assets. Tangibility is the ratio of property, plants, & equipments (ppent) to assets. CAPX is capital expenditures (capx) divided by assets. R&D is the ratio of R&D expenses (XRD) to assets. CASH is defined as cash and short-term investments (che) divided by assets. CL-Goodwill is equal to one if the firm receive SEC comment letter regarding to current year goodwill, zero otherwise. CL-SEC is equal to one if the firm receive any comment letter from SEC, zero elsewise. The firm characteristics variables are winsorized at 1 / 99%

Panel A: Summary Statistics for the whole sample

Variables	Mean	Median	St. Dev.	25th PCTLE	75th PCTLE	Obs.
Expert	0.018	0.000	0.135	0.000	0.000	66,948
Goodwill Impairment	-0.009	0.000	0.115	0.000	0.000	66,948
Goodwill	0.086	0.000	0.140	0.000	0.127	66,948
LnSize	4.912	5.165	2.905	3.153	6.990	63,017
Market-to-Book	1.946	1.602	10.293	0.761	3.135	55,249
ROA	-0.355	0.081	1.604	-0.095	0.143	62,593
Dummy Loss	0.314	0.000	0.464	0.000	1.000	66,948
Growth	0.239	0.067	1.081	-0.052	0.219	55,804
Leverage	0.418	0.198	0.915	0.011	0.409	62,805
Tangibility	0.257	0.159	0.255	0.055	0.400	62,975
CAPX	0.051	0.028	0.071	0.011	0.060	62,547
R&D	0.088	0.000	0.216	0.000	0.071	63,017
CASH	0.227	0.115	0.262	0.029	0.339	63,007
CL-Goodwill	0.008	0.000	0.087	0.000	0.000	66,948
CL-SEC	0.009	0.000	0.097	0.000	0.000	66,948

Panel B: Comparison between firms with expert use disclosure and firms without

	Variables	Mean	Median	St. Dev.	25th PCTLE	75th PCTLE	Obs.
Non-Expert	Goodwill Impairment	-0.008	0.000	0.115	0.000	0.000	65711
	Goodwill	0.084	0.000	0.139	0.000	0.121	65711
	LnSize	4.890	5.145	2.916	3.119	6.981	61781
	Market-to-Book	1.948	1.607	10.381	0.761	3.158	54074
	ROA	-0.363	0.081	1.618	-0.099	0.143	61359
	Dummy Loss	0.316	0.000	0.465	0.000	1.000	65711
	Growth	0.243	0.068	1.089	-0.051	0.221	54577
	Leverage	0.421	0.198	0.923	0.010	0.409	61576
	Tangibility	0.258	0.160	0.256	0.054	0.403	61739
	CAPX	0.051	0.028	0.071	0.011	0.061	61312
	R&D	0.089	0.000	0.217	0.000	0.071	61781
	CASH	0.228	0.115	0.263	0.029	0.341	61771
Expert	Goodwill Impairment	-0.031	0.000	0.138	-0.005	0.000	1237
	Goodwill	0.181	0.158	0.159	0.044	0.274	1237
	LnSize	6.030	6.066	1.935	4.618	7.361	1236
	Market-to-Book	1.881	1.393	4.673	0.778	2.423	1175
	ROA	0.033	0.086	0.284	0.023	0.136	1234
	Dummy Loss	0.214	0.000	0.410	0.000	0.000	1237
	Growth	0.084	0.040	0.589	-0.065	0.141	1227
	Leverage	0.281	0.216	0.307	0.049	0.415	1229
	Tangibility	0.206	0.141	0.189	0.061	0.293	1236
	CAPX	0.036	0.026	0.034	0.014	0.047	1235
	R&D	0.039	0.000	0.098	0.000	0.034	1236
	CASH	0.158	0.095	0.174	0.026	0.225	1236

Panel C: Expert use disclosure frequency by year and industry (Fama-French 17 Industries classification)

