# A Run-down of Merger Target Run-ups

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*December 6, 2017*

**Abstract**

We show that run-ups in U.S. target firm stock returns preceding merger announcements have declined drastically over the past decades. Whilst median pre-announcement target run-ups historically constituted approximately half of the overall target stock price effects, this fraction has dropped to approximately 17% over the period 2010 to 2016, with the median 20-day run-up before the announcement falling to approximately 2%. The negative trend in target run-ups goes along with an increasing trend in announcement-period target stock returns. The decline in target run-ups cannot be fully explained by changes in deal or target characteristics associated with merger anticipation. However, it disappears after controlling for changes in the strength of U.S. insider trading regulation over the research period. Our results suggest that insider trading rules can substantially curb pre-announcement trading.

Keywords: target run-up, mergers and acquisitions, merger prediction, insider trading

## 1. Introduction

It is well-established in the corporate finance literature that target firm stock prices tend to increase substantially before merger and acquisition (M&A) announcements. Studies on this topic consistently find positive average pre-announcement target abnormal stock returns in the area of 10%, representing approximately half of the total M&A stock price effect for target firms (Dodd, 1980; Keown and Pinkerton, 1981; Dennis and McConnell, 1986; Meulbroek, 1992; Schwert, 1996).

In this paper, we document that U.S. target run-ups have been declining steadily and significantly since the research period covered by most previous studies on this topic. While we observe run-ups similar in magnitude to other studies for the early 1980s, the median target run-up over the final years covered by our study, 2010 to 2016, is only 2.11%. We further find that the significant decline in target run-ups goes along with a significant increase in target abnormal stock returns around merger announcement dates. Total (pre-announcement plus announcement) stock price effects of M&A deals for target firms have remained unchanged over time.

We next examine whether the decrease in target run-ups can be attributed to changes in deal and firm characteristics associated with the degree to which market participants can anticipate mergers, as per the 'merger anticipation' explanation for pre-merger announcement stock returns (Asquith, 1983; Jensen and Ruback, 1983). Our analysis includes a wide range of merger anticipation proxies suggested by the literature (Jarrell and Poulsen, 1989; Palepu, 1990; Espahbodi and Espahbodi, 2003). The negative trend in target run-ups persists when we split deals according to these characteristics. To note a few examples, we find that the trend is not driven by a higher occurrence of deals without pre-deal rumors, deals with single bidders, or deals without pre-bid toehold stakes in recent years. Similarly, the negative trend holds across targets with high and low market to book ratios, free cash flows, and firm size.

Regressions of target run-ups consistently show a negative time trend, even when including merger anticipation proxies. Moreover, predicted target run-ups generated by a regression model with deal- and target-specific variables systematically fail to fully capture the decrease in actual run-ups. Thus, we conclude that changes in deal and firm characteristics cannot explain the decline in target run-ups.

Subsequently, we examine whether the decline in target run-ups can be attributed to the increasingly stringent insider trading regulations adopted in the U.S. over the research window. These rules might have curbed insiders' potential to perform illegal trades on knowledge about the upcoming deal (Keown and Pinkerton, 1981; Meulbroek, 1992). We focus on five important changes in insider trading regulation over the research period. We find that, after controlling for the adoption of these regulatory changes, the time trend in the run-ups is no longer significant. We obtain similar results when using abnormal trading volumes instead of target run-ups as the dependent variable of our regression analyses.

Our findings are robust to different measurements of target run-ups and are not an artifact of changes in takeover premiums over time. In fact, we do not find any significant trend in premiums over our research period. We also conduct a placebo test on a sample of Canadian M&A deals, which have much less stringent insider trading rules (Bris, 2005). The results of this test, which detects no effect of U.S. regulations on target run-ups for these foreign deals, further corroborate our conclusion that the decline in target run-ups is associated with U.S. insider trading laws.

Our results are relevant for policy makers and regulators. Pre-merger trading based on legitimate research and analysis of corporate news is beneficial to the extent that it aligns stock prices with their theoretically correct values, thereby allowing capital to be efficiently allocated (Jarrell and Poulsen, 1989). Under this 'merger anticipation' viewpoint, target run-ups are not a cause for concern as they merely encourage the active gathering and processing

of information by skilled corporate outsiders such as arbitrageurs. By contrast, pre-merger trading by corporate insiders represents a significant legal problem, as it gives these parties an unfair advantage over the rest of the market (Pound and Zeckhauser, 1990).[1] Although regulators routinely examine abnormal trading patterns in target firms' stock, establishing the presence of illegal insider trading by analysing trading volumes or abnormal stock returns is a difficult and often impossible task (Minenna, 2003). The key problem is that the occurrence of abnormal trading volumes and abnormal stock returns does not necessarily imply illegal activity, as it could be caused by informed traders exploiting legally obtained information. Exploiting time-series variation in target run-ups enables us to verify the relative importance of merger anticipation versus insider trading explanations. Our results suggest that target run-ups result from privileged insider trading rather than from merger anticipation based on publicly available information. However, reassuringly, insider trading rules seem increasingly effective at curbing these illegal practices.

The remainder of the paper is structured as follows. Section 2 positions our work within the literature and outlines its contributions. Section 3 describes the sample construction and measurement of target run-ups, and documents the trend in run-ups over our sample period. Section 4 explores potential explanations for this trend. Section 5 discusses a number of robustness tests. Section 6 concludes.

## 2. Literature review

The literature proposes two non-mutually exclusive explanations for pre-merger target stock returns. The 'merger anticipation' hypothesis argues that target run-ups are driven by legal trading from informed market participants such as proprietary traders and risk arbitrageurs, who expect that certain firms will be the target of an upcoming deal (Jensen and

[1] Some scholars, however, argue that insider trading is harmless and should not be penalized. Minenna (2003) provides an overview of the opposing views on insider trading.

Ruback, 1983). These traders' expectations regarding upcoming deals may be based on information in press releases, mandated disclosures, comments by 'shark watchers' and observations of activity by merger arbitrageurs (Jabbour et al., 2000).

The 'insider trading' hypothesis, in turn, argues that target run-ups are caused by illegal trading from corporate insiders who know about the upcoming M&A deal ahead of the rest of the market (Keown and Pinkerton, 1981). Insider trading is restricted by Sections 10 (b) and 14 (e) of the Securities Exchange Act of 1934, and Rules 10b-5 and 14e-3 thereunder. According to court interpretations of these rules, there are four essential requirements for trades to be qualified as illegal insider trading (Netter et al., 1988). First, the trade must be done by a corporate insider. In applying this rule, it is often difficult to determine who is a covered insider and thus who has this duty. Second, the insider must trade on material inside information. Information is material if there is a substantial likelihood that a reasonable investor would consider it important in his or her investment decision (Jarrell and Poulsen, 1989). The difference between material and non-material private information is also often unclear. Under the so-called mosaic theory, insider trading violations should not result when a perceptive analyst reaches a conclusion about a corporate action or event through an analysis of public information and items of nonmaterial non-public information (Doffou, 2003). Third, the transaction must involve a purchase or sale of a security. Finally, there must be evidence of an intention to deceive, manipulate, or defraud. Although the media regularly report about penalties given for high-profile insider trading offenses, academic studies find that insider trading often goes unpunished (Bris, 2005). Schwert (1996) argues that merger announcements are particularly challenging for regulatory authorities trying to curb illegal trading practices. The reasons are twofold. First, mergers typically involve significant price affecting information, increasing the potential profits associated with illegal trading. Second, their planning tends to involve a wide circle of people (e.g. lawyers, consultants, target firm

employees, and regulators), all of whom possess material inside information. Thus, target run-ups might be caused by illegal insider trading.

Empirical evidence for the two above explanations for target run-ups is mixed and inconclusive. Early studies mostly focus on testing one of the two explanations. Consistent with the merger anticipation hypothesis, Jarrell and Poulsen (1989) find that stock price run-ups and trading volumes before tender offer announcements are associated with several observable and legal factors, such as the presence of media rumors about an impending bid. They conclude that: "*to argue that pre-bid runups necessarily reflect insider trading (…) is a misrepresentation of the data."* Also in line with the merger anticipation hypothesis, Gupta and Misra (1989) find that stock run-ups for firms that are mentioned in the news as potential targets are larger than those for 'no news' firms. However, they acknowledge that they cannot completely rule out insider trading as a potential explanation for target run-ups.

For the insider trading hypothesis, Keown and Pinkerton (1981) allege that *"impending merger announcements are poorly held secrets, and trading on this non-public information abounds."* However, they do not formally test whether pre-announcement trading is effectively caused by corporate insiders. Meulbroek (1992) makes headway on this question by using SEC data on illegal insider trading. She finds that almost half of the pre-announcement target stock run-up occurs on days with insider trading. Jabbour et al. (2000) examine M&As in Canada, a country with more lenient insider trading restrictions and weaker enforcement procedures than the U.S. They find that early-stage abnormal target firm stock price performance can be explained by insider trading, while the run-up immediately preceding the takeover announcement appears due to market anticipation about an impending deal. King (2009) also finds evidence of insider trading in a Canadian setting.

Brigida and Madura (2012) examine the impact of a wide range of deal- and firm-specific characteristics on target run-ups. While they identify several drivers of run-ups, they

do not directly link them with the merger anticipation or insider trading explanation. Thus, overall, the literature is unclear on the relative importance of merger anticipation versus insider trading explanations for target run-ups.

One reason for the ambiguity in previous empirical results is the difficulty in distinguishing econometrically between informed trading by corporate outsiders based on legally obtained public information, and informed trading by corporate insiders based on private information. The identities of the informed traders are not known ex ante, and may not be known ex post (King, 2009). Without a model to distinguish among different types of informed trading, patterns in stock behaviour cannot be used to distinguish between the two hypotheses (Minenna, 2003).

Our paper contributes to the above literature by documenting and explaining the trend in target run-ups from the 1980s, the period covered by most previous studies, to 2016. Our long research window enables us to exploit the increasingly stringent insider trading regulation over time. In a recent paper, Del Guercio et al. (2017) document that enhanced Securities and Exchange Commission (SEC) enforcement efforts, as measured by fluctuations in the SEC's budget, are effective at deterring illegal insider trading around earnings announcements and takeover bids. We control for a wide range of deal- and firm-specific merger anticipation proxies suggested by the literature, allowing us to test the relative importance of merger anticipation and insider trading explanations for target run-ups.

Our study contributes to a stream of papers documenting and explaining trends in variables associated with corporate finance decisions and their outcomes. To name a few, Fama and French (2001) find that the proportion of firms paying dividends has decreased over time. Custódio et al. (2013) report an increasing trend in the use of short-term corporate debt. Both these studies attribute their findings to the fact that the percentage of young and small firms among U.S. publicly listed companies has increased over time. Autore et al.