Year	Freq.	Percent
2002	147	11.88
2003	169	13.66
2004	160	12.93
2005	116	9.38
2006	110	8.89
2007	62	5.01
2008	86	6.95
2009	93	7.52
2010	76	6.14
2011	77	6.22
2012	77	6.22
2013	64	5.17
Total	1,237	100

Fama-French industry code (17 industries)	Freq.	Percent
Food	24	1.94
Mining and Minerals	6	0.49
Oil and Petroleum Products	27	2.18
Textiles, Apparel & Footware	13	1.05
Consumer Durables	20	1.62
Chemicals	46	3.72
Drugs, Soap, Prfums, Tobacco	57	4.61
Construction and Construction Materials	47	3.8
Steel Works Etc	28	2.26
Fabricated Products	21	1.7
Machinery and Business Equipment	129	10.43
Automobiles	28	2.26
Transportation	57	4.61
Utilities	21	1.7
Retail Stores	34	2.75
Other	679	54.89
Total	1,237	100

Table 2 – Panel A: Goodwill Impairment and Expert Usage Disclosure with Firm-Fixed Effects

This table presents the coefficient estimates from OLS regressions. All firm level data are from the COMPUSTAT industrial database. The expert usage disclosure data are gathered from 10-K filings from SEC Edgar database for the period from 2002 to 2013. The sample excludes financial firms (SICs from 6000 to 6999). Refer to Table 1 for detailed variable definitions. Standard errors reported in parentheses are robust and clustered at the firm level.

Dependent variable:	Goodwill Impairment				
	(1)	(2)	(3)	(4)	(5)
Expert	-0.022***	-0.024***			-0.026***
	(0.006)	(0.006)			(0.007)
L.Expert			-0.003		0.006*
			(0.003)		(0.004)
L2.Expert				0.003	0.001
				(0.002)	(0.002)
L.W1ImpairGW		-0.179***	-0.179***	-0.167***	-0.167***
		(0.049)	(0.049)	(0.049)	(0.049)
Goodwill		0.070***	0.070***	0.070***	0.070***
		(0.016)	(0.016)	(0.015)	(0.015)
LnSize	-0.001	-0.003**	-0.004**	-0.004***	-0.004**
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Market-to-Book	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ROA	0.000	0.001	0.001	0.002	0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Dummy Loss	-0.007***	-0.008***	-0.008***	-0.007***	-0.007***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Growth	-0.003**	-0.003*	-0.003*	-0.003*	-0.003*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Leverage	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Tangibility	-0.009	0.003	0.003	0.004	0.004
	(0.008)	(0.010)	(0.010)	(0.010)	(0.010)
CAPX	-0.012	-0.011	-0.010	-0.013	-0.013
	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
R&D	0.012	0.017	0.017	0.015	0.015
	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)
CASH	-0.001	0.015	0.015*	0.013	0.013
	(0.007)	(0.009)	(0.009)	(0.008)	(0.008)
Firm-Fixed Effect	Yes	Yes	Yes	Yes	Yes
Year-Fixed Effect	Yes	Yes	Yes	Yes	Yes
Obs.	49,931	49,931	49,931	49,550	49,550
R-2 (within)	0.006	0.049	0.048	0.045	0.046

Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Table 2 – Panel B: Goodwill Impairment and Expert Usage Disclosure with Industry Fixed Effects

This table presents the coefficient estimates from OLS regressions. All firm level data are from the COMPUSTAT industrial database. The expert usage disclosure data are gathered from 10-K filings from SEC Edgar database for the period from 2002 to 2013. The sample excludes financial firms (SICs from 6000 to 6999). Refer to Table 1 for detailed variable definitions. Standard errors reported in parentheses are robust and clustered at the firm level.