(2008) find that motives for firms to choose shelf-registered over traditional seasoned equity offerings (SEOs) have shifted over their sample period, but do not explain this trend. Dissanaike et al. (2014) document a strong decrease in stock price reactions to SEOs, which they attribute to the effects of the Global Financial Crisis. Duca et al. (2012) find that stock price reactions to convertible bond announcements have also become more negative over time, and show that this is caused by the increasing involvement of convertible bond arbitrageurs as investors into these hybrid securities. Together, these studies suggest that, for a variety of firm-specific and macroeconomic reasons, commonly accepted truths about corporate finance decisions and outcomes dating from decades ago might no longer hold. Our work complements these papers by showing that existing assumptions about the magnitude of target stock run-ups, based on data from older studies, also need to be revised.

## 3. Sample construction and target run-ups

This section describes the data set of M&A deals, the measurement of target run-ups, and the evolution of abnormal stock returns for target firms over our sample period.

### *3.1. Sample*

The sample consists of M&As in which U.S.-domiciled, exchange-listed bidding firms acquire U.S.-domiciled, exchange-listed target firms. The research period is January 1985 to December 2016. We obtain M&A deals from Thomson ONE. For deals to be retained in the final sample, the following standard inclusion criteria need to apply (e.g. Fuller et al., 2002): the bidder purchases at least 50 percent of target shares, the deal value is at least 1% of the market value of the bidding firm, the market value of the target firm is at least $1 million, and the target firm's stock price data are available from the Center for Research in Security Prices (CRSP) over the year before the merger announcement. After imposing these standard

criteria, our sample consists of 5,946 M&A deals. Moreover, we eliminate deals with missing values for the deal- and target-specific merger anticipation proxies discussed in the next Section. After imposing these restrictions, we obtain a final sample consisting of 2,101 M&A deals.

### *3.2. Measurement of target run-ups*

We use market model regressions to estimate abnormal stock returns. We estimate the regressions over a period of 190 trading days ending at trading day –76 relative to the merger's announcement date retrieved from Thomson ONE. We use the CRSP equally weighted index as a proxy for the market portfolio. We define our main target run-up measure as the cumulative abnormal stock return over the 20 trading days prior to the merger announcement date. Several previous studies use a similar time window for calculating target run-ups (e.g., Dennis and McConnell, 1986; Jarrell and Poulsen, 1989). In robustness tests discussed further, we use alternative run-up measures suggested by the literature. We winsorize target run-ups at the 5% and 95% level to avoid the risk of outliers affecting the results.[2]

### *3.3. Trend in target run-ups*

Figure 1 plots median target run-ups over five five-year intervals within the research period. The sixth interval counts seven years as our sample period ends in 2016. In line with previous studies, the figure shows evidence of positive target abnormal stock returns before M&A announcement dates. However, run-ups clearly become substantially smaller in more recent intervals. Average run-ups (which we do not show for parsimony) show a similar trend.

[2] Stata software's "extremes" command indicates the presence of outliers in target run-ups, as identified by the interquartile range criterion.

<< Please insert Figure 1 here >>

Table 1 provides quantitative evidence of the evolution in target run-ups over time. It gives a breakdown of average and median run-ups over each of the six intervals. For the earliest time period (1985–1989), we register an average abnormal stock return of approximately 10%, consistent with the magnitude of target run-ups reported in prior studies (Keown and Pinkerton, 1981; Meulbroek, 1992; Schwert, 1996). Average target run-ups become smaller in each consecutive interval. In the most recent interval (2010–2016), we register average run-ups of only 3.54%. Median run-ups show a similar pattern, steadily dropping from 8.59% in the initial interval to 2.11% in the most recent interval. When regressing the run-ups on a yearly time trend variable and an intercept, we find a significant negative annual time trend of –0.29% for average run-ups, and –0.26% for median run-ups.

We also consider target run-ups over a longer pre-announcement window. Sixty trading days before the announcement seems to be the longest pre-merger window considered in the run-up literature, so we use this window as an alternative for our main run-up measure (Keown and Pinkerton, 1981; Brigida and Madura, 2002). As Table 1, Column (2) shows, we find a similar pattern for this run-up measure.

<< Please insert Table 1 here >>

Column (3) of Table 1 provides target announcement returns. We measure these over the merger announcement date and the subsequent trading day, to account for M&A announcements that may have been made after stock market closure or on a non-trading day. We find that announcement returns show a positive time trend. Average (median) returns monotonically increase from 16.55% (12.66%) over the period 1985–1989 to 24.78% (21.89%) over the period 2010–2016. Combined with the findings in Columns (1) and (2), this pattern suggests that the decline in the pre-merger run-ups is compensated by an increase in announcement-period returns. In line with this insight, Column (4) shows that total (pre-

announcement plus announcement-period) stock price effects for target firms remain largely similar over time. Accordingly, we do not find a significant time trend for the average or median total returns. Column (5) reports similar findings for an extended measurement period ranging over sixty pre-announcement trading days.

Some studies consider run-up index measures, constructed as the ratio of target run-ups to total M&A-related stock price effects (Jarrell and Poulsen, 1989; Bris, 2005). We present run-up index ratios in Columns (6) and (7). Consistent with the results reported in the previous columns, we find that the proportion of pre-merger run-ups relative to total stock price effects steadily decreases over time, with a statistically significant negative time trend both for average and median ratios. More particularly, while we find a ratio of approximately 40% on average at the start of our research period, which is consistent with previous studies (Jarrell and Poulsen, 1989; Meulbroek, 1992), we find that the ratio drops below 20% towards the end of our research period.

## 4. Potential explanations for the trend in target run-ups

This section examines potential reasons for the significant decline in target run-ups over the past decades. We start by exploring the merger anticipation hypothesis and then turn to changes in insider trading as a potential explanation.

### *4.1. Merger anticipation hypothesis*

According to the merger anticipation hypothesis, target run-ups are caused by legal trading activity by market participants who can predict targets of upcoming deals. Under this viewpoint, the decline in target run-ups may result from a reduced ability of informed traders to anticipate future targets based on publicly available information. This reduction in target predictability might result from a variety of factors. Acquirers may have become more skilled

to prevent unintentional leaks about their takeover plans. Deal characteristics linked with deal anticipation, such as pre-bid toehold stakes and bid financing might have changed over time, making takeovers less predictable. Finally, it may have become less straightforward to predict upcoming merger targets based on their firm characteristics. Previous studies on M&A target prediction typically find that firms with inefficient management, low investment opportunities, and a smaller size are more likely to be acquired (Palepu, 1990; Song and Walkling, 2000; Espahbodi and Espahbodi, 2003), consistent with agency cost reduction motives for mergers (Jensen, 1986). However, merger motives change over time (Bruner, 2004), potentially making these traditional merger prediction models less capable of predicting targets. Of course, market participants nowadays have a wealth of information and sophisticated prediction tools at their fingertips, but it might still have become more difficult for them to weed out relevant pieces of information and accurately predict target firms.

To examine this possibility, we analyze whether the trend in target run-ups persists after controlling for variables associated with a higher likelihood of being perceived as a target firm. We call these variables 'merger anticipation proxies'. The literature suggests several merger anticipation proxies (Jarrel and Poulsen, 1989; Palepu, 1990; Espahbodi et al., 2003; King, 2009; Brigida and Madura, 2012). We now motivate each of these proxies. The Appendix provides a detailed description of their sources and definition. Consistent with our target run-up measure, we winsorize all continuous merger anticipation proxies at the 5% and 95% level.

First, we include the following deal characteristics associated with an increased probability of a merger:

- *Rumor*: a dummy variable capturing whether there have been media rumors about the deal before its official announcement. Deals preceded by rumors are likely to be more anticipated by the market (Gupta and Misra, 1989).

- *Toehold*: a dummy variable equal to one when the bidder has an equity stake in the target firm before the merger announcement. A pre-merger equity stake may signal a merger intention (Betton et al., 2008; Brigida and Madura, 2012). On the other hand, a toehold stake tends to decrease the remaining target shareholders' takeover gains, which might reduce the probability of an eventual takeover (Stulz et al., 1990). The impact of the *Toehold* dummy variable on the likelihood of a deal is therefore unclear.
- *Cash financing*: measures the percentage of cash financing for the target firm. Cash-financed deals are typically more anticipated (Brigida and Madura, 2012). The reason is that firms often have to raise debt financing to obtain the cash payments required for a deal.
- *Hostile* dummy variable: Hostile bidders typically try to make the bid a surprise, in order to reduce the target's potential to activate takeover defenses. In comparison to a hostile bid, a friendly bid tends to involve negotiations and considerable sharing of information between the two firms involved and their advisors, increasing the likelihood that the market will learn about the deal. On the other hand, some hostile bidders attempt to place target firm stocks in the hands of arbitrageurs, who are typically more willing to tender their shares. Under this scenario, hostile bids might be more anticipated than friendly bids. The relation between the hostile nature of bids and their anticipation is therefore unclear (Jarrel and Poulsen, 1989).
- *Poison pill* dummy variable: Firms with poison pill takeover defenses in place may be less likely to become a target (Malatesta and Walkling, 1988), although Comment and Schwert (1995) show that poison pill rights typically do not deter takeovers.
- *Number of bidders*: If more firms are interested in the target, the deal is more likely to be anticipated.

- *Industry activity*: Dummy variable equal to one if at least one acquisition occurred in a firm's industry during the year prior to the M&A deal (Palepu, 1990). M&A activity typically clusters within certain industries (Mitchell and Mulherin, 1996). Therefore, recent M&As in a firm's industry can act as a signal that the firm might also end up as a target (Song and Walkling, 2000).

Based on the merger anticipation hypothesis, we expect target run-ups to be higher for deals that the market is more likely to anticipate. Thus, we predict a positive impact of *Rumor*, *Cash financing*, *Number of bidders*, and *Industry activity*, and a negative impact of *Poison pill* on target run-ups, while we have no clear expectations for the impact of *Toehold* and *Hostile*. In addition to these deal-specific merger anticipation proxies, we also control for the *Target-bidder* ratio, which captures the relative market values of target and bidder firms, as in Alexandridis et al. (2013). We do not have clear expectations for the sign of this variable.

Table 2, Panel A reports the median value of our main target run-up measure for our data set split in two subsamples according to the above deal characteristics. For binary deal characteristics, the split is straightforward. For continuous deal characteristics, we split the sample into subsamples based on their median value.

<< Please insert Table 2 here >>

Column (1) creates subsamples using deals over the entire sample period. The values in italics printed below the full-period target run-up medians represent differences in median target run-up values between each pair of subsamples based on deal characteristics.[3] Superscripted asterisks indicate the significance of (unreported) Wilcoxon test statistics for the differences in full-period subsample medians.

[3] In unreported tests, we obtain similar results when focusing on averages.

The first variable that we consider is the *Rumor* dummy variable equal to one for deals about which there are pre-announcement rumors in the market. We find significantly larger run-ups for rumored deals over the entire research period, consistent with the intuition that such deals are more anticipated by the market. Also consistent with our prediction based on merger anticipation, we obtain significantly higher run-ups for deals with a higher *Number of bidders*. We find smaller run-ups, in turn, for deals with a larger *Target-bidder ratio*. We find no significant differences in run-ups when splitting the sample according to the other deal characteristics.