Dependent variable:	Goodwill Impairment				
	(1)	(2)	(3)	(4)	(5)
Expert	-0.022***	-0.020***			-0.027***
	(0.004)	(0.004)			(0.006)
L.Expert			-0.005**		0.012**
			(0.002)		(0.005)
L2.Expert				-0.004*	-0.001
				(0.002)	(0.002)
L.Goodwill Impairment		0.044*	0.044*	0.045*	0.045*
		(0.024)	(0.024)	(0.024)	(0.024)
Goodwill		-0.014**	-0.015**	-0.015**	-0.014**
		(0.007)	(0.007)	(0.007)	(0.007)
LnSize	0.000	0.001**	0.001**	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Market-to-Book	0.000**	0.000**	0.000**	0.000*	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ROA	0.005***	0.005***	0.005***	0.004**	0.004**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Dummy Loss	-0.012***	-0.012***	-0.012***	-0.012***	-0.011***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Growth	-0.006***	-0.006***	-0.006***	-0.006***	-0.006***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Leverage	0.002	0.002	0.002	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Tangibility	0.014***	0.010**	0.010**	0.007**	0.007**
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
CAPX	-0.012	-0.013	-0.013	-0.008	-0.008
	(0.018)	(0.018)	(0.018)	(0.014)	(0.014)
R&D	0.024***	0.023***	0.023***	0.021***	0.021***
	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
CASH	0.018***	0.014***	0.014***	0.013***	0.013***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Industry-Fixed Effect	Yes	Yes	Yes	Yes	Yes
Year-Fixed Effect	Yes	Yes	Yes	Yes	Yes
Obs.	49,931	49,931	49,931	49,550	49,550
R-2	0.022	0.025	0.024	0.024	0.025

Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Table 3 – Determinants of Goodwill Impairment: Logit estimation

This table presents the coefficient estimates from Logit regressions. All firm level data are from the COMPUSTAT industrial database. The expert usage disclosure data are gathered from 10-K filings from SEC Edgar database for the period from 2002 to 2013. Dummy Impairment is equal to one if firm reports goodwill impairment at that fiscal year, zero elsewise. The sample excludes financial firms (SICs from 6000 to 6999). Refer to Table 1 for detailed variable definitions. Standard errors reported in parentheses are robust and clustered at the firm level.

Dependent variable:			Dummy Impairment		
	(1)	(2)	(3)	(4)	
Expert	1.404***	1.378***			1.422***
	(0.087)	(0.086)			(0.107)
L.Expert			0.905***		-0.077
			(0.099)		(0.139)
L2.Expert				0.622***	-0.025
				(0.109)	(0.127)
L.Goodwill Impairment		-0.789***	-0.788***	-0.807***	-0.790***
		(0.105)	(0.109)	(0.110)	(0.105)
Goodwill		0.404**	0.473***	0.517***	0.420***
		(0.159)	(0.157)	(0.158)	(0.160)
LnSize	0.191***	0.186***	0.188***	0.189***	0.186***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Market-to-Book	-0.015***	-0.015***	-0.015***	-0.015***	-0.015***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
ROA	-0.023	-0.007	-0.006	-0.003	-0.005
	(0.026)	(0.026)	(0.026)	(0.027)	(0.027)
Dummy Loss	1.002***	0.996***	1.003***	1.007***	0.995***
	(0.054)	(0.054)	(0.054)	(0.055)	(0.055)
Growth	-0.109***	-0.119***	-0.121***	-0.126***	-0.120***
	(0.029)	(0.029)	(0.029)	(0.031)	(0.030)
Leverage	0.061*	0.063*	0.062*	0.065*	0.067*
	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)
Tangibility	-1.128***	-0.978***	-0.976***	-0.983***	-0.983***
	(0.119)	(0.129)	(0.128)	(0.129)	(0.130)
CAPX	-1.260***	-1.264***	-1.306***	-1.284***	-1.227***
	(0.420)	(0.419)	(0.418)	(0.421)	(0.422)
R&D	-1.270***	-1.233***	-1.243***	-1.253***	-1.238***
	(0.178)	(0.178)	(0.180)	(0.182)	(0.180)
CASH	-1.726***	-1.606***	-1.604***	-1.597***	-1.597***
	(0.116)	(0.121)	(0.121)	(0.121)	(0.121)
Year-Fixed Effect	Yes	Yes	Yes	Yes	Yes
Obs.	49,931	49,931	49,931	49,550	49,550
Pseudo R-2	0.087	0.090	0.083	0.080	0.090

Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Table 4 – Determinants of Goodwill Impairment: Cox Proportional Hazard Model

This table presents the coefficient estimates from Cox Proportional Hazard Model regressions. We categorize as failure event the year in which a firm reports goodwill impairment. To construct our sample, we use all firms that haven't reported goodwill impairment in the last 5 years. Once a firm disclosure expert usage, the firms drop out of the sample. We allow a firm to re-enter the sample after the firm has disappeared for at least 5 years. The expert usage disclosure data are gathered from 10-K filings from SEC Edgar database for the period from 2002 to 2013. Dummy Impairment is equal to one if firm reports goodwill impairment at that fiscal year, zero elsewise. The sample excludes financial firms (SICs from 6000 to 6999). All firm level data are from the COMPUSTAT industrial database. Refer to Table 1 for detailed variable definitions. Standard errors reported in parentheses are robust and clustered at the firm level.

Dependent variable:	Dummy Impairment				
	(1)	(2)	(3)	(4)	
Expert	1.311***	1.312***			1.448***
	(0.103)	(0.103)			(0.110)
L.Expert			0.786***		-0.226
			(0.137)		(0.194)
L2.Expert				0.358*	-0.293
				(0.191)	(0.242)
Goodwill		-0.026	0.177	0.158	0.078
		(0.171)	(0.170)	(0.174)	(0.176)
LnSize	0.164***	0.165***	0.165***	0.170***	0.165***
	(0.012)	(0.013)	(0.012)	(0.012)	(0.012)
Market-to-Book	-0.017***	-0.017***	-0.017***	-0.016***	-0.016***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
ROA	-0.023	-0.023	-0.033	-0.036	-0.037
	(0.034)	(0.034)	(0.034)	(0.035)	(0.035)
Dummy Loss	0.865***	0.864***	0.941***	0.933***	0.906***
	(0.065)	(0.065)	(0.065)	(0.067)	(0.066)
Growth	-0.195***	-0.194***	-0.196***	-0.201***	-0.195***
	(0.055)	(0.055)	(0.058)	(0.064)	(0.064)
Leverage	-0.023	-0.023	0.010	0.026	0.030
	(0.043)	(0.043)	(0.043)	(0.045)	(0.045)
Tangibility	-0.908***	-0.916***	-0.958***	-0.960***	-0.934***
	(0.126)	(0.137)	(0.143)	(0.148)	(0.149)
CAPX	-1.095**	-1.095**	-0.718	-1.035*	-0.976*
	(0.494)	(0.495)	(0.513)	(0.554)	(0.554)
R&D	-0.751***	-0.750***	-0.944***	-0.991***	-0.976***
	(0.206)	(0.207)	(0.217)	(0.232)	(0.224)
CASH	-1.862***	-1.868***	-1.663***	-1.637***	-1.603***
	(0.140)	(0.147)	(0.148)	(0.155)	(0.154)
Obs.	32,095	32,095	30,072	28,109	28,109
Wald chi-2	822.33	822.83	669.88	593.84	840.23

Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively

Table 5 – Goodwill Impairment and Expert Usage Disclosure: Instrumental Variable

This table presents the coefficient estimates from Instrumental Variable regressions. All firm level data are from the COMPUSTAT industrial database. The expert usage disclosure data are gathered from 10-K filings from SEC Edgar database for the period from 2002 to 2013. The sample excludes financial firms (SICs from 6000 to 6999). CL-Goodwill is a dummy which equals to one if the firm receives comment letter regarding to current year goodwill impairment, zero elsewise. CL-SEC is a dummy equals to one if the firm receives any comment letter from SEC, zero elsewise. Refer to Table 1 for detailed variable definitions. Standard errors reported in parentheses are robust and clustered at the firm level.