The main aim of this univariate analysis is to establish whether the significant time trend in run-ups persists across subsamples based on various deal characteristics associated with merger anticipation. We find that this is the case. In Columns (2) to (7), we report median target run-ups for subsamples based on median deal characteristics in six subperiods within the overall sample period. We find a declining trend in each of the separate subsamples. As a more formal test, we regress target run-ups on an annual time trend variable and an intercept for each of the individual subsamples. Column (8) shows that we find a significant negative trend in target run-ups across each of the subsamples that we consider. We conclude that the decline in pre-merger target run-ups over time seems robust across mergers with different deal characteristics.

The literature also suggests a number of target characteristics associated with an increased likelihood of a takeover. We therefore include the following firm-specific determinants:

- *Market-to-book:* Firms with low market-to-book ratios are often perceived as cheap, even though the book value of assets does not necessarily reflect their replacement value (Palepu, 1990). Moreover, lower market-to-book ratios may indicate low growth opportunities, suggesting that shareholder value can be improved by being taken over

(Espahbodi and Espahbodi, 2003). A low market-to-book value should therefore be associated with a higher anticipated likelihood of becoming a target firm.

- *FCF:* This variable measures free cash flow over total assets. Firms with greater values for this ratio tend to have lower investment opportunities and more agency problems (Jensen, 1986). As such, these firms are more likely to be taken over by acquirers who spot room for improvement (Espahbodi and Espahbodi, 2003).
- *Dividend yield:* Firms with a high dividend yield typically have fewer growth opportunities, higher agency costs, and higher financial constraints, making them more likely to be targeted for an M&A deal (Espahbodi and Espahbodi, 2003).
- *Sales growth:* This variable acts as a measure of firm growth and should be negatively related to the odds of being targeted for a takeover (Espahbodi and Espahbodi, 2003).
- *Growth-resource (GR) mismatch:* This variable is inspired by the growth-resource imbalance hypothesis, which indicates that growth (as captured by changes in sales) and resource availability (as captured by leverage and cash reserves) are important drivers of a firm's likelihood of becoming a takeover target. More particularly, firms with high growth and low liquidity, or low growth and high liquidity, are more likely to be taken over (Palepu, 1990). The growth-resource mismatch dummy is equal to one for these firms, and equal to zero otherwise.
- *Leverage:* In addition to the growth-resource mismatch dummy variable, we also separately control for target leverage and liquidity (which we describe below). Firms with high leverage are typically less attractive as takeover targets, as an acquisition of low-debt firms is less costly to finance (Song and Walkling, 2000).
- *Liquidity:* Measured by cash reserves. Firms with higher cash reserves have a higher likelihood of being acquired, as there is an opportunity for the bidders to finance the

acquisition with the target's own resources (Song and Walkling, 1993; Espahbodi and Espahbodi, 2003).

- *Firm size:* Captures the size of the target firm, as measured by total assets converted into constant 1980s U.S. dollar using the U.S. Consumer Price Index obtained from Datastream. Smaller firms are more likely to end up as targets for an M&A deal (Song and Walkling, 2000).
- *Abnormal stock return:* Consistent with Palepu (1990), we use excess stock returns as a proxy for management efficiency. Firms managed by inefficient managers are more likely to be taken over. Hence, this variable has a negative predicted association with merger anticipation, as lower abnormal stock returns should reflect a higher likelihood of being acquired.
- *State*: Dummy variable equal to one for firms incorporated in Delaware. This state has the toughest anti-takeover laws in the U.S, which could potentially make its incumbent firms less likely to be targeted (Espahbodi and Espahbodi, 2003), although Comment and Schwert (1995) find only weak evidence for any mitigating impact of anti-takeover laws on takeover frequency.

Table 2, Panel B reports median target run-ups for subsamples split according to these target characteristics. For the full research window (1985–2016), we find higher run-ups for targets with a smaller *Leverage* and *Firm size*, consistent with the prediction that low-leverage, smaller firms are more likely to be identified as likely takeover targets. We also find higher run-ups for firms with a lower *Dividend yield*, inconsistent with our prediction. We find no significant differences in run-ups when splitting the sample according to other target characteristics potentially associated with merger anticipation. Again, we are mostly interested in investigating whether the negative trend in run-ups persists across different target subsamples. Column (8) shows strong evidence that this is the case. In each of the

subsamples based on target characteristics, we find a strong negative time trend in the pre-merger run-ups. Hence, a single target characteristic cannot explain the decline in pre-merger run-ups over time.

In addition to the above univariate tests, we conduct an ordinary least squares (OLS) regression in which we regress target run-ups on deal and target characteristics associated with merger anticipation. Table 3 presents the regression results. We follow a similar approach as Custódio et al. (2013) in their analysis of the decline in corporate debt maturity. In a first model, reported in Column (1), we include the merger anticipation proxies without time trend controls. Among the deal characteristics, *Rumor* has a significant impact with the predicted positive sign. We also find a negative impact for *Target-bidder ratio*, for which we had no clear expectations. For the target characteristics, we find a negative impact for *Firm size* and *Abnormal stock return*, in line with our predictions. In addition, we find a negative impact of *FCF*, inconsistent with the intuition that firms with higher free cash flows are more likely to be acquired, as they may have run out of profitable growth opportunities (Espahbodi and Espahbodi, 2003). The $R^2$ of the model is approximately 4%, which is a similar order of magnitude as the $R^2$s in the Brigida and Madura (2002) analysis of pre-merger run-ups.

<< Please insert Table 3 here >>

In Column (2), we include five dummy variables allowing the intercept to shift with respect to the first five-year interval considered in our analysis (1985–1989). We find a significant negative impact for the dummy variables capturing time periods after 2000. This result suggests that changes in deal- and target-specific merger anticipation proxies cannot fully account for the decline in target run-ups since the start of the 21st century. The coefficients for the final time periods have the greatest value in absolute terms, indicating that a larger part of the decline in run-ups is unexplained by the variables in the model during those intervals. With regards to the merger anticipation proxies, we now find a significant

impact for *Cash financing*, which was insignificant in the previous model. The positive sign of its coefficient is consistent with our prediction. The results for other merger anticipation proxies remain largely similar to those in Column (1). The model in Column (3) replicates the previous model including year dummies instead of period dummies. There are 31 year dummies in total, starting in 1986 and ending in 2016. We do not report the coefficients of individual year dummy variables for parsimony. We find that 21 out of the 31 year dummies have a significant negative coefficient, with the remainder having an insignificant impact. The *F*-test statistic for the significance of the year dummy variables is 4.77, strongly rejecting that the year dummy coefficients are jointly equal to zero ($p$-value < 0.001).

In the remaining analyses, we work with a time trend variable instead of period dummies, as a time trend variable makes it easier to quantify the magnitude of the annual decrease in pre-merger run-ups. In Column (4), we run a baseline regression in which we only include an annual time trend and an intercept. We find a significant decrease in pre-merger run-ups of 0.29% per year. In Column (5), we test whether this time trend persists when controlling for deal- and target-specific merger anticipation proxies. We find that this is the case. The coefficient of the time trend variable (–0.31%) remains negative and significant. We conclude that merger anticipation proxies are unable to explain the decline in pre-merger run-ups over time. In the next subsection, we explore the insider trading hypothesis as an alternative explanation.

### *4.2. Insider trading hypothesis*

The insider trading hypothesis argues that pre-merger target run-ups result from illegal trading activity by corporate insiders. It is very hard, even with sophisticated econometric models, to empirically disentangle illegal trading from legal trading by informed traders (Minenna, 2003). However, our long sample window enables us to exploit changes in U.S.

insider trading regulation over the past decades. U.S. insider trading rules have become increasingly stringent over time. Under the insider trading hypothesis, the decrease in target run-ups might be attributed to enhanced insider trading regulation, as traders increasingly fear detection and punishment. Based on a search of relevant literature, we identify five important potential increases in the strength of insider trading rules and their enforcement over our sample period. We start our search after 1985, as we do not have sufficient sample observations before that year.

A first important change in regulation occurs in 1988, with the Insider Trading and Securities Fraud Enforcement Act (Seitzinger, 2016). The Act is effective from November 1988 and expands the scope of civil penalties for persons who fail to take adequate steps to prevent insider trading. The Act specifies that investors suffering financial losses because of the misuse of non-public information have the right to take legal action against insider traders and explains that both the tipper and tippee can be penalized. The next large regulation change is effective from October 2000 and concerns the Selective Disclosure and Insider Trading Rules, which include Regulation Fair Disclosure (Reg FD). This regulation states that when an issuer or representative reveals nonpublic information to third parties, such as analysts or shareholders, the information must simultaneously be disclosed publicly. Gintschel and Markov (2004) show that Reg FD is effective in reducing the informativeness of analysts' information outputs. A third important change to regulation is the Sarbanes-Oxley (SOX) Act, effective from August 2003. While SOX was not primarily intended to reduce illegal leakages of information, it may have indirectly led to this effect by requiring executives to be more accountable for the information disclosed by their firm (Brigida and Madura, 2012). The SOX Act also increases penalties associated with insider trading. The fourth important change that we consider in our analysis relates to the enforceability of insider trading regulation, which increased substantially after the appointment of Mary

Schapiro as SEC Chairwoman in January 2009. Her appointment led to a strong increase in investigations, including the Galleon insider trading case later that year, which resulted in the arrest and sentencing of hedge fund manager Raj Rajaratnam. As the most recent change in insider trading regulation, Sietzinger (2016) highlights the Stop Trading on Congressional Knowledge (STOCK) Act, enacted on April 2012. This Act makes it clear that insider trading prohibitions apply to Members of Congress, congressional staff, and other federal officials. The STOCK Act also has provisions concerning financial disclosure reporting requirements for legislative and executive branch officials.

<< Please insert Figure 2 here >>

Figure 2 reports graphs in which we analyse median target stock run-ups only for those deals announced in 180-trading day windows before and after the enactment of each event associated with a strengthening of insider trading rules. We henceforth label these dates 'insider trading (IT) events'. We leave a 90-trading day gap after each IT event so that the run-ups of post-event deals do not include pre-event trading days. We calculate abnormal stock returns in the same way as for our main run-up measure, i.e. we use 20-day abnormal stock returns obtained through market model regressions. The median 20-day target run-up (i.e., until day –1) for merger bids announced during the 180-day period prior to the first regulation (19/11/1988) is 15.55%. The median target run-up falls to 8.57% for merger bids announced in the period ranging from trading days 90 up to 270 after the enactment of that first regulation. Similarly, median target run-ups around the second event (23/10/2000) declined from 8.68% for deals announced in the 180 days before, to 3.06% for deals announced in the 180 days after the enactment of the regulation. Following the third IT event on 14/08/2003, median run-ups declined from 3.73% to 1.31%. The fourth graph shows a very high median run-up prior to the 2009 IT event (11.03%), dropping down to 0.50% after the event. Median run-ups on yearly basis (not shown for parsimony) confirm that pre-merger

run-ups in 2008 tend to be abnormally high. One potential explanation is that the Global Financial Crisis may have led to stronger insider trading on private knowledge about upcoming mergers, in an attempt to compensate for losses occurring on other (legal) trades during that period. In the final graph, we do not find strong differences in median target run-ups for deals in the periods immediately prior versus after the 2012 IT event (medians of 4.03% versus 5.27%, respectively).

Together, these graphs provide a first indication that events associated with more stringent insider trading rules tend to be associated with declines in pre-merger target run-ups. To examine this conjecture more formally, we re-estimate the model in Column (5) of Table 3, adding dummy variables capturing each of the five IT events. Each IT event dummy variable has a value of zero before the relevant event, and one afterwards. Table 3, Column (6) reports the findings. We see that, except for the dummy variable associated with the very first regulatory change in November 1988, the remaining four IT event dummy variables have a strongly significant, negative coefficient. Most importantly, the time trend coefficient now has a positive sign and is no longer statistically significant following the introduction of the IT event dummies. In other words, no time trend in target run-ups remains in the periods in-between the regulation changes. Thus, our results suggest that stronger insider trading mitigation over time can explain the decline in pre-merger target run-ups. The findings regarding the impact of merger anticipation proxies on run-ups remain similar to those in previous columns.

## 5. Additional tests

This section discusses a number of additional tests to better understand the results in Section 4, as well as assess their robustness.

In a first additional test, we quantify the component of pre-merger run-ups that is not attributable to changes in deal- and target-specific characteristics. This analysis serves to obtain a better insight into the magnitude of the target run-up decline unexplained by merger anticipation proxies. In line with Custódio et al. (2013), we first estimate the regression in Table 3, Column (1) over an estimation window ranging from January 1, 1985 (the start of our research period) until October 22, 2000. We stop right before October 23, 2000 since this day coincides with the first IT event with a significant impact in Table 3, and we want to use an estimation sample that is relatively clean of key changes in insider trading rules.[4] The results of this regression are highly similar to those for the full sample in Table 3, Column (1). We use the coefficients of this regression to predict the run-ups for a holdout sample consisting of the remaining deals taking place from October 23, 2000 until the end of 2016. We then compute how actual run-ups over the period 2001 to 2016 differ from those predicted by the model. The difference between actual and predicted run-ups measures the change in run-ups that cannot be attributed to changes in deal and target characteristics. As we show in Table 4, the predicted run-ups are higher than the actual run-ups in all years. Except for 2008 and 2015, the difference is statistically significant at the 5% level or better. The magnitude of the difference is of the order of 6%. The predicted run-ups do not decrease over time, again suggesting that deal- and target-related determinants cannot account for the decline in actual pre-merger target stock returns.

<< Please insert Table 4 here >>

In a second set of tests, we replace our main run-up measure with alternative measures. Table 3, Column (7) replicates the analysis in Column (6) with a target run-up measured over the 60 trading days before the M&A announcement date as the dependent variable. We find

[4] Restricting the estimation period from 1985 to the trading day before the enactment of the first IT event in 1988 substantially reduces the size of the sample that can be used (to 86 observations), but leads to qualitatively similar results. Our different analyses in Table 3 show that this event never has a significant impact on run-ups.

that the time trend dummy variable is again insignificant, and the findings with regards to the merger anticipation proxies remain largely unchanged. The significance levels of the IT event dummy variables are lower than in Column (6), which could be attributable to the larger amount of noise in run-ups measured over longer windows. Only the dummy variable associated with IT event 3 is statistically significant at less than 10%. Column (8) replicates the analysis in Column (6) with our main run-up measure scaled with total (pre-announcement and announcement) target firm stock price effects as the dependent variable. We obtain similar findings to those in the baseline analysis in Column (6). Most importantly, the coefficients on all IT event dummy variables except for the dummy variable capturing the first event are significantly negative, and the time trend variable is not significant. In unreported tests, we also replicate all other columns in Table (3) with these two alternative run-up measures, and obtain highly similar results. Thus, our key finding that the decline in pre-merger run-ups is explained by a reduction in insider trading (caused by stricter insider trading rules) rather than by changes in deal- and firm-specific characteristics is robust to the use of alternative dependent variables. In addition, we replicate the full analysis in Table 3 using target pre-announcement trading volumes (measured as outlined in the Appendix) instead of run-ups as the dependent variable. Table 5 reports the results of this analysis. We see that the results are highly similar to those for run-ups. We find a negative time trend in pre-announcement target trading volumes, which cannot be explained by deal or target characteristics, and disappears after controlling for events associated with stronger insider trading rules. This finding corroborates our earlier conclusion that stricter insider trading rules drive the decline in target stock run-ups.

<< Please insert Table 5 here >>

Under the merger anticipation hypothesis, pre-merger run-ups reflect changes in the anticipated likelihood of a merger multiplied with the anticipated takeover premium

(Schwert, 1996). Thus, the finding of declining run-ups may be caused by an overall decrease in takeover premiums over time, for reasons unexplained by our model. To investigate this possibility, Table 6 reports the evolution of two alternative takeover premium measures suggested by the literature over the research period. The number of observations that we can use for this analysis is slightly lower than those in our main sample (2,004 in total), due to some missing post-event stock price data. We find no significant time trend in takeover premiums over our research window, suggesting that our key result is not attributable to a shift in the size of merger payments.

<< Please insert Table 6 here >>

The negative impact of the IT event dummy variables on U.S. target run-ups might be caused by factors unrelated to insider trading, such as changes in macroeconomic conditions. To verify whether this is the case, we run a placebo test on the impact of U.S. IT events on Canadian M&A target firms. Canadian target firms provide a suitable counterfactual for U.S. target firms due to important similarities in Canadian and U.S. institutional settings (King, 2009), driven by a common legal ancestry (Buckley, 1970). However, insider trading rules are much laxer in Canada, compared with U.S. rules (Bris, 2005). If the negative impact of the IT event dummy variables on U.S. target run-ups is indeed due to stronger insider trading mitigation in the U.S., then we do not expect these dummy variables to be significant in a Canadian context. Conversely, if omitted variables cause the negative impact of the IT event dummy variables, then they may also be significant in a Canadian context. We obtain a sample of Canadian M&A deals from Thomson ONE, and clean the sample using similar criteria as those outlined for U.S. deals. We only start the analysis in 1990, due to a lack of deals in the 1980s. Our final Canadian M&A deal sample consists of 690 observations, with an average 20-day target abnormal stock run-up of approximately 8%. Table 7 provides the

placebo test results. The dependent variable is the target abnormal stock run-up calculated over trading days –20 to –1 before the merger announcement date.

<< Please insert Table 7 here >>

Column (1) only includes a time trend variable, which is insignificant. Thus, unlike in the U.S., we do not find that Canadian target stock run-ups are declining over time. In Column (2), we include the same deal-specific variables as those for U.S. deals. We again find no significant time trend. The target-bidder ratio has a negative impact on target run-ups, similar to the U.S. findings. In Column (3), we add a dummy variable capturing an event that potentially strengthened insider trading rules in Canada. More particularly, on January 30, 2003, Canadian authorities responded to the lack of enforcement of insider trading rules by proposing a national framework for securities regulation and by introducing Bill C-46, which provided specific Criminal Code offences in relation to insider trading and substantially increased the penalties.[5] Integrated Market Enforcement Teams were established around the same period (King, 2009). We find a negative effect of these changes on Canadian stock price run-ups, with the overall trend now obtaining a positive coefficient. In Column (4), we add U.S. IT event dummy variables capturing insider trading mitigation events in the U.S. after 1990, which serve as our placebo variables. We find that the U.S. IT event dummies do not have a significant impact on Canadian target run-ups. In Column (5), we repeat the analysis in Column (4) when excluding the trend variable. Findings remain similar to those in Column (4) in that the U.S. event dummies do not have significant effects on Canadian target run-ups. Overall, this placebo test suggests that the negative impact of IT event dummies in a U.S. context is not spurious.

## 6. Conclusion

[5] See the Insider Trading Task Force Report (2003).

Merger target run-ups are among the most famous examples of pre-announcement stock price patterns. The literature provides robust evidence that firms that are acquired exhibit a stock price increase in the period prior to the acquisition announcement (Dodd, 1980; Keown and Pinkerton, 1981; Dennis and McConnell, 1986; Meulbroek, 1992; Schwert, 1996).

In this paper, we report that this well-established stylized fact has dramatically diminished over time. In our sample of U.S. domestic acquisitions from 1985 to 2016, we obtain average pre-acquisition target run-ups of 10.09% during the last half of the 1980s, similar to previous studies. However, average run-ups have declined to 8.4% by the last half of the 90s, and keep diminishing during the 21st century. In the last subperiod of our research window, 2010–2016, average target run-ups are as low as 3.5%, with the median being 2.1%. On the contrary, we observe that announcement-period target abnormal stock returns increase over time, resulting in a relatively constant magnitude of overall target stock returns associated with merger announcements over time. In other words, our findings show that, over time, a smaller proportion of the target abnormal stock returns materialize prior to the announcement date, and a larger proportion occurs on or after the announcement date.

The decline in target run-ups over time could potentially be attributed to a diminishing ability of investors to anticipate mergers, and/or to a decline in illegal insider trading activity in the market. Using a range of deal and target characteristics suggested by previous studies, we do not find evidence for the merger anticipation explanation. To give one illustration, our findings show that the likelihood of deals being preceded by pre-announcement rumors actually increases over time, inconsistent with the assumption that M&A deals have become less predictable over the research period.

We then test whether changes in the strength of insider trading rules can explain the decline in target run-ups. From a literature and internet search, we identify five relevant events associated with a potential mitigation in insider trading in a U.S. context. A regression

of target run-ups on dummy variables capturing these events, as well as merger anticipation control proxies, suggests that the significant time trend in the run-ups disappears after controlling for stricter insider trading regulation over time. Our results survive a series of robustness tests and a placebo analysis using Canadian target firms.

Overall, our findings suggest that U.S. policy makers are successful in reducing illegal trading practices around takeover announcements.

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**Figure 1: Median target stock run-ups over six subperiods of the research period**

This figure reports median daily target firm abnormal stock returns for six subperiods within our research window 1985–2016. Days represent trading days relative to the merger announcement date retrieved from Thomson ONE. Abnormal stock returns are estimated with the market model. We estimate the regression model over a period of 190 days ending on day –76 relative to the announcement date. We use the CRSP equally weighted index as a proxy for the market portfolio.