Dependent variable:	Goodwill Impairment			
Instrumental Variable	CL-Goodwill	L.CL-Goodwill	CL-SEC	L.CL-SEC
	(1)	(2)	(3)	(4)
Expert	-0.793**	-0.706*	-0.922***	-0.559*
	(0.393)	(0.395)	(0.328)	(0.301)
L.Goodwill Impairment	0.026**	0.029**	0.023**	0.033***
	(0.012)	(0.012)	(0.011)	(0.009)
Goodwill	0.036	0.030	0.045*	0.020
	(0.028)	(0.028)	(0.023)	(0.021)
LnSize	0.003**	0.002**	0.003***	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)
Market-to-Book	0.000***	0.000***	0.000**	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
ROA	0.005***	0.005***	0.005***	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
Dummy Loss	-0.006*	-0.007**	-0.005	-0.008***
	(0.003)	(0.003)	(0.003)	(0.003)
Growth	-0.007***	-0.007***	-0.008***	-0.007***
	(0.001)	(0.001)	(0.001)	(0.001)
Leverage	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Tangibility	0.002	0.003	0.000	0.005
	(0.007)	(0.007)	(0.006)	(0.005)
CAPX	-0.034**	-0.032**	-0.038**	-0.028**
	(0.017)	(0.016)	(0.017)	(0.014)
R&D	0.016***	0.017***	0.015***	0.018***
	(0.006)	(0.005)	(0.006)	(0.005)
CASH	0.002	0.003	0.000	0.005
	(0.007)	(0.006)	(0.006)	(0.005)
Year-Fixed Effect	Yes	Yes	Yes	Yes
Obs.	49,931	49,931	49,931	49,931
Wald chi-2	434.25	491.49	365.01	608.94

Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Table 6: Panel A – Goodwill Impairment and Expert Usage Disclosure: Matching Sample

This table presents the coefficient estimates from Instrumental Variable regressions. All firm level data are from the COMPUSTAT industrial database. The expert usage disclosure data are gathered from 10-K filings from SEC Edgar database for the period from 2002 to 2013. The sample excludes financial firms (SICs from 6000 to 6999). Refer to Table 1 for detailed variable definitions. Standard errors reported in parentheses are robust and clustered at the firm level. For each target firm, we construct the matching observation from non-target firms using log of Size, Goodwill, Market-to-Book and two digit SIC code.

Dependent variable:	Goodwill Impairment			
	(1)	(2)	(3)	(4)
Expert	-0.013***	-0.016***	-0.022***	-0.022***
	(0.005)	(0.005)	(0.005)	(0.005)
L.W1ImpairGW		-0.333***		0.008
		(0.110)		(0.018)
Goodwill		0.211***		-0.010
		(0.045)		(0.026)
LnSize	-0.021***	-0.028***	0.004*	0.004
	(0.007)	(0.009)	(0.002)	(0.003)
Market-to-Book	0.001	0.001	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
ROA	-0.059*	-0.053	0.016	0.015
	(0.031)	(0.034)	(0.011)	(0.011)
Dummy Loss	-0.022**	-0.026**	-0.016	-0.016
	(0.010)	(0.010)	(0.012)	(0.012)
Growth	0.003	0.005	-0.015	-0.015
	(0.006)	(0.006)	(0.012)	(0.012)
Leverage	-0.019	-0.024	-0.012	-0.012
	(0.023)	(0.022)	(0.012)	(0.012)
Tangibility	-0.113**	-0.003	-0.008	-0.012
	(0.045)	(0.048)	(0.015)	(0.021)
CAPX	0.016	0.066	-0.037	-0.038
	(0.125)	(0.114)	(0.086)	(0.083)
R&D	-0.154*	-0.171**	0.030	0.029
	(0.081)	(0.078)	(0.035)	(0.035)
CASH	-0.023	0.066**	-0.002	-0.006
	(0.030)	(0.031)	(0.015)	(0.016)
Firm-Fixed Effect	Yes	Yes	No	No
Industry-Fixed Effect	No	No	Yes	Yes
Year-Fixed Effect	Yes	Yes	Yes	Yes
Obs.	2,237	2,237	2,237	2,237
R-2	0.084	0.244	0.097	0.097

Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.

Panel B - ATE (Average Treatment Effects) estimation by Nearest Neighborhood Matching

Goodwill Impairment	**coef.**	**Robust Std. Err**	**z**	**P>|z|**	**observations**
ATE Expert (1 vs 0)	-0.028***	0.006	-4.57	0.000	49,931

Table 7 – Debt and Valuation Expert

This table presents the coefficient estimates from OLS regressions. All firm level data are from the COMPUSTAT industrial database. The expert usage disclosure data are gathered from 10-K filings from SEC Edgar database for the period from 2002 to 2013. TDoverASSETS is defined as the total debt divided by total assets ((dltt + dlc)/at). LTDoverASSETS is defined as long term debt over total assets (dltt/at). STDoverASSETS is defined as short term debt over total assets (dlc/at). LTUoverASSETS is defined as long term unsecured debt over total assets ((dltt -dm)/at). CDoverASSETS is defined as convertible debt over total assets (dcvt/at). The sample excludes financial firms (SICs from 6000 to 6999). Refer to Table 1 for detailed variable definitions. Standard errors reported in parentheses are robust and clustered at the firm level.

Dependent variable:	**TDoverASSETS**	**LTDoverASSETS**	**STDoverASSETS**	**LTUDoverASSETS**	**CDoverASSETS**
Expert	-0.019*	-0.012	-0.003	-0.007	-0.001
	(0.011)	(0.008)	(0.007)	(0.006)	(0.003)
L.Expert	-0.021*	-0.003	-0.012	-0.009	-0.003
	(0.012)	(0.008)	(0.008)	(0.006)	(0.003)
LnSize	-0.027***	0.010***	-0.031***	0.019***	0.001*
	(0.004)	(0.001)	(0.002)	(0.001)	(0.000)
Market-to-Book	-0.002***	-0.002***	-0.001*	-0.001***	-0.000***
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
ROA	-0.394***	-0.029***	-0.273***	-0.027***	-0.007***
	(0.013)	(0.004)	(0.009)	(0.002)	(0.001)
Growth	-0.016***	0.001	-0.010***	0.000	0.001**
	(0.004)	(0.001)	(0.003)	(0.001)	(0.001)
Tangibility	0.199***	0.175***	0.026	0.022*	-0.000
	(0.043)	(0.019)	(0.028)	(0.012)	(0.006)
CAPX	-0.612***	-0.188***	-0.347***	-0.041	0.016
	(0.098)	(0.039)	(0.067)	(0.026)	(0.013)
R&D	0.011	0.108***	-0.099*	0.053***	0.020**
	(0.081)	(0.021)	(0.055)	(0.013)	(0.008)
CASH	-0.546***	-0.220***	-0.267***	-0.078***	-0.010**
	(0.034)	(0.011)	(0.023)	(0.007)	(0.005)
Industry-Fixed Effect	Yes	Yes	Yes	Yes	Yes
Year-Fixed Effect	Yes	Yes	Yes	Yes	Yes
Obs.	49,931	49,944	49,952	46,618	49,794
R-2	0.453	0.183	0.469	0.143	0.051

Note: ***, ** and * indicate statistical significance at the 1%, 5%, and 10% (two-tail) test levels, respectively.