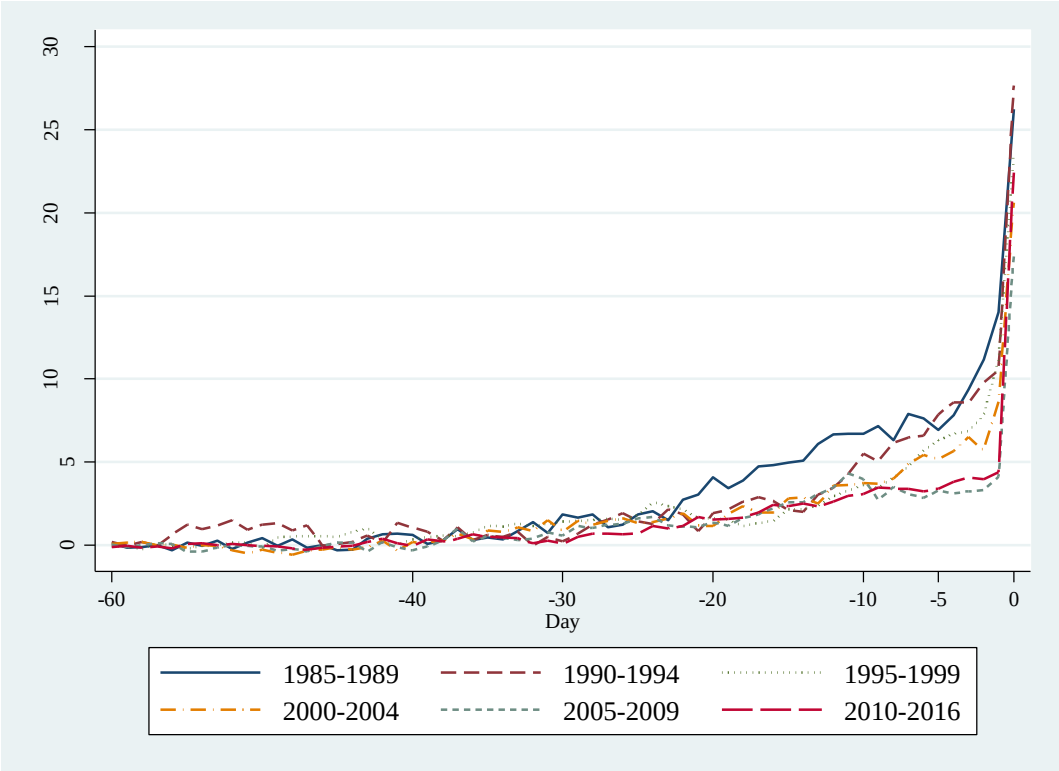

**Figure 2: Median target stock run-ups for deals immediately before and after insider-trading reducing events**

This figure shows median target stock run-ups for merger bids announced before and after the enactment of five events associated with more stringent insider trading rules. We leave a 90-trading day gap after each event so that the run-up period for post-event deals does not include pre-event trading days. Days represent trading days relative to the merger announcement date retrieved from Thomson ONE. The vertical line highlights day –1. Abnormal stock returns are estimated with the market model. We estimate the regression model over a period of 190 days ending on day –76 relative to the announcement date. We use the CRSP equally weighted index as a proxy for the market portfolio.

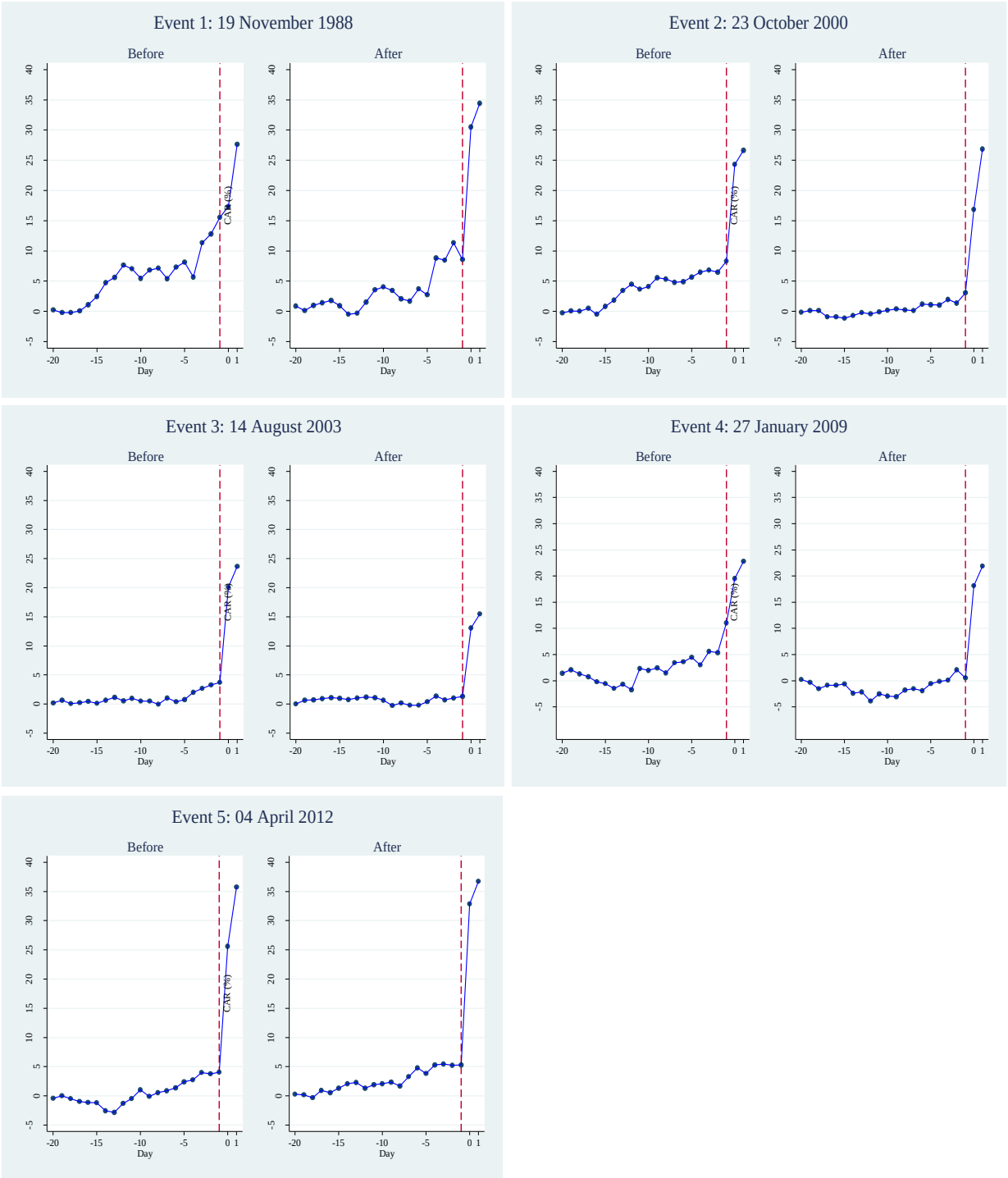

**Table 1: Target run-ups and announcement effects over time**

This table reports mean and median target firm stock run-ups and announcement effects over six different subperiods, as well as the number of deals for each subperiod. Trading day 0 represents the deal's announcement date retrieved from Thomson ONE. The sample consists of public-to-public takeovers of U.S. firms from 1985 to 2016 obtained from Thomson ONE. The Appendix provides variable definitions.

| Year | CAR[–20,–1] (1) | | CAR[–60,–1] (2) | | CAR[0,+1] (3) | | CAR[–20,+1] (4) | | CAR[–60,+1] (5) | | CAR[–20,–1] / CAR[–20,+1] (6) | | CAR[–60,–1] / CAR[–60, +1] (7) | | Number of deals |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Mean | Median | Mean | Median | Mean | Median | Mean | Median | Mean | Median | Mean | Median | Mean | Median | |
| 1985–1989 | 10.091 | 8.590 | 12.718 | 12.950 | 16.551 | 12.660 | 27.667 | 25.105 | 29.645 | 29.945 | 40.328 | 41.531 | 39.558 | 50.150 | 114 |
| 1990–1994 | 9.037 | 8.270 | 11.596 | 10.505 | 21.300 | 18.630 | 30.354 | 28.945 | 33.000 | 30.350 | 29.732 | 23.138 | 39.283 | 46.149 | 156 |
| 1995–1999 | 8.442 | 6.035 | 10.871 | 11.155 | 17.536 | 14.070 | 26.484 | 24.365 | 29.248 | 26.890 | 34.695 | 34.821 | 44.192 | 53.428 | 594 |
| 2000–2004 | 6.561 | 4.350 | 8.413 | 8.740 | 20.067 | 16.580 | 26.086 | 21.150 | 28.269 | 24.750 | 27.965 | 24.269 | 47.607 | 49.716 | 476 |
| 2005–2009 | 3.677 | 2.070 | 5.415 | 4.120 | 22.765 | 19.110 | 26.448 | 22.630 | 28.139 | 25.150 | 17.460 | 13.598 | 24.689 | 28.893 | 341 |
| 2010–2016 | 3.537 | 2.110 | 5.523 | 4.315 | 24.797 | 21.890 | 28.919 | 25.095 | 31.181 | 28.185 | 16.672 | 9.963 | 17.986 | 20.132 | 420 |
| 1985–2016 | 6.395 | 4.070 | 8.514 | 6.950 | 20.635 | 16.82 | 27.226 | 23.900 | 29.533 | 26.700 | 26.707 | 22.128 | 35.946 | 41.634 | 2,101 |
| Trend | –0.289 | –0.257 | –0.327 | –0.391 | 0.301 | 0.323 | 0.011 | –0.062 | –0.021 | –0.082 | –0.919 | –1.221 | –1.139 | –1.498 | |
| *p*–value | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.868 | 0.456 | 0.794 | 0.380 | 0.000 | 0.000 | 0.000 | 0.000 | |

**Table 2: Target run-ups by subsamples of deals and firms**

This table reports median target firm abnormal stock run-ups for subsamples based on deal and target characteristics. We calculate target run-ups as abnormal stock returns over trading days –20 to –1 relative to the merger announcement date retrieved from Thomson ONE. We create subsamples for dummy variables on a 0/1 basis. We create subsamples for continuous variables using their median value. For each subsample, we report median target stock run-ups calculated over the entire sample period (Column (1)), as well as for six subperiods (Columns (2) to (7)). The values reported in italics in the 1985–2016 column represent the difference in the medians for the 1 versus 0 (dummy variables) or High versus Low (continuous variables) subsamples. We assess statistical significance of the differences in medians using a Wilcoxon test. For each subsample in Column (1), we regress target stock run-ups on a yearly time trend variable and an intercept, using the entire sample period. We report the coefficient and *p*-value of the time trend in the last two columns of this table. The sample consists of public-to-public takeovers of U.S. firms from 1985 to 2016, obtained from Thomson ONE. The Appendix provides variable definitions. *, **, and *** indicate statistical significance at the 10, 5, and 1% level, respectively.

| Variable | Sub-sample | 1985–2016 (1) | 1985–1989 (2) | 1990–1994 (3) | 1995–1999 (4) | 2000–2004 (5) | 2005–2009 (6) | 2010–2016 (7) | Trend (8) | *p*–value |
|---|---|---|---|---|---|---|---|---|---|---|
| *Panel A: Deal characteristics* | | | | | | | | | | |
| Rumor | 0 | 3.790 | 7.650 | 7.750 | 5.750 | 3.715 | 1.625 | 2.240 | –0.231 | 0.000 |
| | 1 | 6.330 | 19.420 | 11.000 | 12.130 | 7.360 | 4.930 | 1.840 | –0.571 | 0.000 |
| | | *2.540**** | | | | | | | | |
| Toehold | 0 | 3.985 | 7.690 | 8.010 | 6.620 | 4.350 | 2.070 | 2.040 | –0.261 | 0.000 |
| | 1 | 5.590 | 12.250 | 8.390 | –0.350 | 5.325 | 1.430 | 3.740 | –0.424 | 0.060 |
| | | *1.605* | | | | | | | | |
| Cash financing | Low | 3.880 | 1.090 | 7.795 | 5.315 | 4.195 | 2.400 | 1.365 | –0.216 | 0.001 |
| | High | 4.380 | 13.230 | 8.475 | 9.290 | 4.460 | 2.030 | 2.660 | –0.354 | 0.000 |
| | | 0.500 | | | | | | | | |
| Hostile | 0 | 4.195 | 8.910 | 8.180 | 6.240 | 4.350 | 2.165 | 2.245 | –0.266 | 0.000 |
| | 1 | 1.850 | 5.265 | 36.330 | –0.685 | 2.220 | –1.340 | 0.235 | –0.121 | 0.603 |
| | | *–2.345* | | | | | | | | |
| Poison pill | 0 | 4.070 | 8.910 | 8.270 | 6.035 | 4.460 | 2.110 | 2.150 | –0.261 | 0.000 |
| | 1 | 2.550 | 7.975 | 7.520 | 8.555 | –6.930 | –1.340 | –0.370 | –0.536 | 0.274 |
| | | *–1.520* | | | | | | | | |
| Number of bidders | Low | 3.915 | 6.515 | 8.180 | 6.320 | 4.230 | 2.165 | 1.945 | –0.242 | 0.000 |
| | High | 8.360 | 17.680 | 11.790 | 4.195 | 9.120 | –1.340 | 7.285 | –0.340 | 0.080 |
| | | *4.445*** | | | | | | | | |
| Industry activity | Low | 4.115 | 9.790 | 8.440 | 7.415 | 4.120 | 1.560 | 2.630 | –0.273 | 0.000 |
| | High | 4.000 | 3.990 | 7.445 | 5.735 | 4.410 | 2.725 | 1.430 | –0.263 | 0.000 |
| | | *–0.115* | | | | | | | | |
| Target-bidder ratio | 0 | 4.920 | 9.560 | 8.530 | 8.550 | 6.200 | 2.235 | 1.930 | –0.348 | 0.000 |
| | 1 | 3.415 | 8.340 | 7.050 | 5.050 | 1.790 | 2.050 | 2.540 | –0.191 | 0.000 |
| | | *–1.505**** | | | | | | | | |

| Variable | Sub-sample | 1985–2016 (1) | 1985–1989 (2) | 1990–1994 (3) | 1995–1999 (4) | 2000–2004 (5) | 2005–2009 (6) | 2010–2016 (7) | Trend (8) | *p*–value |
|---|---|---|---|---|---|---|---|---|---|---|
| *Panel B: Target characteristics* | | | | | | | | | | |
| Market-to-book | Low | 4.340 | 9.880 | 7.430 | 6.590 | 3.780 | 1.920 | 1.850 | –0.281 | 0.000 |
| | High | 3.935 | 3.090 | 8.940 | 5.770 | 5.160 | 2.330 | 2.540 | –0.222 | 0.000 |
| | | *–0.405* | | | | | | | | |
| FCF | Low | 4.525 | 8.910 | 5.710 | 6.300 | 4.230 | 3.370 | 2.270 | –0.222 | 0.000 |
| | High | 3.450 | 8.015 | 11.000 | 5.850 | 4.520 | 1.155 | 2.040 | –0.293 | 0.000 |
| | | *–1.075* | | | | | | | | |
| Dividend yield | Low | 4.585 | 10.320 | 8.910 | 6.850 | 5.750 | 2.325 | 1.960 | –0.319 | 0.000 |
| | High | 3.460 | 7.690 | 3.130 | 5.750 | 2.830 | 1.680 | 2.290 | –0.201 | 0.000 |
| | | *–1.125*** | | | | | | | | |
| Sales growth | Low | 3.670 | 9.835 | 5.730 | 5.935 | 3.575 | 2.240 | 2.240 | –0.223 | 0.000 |
| | High | 4.540 | 6.885 | 9.920 | 6.380 | 5.155 | 2.050 | 1.850 | –0.315 | 0.000 |
| | | *0.870* | | | | | | | | |
| GR mismatch | 0 | 3.990 | 8.550 | 8.270 | 6.340 | 3.430 | 2.050 | 1.810 | –0.281 | 0.000 |
| | 1 | 4.200 | 9.130 | 8.150 | 5.650 | 8.610 | 2.480 | 2.540 | –0.250 | 0.001 |
| | | *0.210* | | | | | | | | |
| Leverage | Low | 4.580 | 10.045 | 9.920 | 6.050 | 7.660 | 1.730 | 1.830 | –0.333 | 0.000 |
| | High | 3.440 | 6.920 | 5.600 | 6.020 | 2.600 | 2.490 | 2.555 | –0.179 | 0.004 |
| | | *–1.140** | | | | | | | | |
| Liquidity | Low | 4.400 | 8.300 | 7.430 | 6.640 | 3.950 | 2.010 | 1.540 | –0.285 | 0.000 |
| | High | 3.760 | 8.800 | 8.740 | 5.670 | 4.740 | 2.180 | 2.630 | –0.230 | 0.000 |
| | | *–0.640* | | | | | | | | |
| Firm size | Low | 5.370 | 10.320 | 8.940 | 7.690 | 6.490 | 2.180 | 1.870 | –0.364 | 0.000 |
| | High | 3.235 | 6.930 | 3.810 | 4.990 | 2.710 | 2.060 | 2.305 | –0.149 | 0.001 |
| | | *–2.135**** | | | | | | | | |
| Abnormal stock return | Low | 4.590 | 7.690 | 7.590 | 6.860 | 5.225 | 2.610 | 2.480 | –0.252 | 0.000 |
| | High | 3.725 | 8.800 | 8.700 | 5.365 | 3.940 | 1.570 | 1.830 | –0.285 | 0.000 |
| | | *–0.865* | | | | | | | | |
| State | 0 | 3.980 | 6.930 | 9.200 | 5.600 | 3.480 | 2.940 | 1.670 | –0.251 | 0.000 |
| | 1 | 4.070 | 11.100 | 5.665 | 6.850 | 5.020 | 1.920 | 2.670 | –0.272 | 0.000 |
| | | *0.090* | | | | | | | | |

**Table 3: OLS regression of target run-ups**

This table reports the estimates of OLS regressions of target firms' stock run-ups on deal- and firm-specific explanatory variables. In Columns (1) to (6), we calculate target run-ups as abnormal stock returns over trading days –20 to –1 relative to the merger announcement date retrieved from Thomson ONE. In Column (7), we calculate target run-ups as abnormal stock returns over trading days –60 to –1 relative to the merger announcement date. In Column (8), we scale the target run-ups over trading days –20 to –1 with the total pre-announcement and announcement-period stock price effect of the merger for the target firm. The sample consists of public-to-public takeovers of U.S. firms from 1985 to 2016, obtained from Thomson ONE. The Appendix provides variable definitions. *, **, and *** indicate statistical significance at the 10, 5, and 1% level, respectively.

| | CAR[–20,–1] | | | | | | CAR[–60,–1] | $\frac{\text{CAR[–20,–1]}}{\text{CAR[–20,+1]}}$ |
|---|---|---|---|---|---|---|---|---|
| Variables | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Trend | | | | –0.289*** | –0.314*** | 0.091 | 0.179 | 0.005 |
| | | | | (–8.12) | (–8.06) | (0.62) | (0.70) | (0.78) |
| Rumor | 4.365*** | 4.517*** | 4.317*** | | 4.522*** | 4.518*** | 8.026*** | 0.245*** |
| | (4.82) | (5.08) | (4.94) | | (5.11) | (5.12) | (5.30) | (5.69) |
| Toehold | 0.333 | –1.329 | –1.335 | | –1.361 | –1.144 | –2.286 | 0.117 |
| | (0.22) | (–0.90) | (–0.89) | | (–0.93) | (–0.77) | (–0.86) | (1.61) |
| Cash financing | 0.004 | 0.019** | 0.020*** | | 0.016** | 0.020*** | 0.028** | –0.001** |
| | (0.50) | (2.51) | (2.65) | | (2.21) | (2.67) | (2.15) | (–2.25) |
| Hostile | –1.664 | –0.853 | –1.237 | | –0.494 | –0.910 | –2.744 | –0.208*** |
| | (–0.84) | (–0.44) | (–0.64) | | (–0.25) | (–0.47) | (–0.90) | (–2.85) |
| Poison pill | 2.650 | 0.209 | 0.476 | | –0.182 | 0.309 | 1.249 | 0.196 |
| | (0.85) | (0.07) | (0.15) | | (–0.06) | (0.10) | (0.21) | (1.63) |
| Number of bidders | 1.643 | 0.498 | 0.691 | | 0.371 | 0.522 | 2.802 | 0.038 |
| | (1.60) | (0.49) | (0.70) | | (0.37) | (0.51) | (1.58) | (0.75) |
| Industry activity | 0.319 | 0.677 | 0.671 | | 0.794 | 0.630 | –0.585 | 0.029 |
| | (0.53) | (1.10) | (1.09) | | (1.31) | (1.03) | (–0.54) | (1.00) |
| Target-bidder ratio | –1.087*** | –1.051*** | –1.007*** | | –1.051*** | –1.001*** | –1.348*** | –0.011 |
| | (–4.55) | (–4.43) | (–4.24) | | (–4.46) | (–4.22) | (–3.31) | (–0.99) |
| Market-to-book | –0.090 | –0.074 | –0.192 | | –0.064 | –0.135 | –1.352*** | 0.003 |
| | (–0.48) | (–0.40) | (–1.02) | | (–0.35) | (–0.73) | (–4.04) | (0.35) |
| FCF | –6.456** | –6.891** | –6.468** | | –6.868** | –6.810** | 1.448 | –0.314** |
| | (–2.22) | (–2.41) | (–2.22) | | (–2.39) | (–2.38) | (0.28) | (–2.43) |
| Dividend yield | –2.905 | –8.738 | –6.382 | | –10.087 | –7.673 | –29.369 | –1.932** |
| | (–0.17) | (–0.50) | (–0.37) | | (–0.57) | (–0.44) | (–0.98) | (–2.14) |
| Sales growth | 0.008 | –0.005 | –0.009 | | –0.005 | –0.010 | 0.026 | –0.000 |
| | (0.49) | (–0.27) | (–0.53) | | (–0.31) | (–0.58) | (0.85) | (–0.41) |
| GR mismatch | –0.147 | 0.171 | 0.084 | | 0.198 | 0.287 | –1.767 | –0.038 |
| | (–0.19) | (0.23) | (0.11) | | (0.26) | (0.38) | (–1.34) | (–1.06) |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Leverage | –0.083 | 0.080 | 0.258 | | 0.041 | 0.231 | 1.202* | 0.010 |
| | (–0.20) | (0.19) | (0.61) | | (0.10) | (0.55) | (1.66) | (0.49) |
| Liquidity | –1.541 | –0.725 | –0.673 | | –0.979 | –0.677 | 1.125 | 0.001 |
| | (–1.35) | (–0.64) | (–0.58) | | (–0.87) | (–0.59) | (0.54) | (0.02) |
| Firm size | –0.981*** | –0.661*** | –0.645*** | | –0.681*** | –0.647*** | –0.678* | –0.002 |
| | (–4.50) | (–3.01) | (–2.92) | | (–3.11) | (–2.94) | (–1.79) | (–0.21) |
| Abnormal stock return | –7.467* | –5.914 | –0.924 | | –6.529* | –2.845 | –1.442 | –0.257 |
| | (–1.88) | (–1.46) | (–0.21) | | (–1.66) | (–0.69) | (–0.20) | (–1.43) |
| State | –0.110 | 0.140 | 0.141 | | 0.184 | 0.161 | 0.360 | –0.020 |
| | (–0.18) | (0.23) | (0.24) | | (0.31) | (0.27) | (0.35) | (–0.68) |
| 1990–1994 | | –1.118 | | | | | | |
| | | (–0.65) | | | | | | |
| 1995–1999 | | –1.766 | | | | | | |
| | | (–1.22) | | | | | | |
| 2000–2004 | | –3.963*** | | | | | | |
| | | (–2.65) | | | | | | |
| 2005–2009 | | –7.034*** | | | | | | |
| | | (–4.78) | | | | | | |
| 2010–2016 | | –7.144*** | | | | | | |
| | | (–4.98) | | | | | | |
| IT event 1 (19/11/1988) | | | | | | –2.266 | –1.683 | –0.131 |
| | | | | | | (–1.07) | (–0.49) | (–1.31) |
| IT event 2 (23/10/2000) | | | | | | –5.647** | –6.522 | –0.264** |
| | | | | | | (–2.00) | (–1.37) | (–2.00) |
| IT event 3 (28/04/2003) | | | | | | –8.439*** | –9.154* | –0.287* |
| | | | | | | (–2.69) | (–1.71) | (–1.92) |
| IT event 4 (27/01/2009) | | | | | | –9.410** | –10.252 | –0.413** |
| | | | | | | (–2.43) | (–1.54) | (–2.28) |
| IT event 5 (04/04/2012) | | | | | | –9.721** | –11.938 | –0.353* |
| | | | | | | (–2.28) | (–1.62) | (–1.79) |
| Constant | 13.102*** | 15.377*** | 19.965*** | 11.349*** | 17.013*** | 14.834*** | 17.296*** | 0.432*** |
| | (6.33) | (6.05) | (4.51) | (15.79) | (8.06) | (5.50) | (3.74) | (3.45) |
| $R^2$ | 0.0425 | 0.0735 | 0.0998 | 0.0275 | 0.0689 | 0.0782 | 0.0538 | 0.0502 |
| Year FEs | No | No | Yes | No | No | No | No | No |
| Observations | 2,101 | 2,101 | 2,101 | 2,101 | 2,101 | 2,101 | 2,101 | 2,101 |

**Table 4: Predicted run–up and deviations from actual run–up by year**

This table reports the differences between the actual and predicted average target firms' stock run-ups. We calculate target run-ups as abnormal stock returns over trading days –20 to –1 relative to the merger announcement date retrieved from Thomson ONE. Predicted values are obtained using the coefficients of the explanatory variables for the sample period between 01/01/1985 until 23/10/2000. *t*-statistics on the differences between actual and predicted average target run-ups are reported in the final column of the table. The sample consists of public-to-public takeovers of U.S. firms from 1985 to 2016, obtained from Thomson ONE. The Appendix provides variable definitions. *, **, and *** indicate statistical significance at the 10, 5, and 1% level, respectively.

| Year | Actual | Predicted | Actual – Predicted | *t*-statistic |
|---|---|---|---|---|
| 2001 | 4.954 | 9.647 | –4.693 | –3.582*** |
| 2002 | 5.596 | 10.033 | –4.436 | –2.403** |
| 2003 | 4.662 | 9.665 | –5.003 | –4.031*** |
| 2004 | 4.356 | 9.222 | –4.866 | –4.837*** |
| 2005 | 3.375 | 9.750 | –6.375 | –6.479*** |
| 2006 | 3.163 | 9.896 | –6.733 | –6.330*** |
| 2007 | 2.112 | 9.824 | –7.712 | –7.189*** |
| 2008 | 8.511 | 10.177 | –1.666 | –0.699 |
| 2009 | 3.311 | 8.934 | –5.623 | –2.890*** |
| 2010 | 3.718 | 10.445 | –6.727 | –3.971*** |
| 2011 | 4.070 | 9.412 | –5.341 | –3.068*** |
| 2012 | 3.222 | 10.516 | –7.294 | –5.278*** |
| 2013 | 1.348 | 9.465 | –8.117 | –9.072*** |
| 2014 | 2.433 | 9.612 | –7.179 | –5.968*** |
| 2015 | 7.313 | 9.189 | –1.877 | –1.448 |
| 2016 | 1.458 | 9.443 | –7.985 | –6.553*** |

**Table 5: OLS regression of target abnormal trading volumes**

This table reports the estimates of OLS regressions of target firms' abnormal trading volume on deal- and firm-specific explanatory variables. We calculate trading volumes as outlined in the Appendix. The sample consists of public-to-public takeovers of U.S. firms from 1985 to 2016, obtained from Thomson ONE. The Appendix provides variable definitions. *, **, and *** indicate statistical significance at the 10, 5, and 1% level, respectively.

| | Model 1 | Model 2 | Model 3 | Model 4 | Model 5 | Model 6 |
|---|---|---|---|---|---|---|
| Trend | | | | –0.293*** | –0.415*** | 0.031 |
| | | | | (–3.15) | (–4.53) | (0.10) |
| Rumor | 21.105*** | 21.602*** | 21.507*** | | 21.312*** | 21.679*** |
| | (8.71) | (8.99) | (9.08) | | (8.84) | (9.05) |
| Toehold | 3.109 | 0.115 | –0.295 | | 0.871 | 0.011 |
| | (0.85) | (0.03) | (–0.08) | | (0.24) | (0.00) |
| Cash financing | 0.016 | 0.023 | 0.023 | | 0.033** | 0.027* |
| | (1.09) | (1.52) | (1.49) | | (2.17) | (1.76) |
| Hostile | 0.057 | 2.379 | 1.753 | | 1.603 | 2.100 |
| | (0.01) | (0.47) | (0.35) | | (0.31) | (0.41) |
| Poison pill | –5.450 | –10.839 | –8.886 | | –9.192 | –10.379 |
| | (–0.81) | (–1.62) | (–1.33) | | (–1.37) | (–1.52) |
| Number of bidders | 13.766*** | 10.951*** | 11.468*** | | 12.084*** | 11.063*** |
| | (3.98) | (3.16) | (3.37) | | (3.48) | (3.14) |
| Industry activity | –0.427 | 0.662 | 0.880 | | 0.201 | 0.637 |
| | (–0.33) | (0.51) | (0.67) | | (0.16) | (0.49) |
| Target-bidder ratio | –0.462 | –0.512 | –0.656 | | –0.415 | –0.461 |
| | (–0.91) | (–1.02) | (–1.29) | | (–0.82) | (–0.91) |
| Market-to-book | 0.231 | 0.211 | 0.148 | | 0.264 | 0.185 |
| | (0.57) | (0.51) | (0.36) | | (0.65) | (0.45) |
| FCF | –0.495 | –0.337 | –0.525 | | –1.040 | –0.613 |
| | (–0.08) | (–0.06) | (–0.09) | | (–0.18) | (–0.10) |
| Dividend yield | –32.481 | –47.219 | –52.467 | | –41.971 | –45.718 |
| | (–0.92) | (–1.35) | (–1.52) | | (–1.19) | (–1.30) |
| Sales growth | –0.019 | –0.034 | –0.032 | | –0.037 | –0.033 |
| | (–0.57) | (–0.98) | (–0.93) | | (–1.07) | (–0.93) |
| GR mismatch | –0.278 | –0.062 | –0.034 | | 0.178 | 0.249 |
| | (–0.16) | (–0.04) | (–0.02) | | (0.11) | (0.15) |
| Leverage | 1.314 | 1.620* | 1.960** | | 1.478 | 1.758* |
| | (1.42) | (1.74) | (2.10) | | (1.59) | (1.88) |
| Liquidity | 3.510 | 4.269* | 4.410* | | 4.252* | 4.132* |
| | (1.48) | (1.79) | (1.83) | | (1.79) | (1.72) |
| Firm size | –0.065 | 0.200 | 0.182 | | 0.331 | 0.199 |
| | (–0.14) | (0.44) | (0.40) | | (0.73) | (0.44) |
| Abnormal stock return | 16.705** | 21.465*** | 23.857*** | | 17.944** | 23.308*** |
| | (2.19) | (2.73) | (2.78) | | (2.35) | (2.86) |
| State | –2.309* | –1.700 | –1.504 | | –1.921 | –1.622 |
| | (–1.83) | (–1.37) | (–1.21) | | (–1.54) | (–1.30) |
| 1990–1994 | | –13.132*** | | | | |
| | | (–3.23) | | | | |
| 1995–1999 | | –12.150*** | | | | |
| | | (–3.40) | | | | |
| 2000–2004 | | –15.683*** | | | | |
| | | (–4.36) | | | | |
| 2005–2009 | | –15.627*** | | | | |
| | | (–4.15) | | | | |
| 2010–2016 | | –17.154*** | | | | |
| | | (–4.63) | | | | |
| IT event 1 (19/11/1988) | | | | | | –14.068*** |
| | | | | | | (–2.76) |

| | | | | | | |
|---|---|---|---|---|---|---|
| IT event 2 (23/10/2000) | | | | | | –19.557*** |
| | | | | | | (–3.13) |
| IT event 3 (28/04/2003) | | | | | | –19.079*** |
| | | | | | | (–2.68) |
| IT event 4 (27/01/2009) | | | | | | –20.391** |
| | | | | | | (–2.35) |
| IT event 5 (04/04/2012) | | | | | | –19.426** |
| | | | | | | (–2.06) |
| Constant | –2.309 | 11.870** | 29.532** | 19.439*** | 2.859 | 13.115** |
| | (–0.48) | (2.00) | (2.13) | (11.15) | (0.57) | (2.04) |
| $R^2$ | 0.1098 | 0.1249 | 0.1462 | 0.0059 | 0.1195 | 0.1274 |
| Year FEs | No | No | Yes | No | No | No |
| Observations | 2,101 | 2,101 | 2,101 | 2,101 | 2,101 | 2,101 |

**Table 6: Takeover premiums over time**

This table reports mean and median takeover premiums over six subperiods, as well as the number of observations per subperiod. In Column (1), we define the premium as the target firms' cumulative abnormal returns over trading days –63 to +126 relative to the merger announcement date 0 retrieved from Thomson ONE. In Column (2), we use an industry-adjusted takeover premium. We regress each of the premium measures on a yearly time trend variable and an intercept. We report the coefficient and *p*-value of the time trend in the last two columns of this table. The sample consists of public-to-public takeovers of U.S. firms from 1985 to 2016 obtained from Thomson ONE. The Appendix provides variable definitions.

| Year | Premium: CAR(–63,+126) (1) | | Premium: Industry-adjusted (2) | | Number of deals |
|---|---|---|---|---|---|
| | Mean | Median | Mean | Median | |
| 1985–1989 | 32.677 | 34.790 | –0.838 | –0.157 | 99 |
| 1990–1994 | 35.948 | 31.975 | –2.519 | –6.767 | 141 |
| 1995–1999 | 31.876 | 29.390 | 0.693 | –2.057 | 572 |
| 2000–2004 | 29.567 | 27.135 | –3.693 | –8.408 | 459 |
| 2005–2009 | 31.996 | 28.690 | 1.538 | –1.973 | 330 |
| 2010–2016 | 32.174 | 29.900 | –0.773 | –4.572 | 403 |
| 1985–2016 | 31.778 | 29.280 | –0.769 | –4.183 | 2,004 |
| Trend | –0.078 | –0.106 | 0.031 | –0.032 | |
| *p*–value | 0.503 | 0.453 | 0.717 | 0.740 | |

**Table 7: OLS regression of Canadian target run–ups**

This table reports the estimates of OLS regressions of target firms' stock run–ups on deal–specific explanatory variables. We calculate target run–ups as abnormal stock returns over trading days –20 to –1 relative to the merger announcement date retrieved from Thomson ONE. The sample consists of public-to-public takeovers of Canadian firms from 1990 to 2016, obtained from Thomson ONE. IT events represent events associated with stricter insider trading regulation in a Canadian and U.S. context. The Appendix provides variable definitions. *, **, and *** indicate statistical significance at the 10, 5, and 1% level, respectively.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Trend | –0.025 | –0.062 | 0.425** | 0.819 | |
| | (–0.17) | (–0.41) | (2.22) | (1.64) | |
| Rumor | | 5.415 | 3.629 | 3.555 | 3.041 |
| | | (0.67) | (0.41) | (0.41) | (0.36) |
| Toehold | | –0.058 | –0.100 | –0.373 | –0.466 |
| | | (–0.02) | (–0.04) | (–0.15) | (–0.19) |
| Cash financing | | 0.001 | –0.001 | –0.003 | –0.004 |
| | | (0.06) | (–0.07) | (–0.17) | (–0.19) |
| Hostile | | –4.100 | –4.856 | –4.759 | –4.687 |
| | | (–1.01) | (–1.21) | (–1.15) | (–1.11) |
| Poison pill | | –2.708 | –0.131 | –0.813 | –0.962 |
| | | (–0.45) | (–0.02) | (–0.13) | (–0.15) |
| Number of bidders | | 2.529 | 1.327 | 1.518 | 1.581 |
| | | (0.69) | (0.36) | (0.40) | (0.42) |
| Industry activity | | 0.661 | 1.030 | 0.817 | 1.013 |
| | | (0.42) | (0.67) | (0.53) | (0.67) |
| Target–bidder ratio | | –1.644*** | –1.525*** | –1.492*** | –1.581*** |
| | | (–3.09) | (–2.91) | (–2.84) | (–3.01) |
| IT event Canada (30/01/2003) | | | –11.102*** | –11.233** | –10.181** |
| | | | (–3.82) | (–2.16) | (–1.97) |
| IT event 2 (23/10/2000) | | | | 0.355 | 4.317 |
| | | | | (0.07) | (1.00) |
| IT event 3 (28/04/2003) | | | | –3.118 | 3.384 |
| | | | | (–0.42) | (0.55) |
| IT event 4 (27/01/2009) | | | | –0.146 | 9.496 |
| | | | | (–0.02) | (1.52) |
| IT event 5 (04/04/2012) | | | | –7.323 | 5.663 |
| | | | | (–0.72) | (0.91) |
| Constant | 8.483*** | 10.871** | 12.444** | 8.357 | 14.124** |
| | (3.07) | (2.06) | (2.36) | (1.32) | (2.40) |
| $R^2$ | 0.0000 | 0.0174 | 0.0374 | 0.0512 | 0.0477 |
| Year FEs | No | No | No | No | No |
| Observations | 690 | 690 | 690 | 690 | 690 |

*t* statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

**Appendix: Variable definitions**

This Appendix presents the variables employed in the empirical analysis and describes the construction of each one. All days represent trading days. We obtain stock price data from CRSP, balance sheet data from Compustat, and deal-specific information from Thomson ONE. We mention variables in their order of appearance in the tables. All balance sheet items are measured at the fiscal year end before the deal's announcement date obtained from Thomson ONE, unless noted otherwise.

| Variable | Definition |
|---|---|
| CAR[–20,–1] | The sum of target abnormal stock returns for a window of 20 days up to 1 day prior to the acquisition announcement date. Abnormal stock returns are estimated with the market model. We estimate the regression model over a period of 190 days ending on day –76 relative to the announcement date. We use the CRSP equally weighted index as a proxy for the market portfolio. |
| CAR[–60,–1] | The sum of target abnormal stock returns for a window of 60 days prior up to 1 day prior to the acquisition announcement date. Abnormal stock returns are estimated with the market model. We estimate the regression model over a period of 190 days ending on day –76 relative to the announcement date. We use the CRSP equally weighted index as a proxy for the market portfolio. |
| CAR[0,+1] | The sum of target abnormal stock returns on the announcement day and one day after the acquisition announcement. Abnormal stock returns are estimated with the market model. We estimate the regression model over a period of 190 days ending on day –76 relative to the announcement date. We use the CRSP equally weighted index as a proxy for the market portfolio. |
| CAR[–20,+1] | The sum of target abnormal returns for a window of 20 days prior up to 1 day after the acquisition announcement. Abnormal returns are estimated with the market model. We estimate the regression model over a period of 190 days ending on day –76 relative to the announcement date. We use the CRSP equally weighted index as a proxy for the market portfolio. |
| CAR[–60,+1] | The sum of target abnormal returns for a window of 60 days prior up to 1 day after the acquisition announcement. Abnormal returns are estimated with the market model. We estimate the regression model over a period of 190 days ending on day –76 relative to the announcement date. We use the CRSP equally weighted index as a proxy for the market portfolio. |
| CATV[–20,–1] | The sum of abnormal trading volume for a window of 20 days prior up to 1 day prior to the acquisition announcement. Abnormal trading volume ($ATV_{it}$) is estimated as: $$ATV_{i,t} = TV_{it} - \left[\overline{TV_i} + 2stdev_{TV}\right] \; if \; TV_{it} > \overline{TV_i} + 2stdev_{TV}$$ Or $$ATV_{i,t} = 0 \qquad if \; TV_{it} < \overline{TV_i} + 2stdev_{TV}$$ Where $TV_{it}$ is the volume of shares of target firm i traded on day t divided by the number of shares outstanding. $\overline{TV_i}$ and $stdev_{TV}$ are the mean and standard deviation of daily turnover of shares in target firm i over the window (−250, −101). |
| Rumor | An indicator variable taking the value one if there is a takeover rumor pertaining to the deal before the announcement date, and zero otherwise. The corresponding data item from Thomson ONE is 'Deal Began as Rumor'. |
| Toehold | An indicator variable taking the value one if the bidder owns target shares as of the announcement date, and zero otherwise. The corresponding data item from Thomson ONE is 'Percent of Shares Held at Announcement'. |
| Cash financing | The natural logarithm of one plus the percent of the deal value paid in cash. The corresponding data item from Thomson ONE is 'Consideration: Percentage of Cash'. |
| Hostile | An indicator variable taking the value one if the deal is hostile and zero otherwise. The corresponding data item from Thomson ONE is 'Deal Started as Unsolicited Flag (Y/N)'. |
| Poison pill | An indicator variable that takes the value of one if the target has a poison pill approved by its Board, and zero otherwise. The corresponding data item from Thomson ONE is 'Defensive Poison Pill Flag (Y/N)' |
| Number of Bidders | The number of bidders involved in the deal. The corresponding data item from Thomson ONE is 'Number of Bidders' |
| Industry activity | A dummy variable equal to one if there was another acquisition in the firm's four digit SIC code industry in the previous 12 months. We construct this dummy variable based on deal information in Thomson ONE. |
| Target-bidder | The natural logarithm of the ratio of target's market capitalization to bidder's market |

| | |
|---|---|
| ratio | capitalization. |
| Market-to-book ratio | Market to book ratio of common equity |
| FCF | Free cash flow over total assets |
| Dividend yield | Dividend yield ratio |
| Sales growth | Average growth (percentage change) in sales over the last three years prior to the announcement date |
| GR mismatch | An indicator variable that takes the value of one for combinations of above-average growth, below-average liquidity, and above-average leverage, or below-average growth, above-average liquidity, and below-average leverage, and zero otherwise |
| Leverage | Average leverage ratio over the last three years prior to the announcement date |
| Liquidity | Average liquidity ratio over the last three years prior to the announcement date |
| Firm size | Total assets converted into constant 2010 US dollar using the US Consumer Price Index obtained from OECD |
| Abnormal stock return | Average daily excess returns over the last four years compared to S&P 500 |
| State | An indicator variable that takes the value of one if the company's state of incorporation is Delaware and zero otherwise. We obtain this information from Compustat. |
| 1990–1994 | An indicator variable that takes the value of one if the deal was announced between 01/01/1990 and 31/12/1994, and zero otherwise. |
| 1995–1999 | An indicator variable that takes the value of one if the deal was announced between 01/01/1995 and 31/12/1999, and zero otherwise. |
| 2000–2004 | An indicator variable that takes the value of one if the deal was announced between 01/01/2000 and 31/12/2004, and zero otherwise. |
| 2005–2009 | An indicator variable that takes the value of 1 if the deal was announced between 01/01/2005 and 31/12/2009, and zero otherwise. |
| 2010–2016 | An indicator variable that takes the value of one if the deal was announced between 01/01/2010 and 31/12/2016, and zero otherwise. |
| IT event 1 (19/11/1988) | An indicator variable that takes the value of one if the deal was announced after 19/11/1988, and zero otherwise. |
| IT event 2 (23/10/2000) | An indicator variable that takes the value of one if the deal was announced after 23/10/2000, and zero otherwise. |
| IT event 3 (28/04/2003) | An indicator variable that takes the value of one if the deal was announced after 28/04/2003, and zero otherwise. |
| IT event 4 (27/01/2009) | An indicator variable that takes the value of one if the deal was announced after 27/01/2009 and zero otherwise. |
| IT event 5 (04/04/2012) | An indicator variable that takes the value of one if the deal was announced after 04/04/2012 and zero otherwise. |
| IT event Canada (30/01/2003) | An indicator variable that takes the value of one if the deal was announced after 30/01/2003 and zero otherwise. |
| Premium: CAR(–63,+126) | The sum of target abnormal returns for a window of 63 days prior up to 126 days after the acquisition announcement. Abnormal returns are estimated with the market model. We estimate the regression model estimated over a period of 190 days ending on day –76 relative to the announcement date. CRSP equally weighted index is used as a proxy for the market portfolio. |
| Premium: Industry-adjusted | The offer premium minus the mean premium paid for targets in the same industry (based on Fama and French 49 industries) during the announcement year and the year prior to the acquisition announcement. |