Winning by Losing: Evidence on the Long-Run Effects of Mergers*

Ulrike Malmendier
UC Berkeley
NBER

Enrico Moretti
UC Berkeley
NBER

Florian Peters
University of Amsterdam

Abstract

We propose a novel approach to measuring returns to mergers. In a new data set of close bidding contests we use losers' post-merger performance to construct the counterfactual performance of winners had they not won the contest. Stock returns of winners and losers closely track each other over the 36 months before the merger, corroborating our approach to identification. Bidders are also very similar in terms of Tobins Q, profitability and other accounting measures. Over the three years after the merger, however, losers outperform winners by 24 percent. Commonly used methodologies such as announcement returns fail to identify acquirors' underperformance. (*JEL* G34, G14, D03)

* Ulrike Malmendier: UC Berkeley and NBER. Enrico Moretti: UC Berkeley and NBER. Florian Peters: University of Amsterdam. We thank Jeff Zeidel and seminar participants at Amsterdam, Chicago Booth, DePaul, LSE, Luxembourg School of Finance, MIT, NYU, Ohio State University, Princeton, Tinbergen Institute, Yale, and at the AFA, EFA, FIRS, and the NBER Summer Institute for valuable comments. We thank Zach Liscow and Jonas Sobott for excellent research assistance. Send correspondence to Ulrike Malmendier, UC Berkeley and NBER, 530 Evans Hall #3880, Berkeley, CA 94720-3880, USA; telephone: (510) 642-5038; email: ulrike@berkeley.edu.

Do acquiring companies profit from acquisitions, or do acquirors overbid and destroy shareholder value? The negative stock-market reactions to a large number of merger announcements (see, e.g., Moeller, Schlingemann, and Stulz (2005)) have attracted considerable attention to this question. Researchers have interpreted such negative stock-price responses as evidence of incentive misalignment (e.g., empire building) or behavioral biases (e.g., CEO overconfidence).[1]

A major obstacle in the evaluation of mergers is that it is difficult to obtain unbiased estimates of the value they create or destroy. The most common approach, announcement returns, may be biased due to price pressure around mergers, information revealed in the merger bid, or market inefficiencies.[2] Another approach, the computation of long-run abnormal returns, may be biased due to unobserved differences between the firms that merge and the firms that do not.[3] For example, a decline in the acquiror's market valuation after a merger might not be caused by the merger, but could reflect, instead, that highly valued firms *choose* to acquire less highly valued targets. In this case, the subsequent decline would have occurred even in the absence of the takeover.[4] It is difficult to find a valid control group to whom we can compare acquiring firms, as the latter are a selected group and engage in mergers at selected points in time.

In this paper, we propose a novel way to address these selection issues. We exploit merger contests to evaluate the long-run effects of mergers on acquiror returns. The basic idea is to use the returns to losing bidders to calculate the counterfactual performance of the matched winners had they not won the contest. Participation in a bidding contest provides a novel matching criterion, over and above the usual market-, industry-, and firm-level observables.

[1] Jensen (1986), (Morck, Shleifer, and Vishny (1990)), (Roll (1986); Malmendier and Tate (2008)).

[2] See, for example, Mitchell, Pulvino, and Stafford (2004) and Asquith, Bruner, and Mullins Jr (1987).

[3] Loughran and Vijh (1997) and Rau and Vermaelen (1998).

[4] See the argument in Shleifer and Vishny (2003) and Rhodes-Kropf and Viswanathan (2004), as well as the empirical tests in Dong, Hirshleifer, Richardson, and Teoh (2006), Savor and Lu (2009), and Rhodes-Kropf, Robinson, and Viswanathan (2005).

This approach offers an improvement over existing analyses to the extent that winners are more similar to losers than to the average firm in the market or other previously used control groups. It can account for strategic considerations that lead firms to attempt a specific takeover at a specific point in time but that are hard to control for with the standard set of financial variables. For example, the contest setting alleviates the concern about acquirors' prior overvaluation since highly-valued winners are benchmarked against similarly-valued losers.

An attractive feature of our approach is that we can probe the validity of our identifying assumption by comparing the valuation paths of winners and losers in the months and years *prior* to the merger contest. Any differences in expected performance between matched winners and losers should materialize in diverging price paths before the merger. Stock prices are particularly suitable for probing the validity of our identifying assumption because stock valuations are forward looking and capture pre-merger market expectations about future profitability. Another advantage of our approach is that it allows for all parties to re-optimize after the merger, e.g., for the loser to acquire another target. In contrast to some of the previous studies, our approach does not condition on endogeneous post-merger choices.

A disadvantage of our approach is that it is restricted to merger contests. We cannot speak to the value generated in a broader set of mergers. In fact, the more we restrict the sample for the sake of identification (e.g., not only to contested mergers, but to "close" cases of contested mergers, as we will discuss below), the more we reduce generalizability. At the same time, the methodological implications of our findings go beyond the sample of contests. By comparing our estimates to estimates based on existing methodologies, both calculated in our sample, we provide evidence on the biases embedded in other approaches and their potential magnitude.

We collect data on all U.S. mergers with concurrent bids of at least two potential acquirors

since 1985. We validate the accuracy of the Thomson data regarding the contested nature of the mergers using press reports.[5] We also collect a similar international data set.

To maximize the similarity between winning and losing bidders, we exclude contests where the initial bidder withdraws shortly after the competing bid arrives. Such short contests tend to feature one bidder who is ex ante particularly likely to win, invalidating the identifying assumption. Instead, we focus on protracted contests where bidders actively compete, for example by making multiple offers and counter-offers. In such cases, all bidders tend to have a significant ex-ante chance of winning the contest, ameliorating concerns about endogeneity in the ultimate outcome.

We find that winners' and losers' have generally similar observable characteristics before the merger, and are more similar than winners are to the average U.S. firm. In particular, winners and losers are comparable in terms of pre-merger Tobin's Q, PP&E, profitability, book leverage, and market leverage. Most importantly, winners and losers display very similar stock-market performance in the months leading up to the announcement, in particular in closely contested cases: Their buy-and-hold abnormal returns closely track each other during the 36 months before the merger announcement, in particular in the sample of protracted merger contests. In addition, analyst forecasts of winners' and losers' future earnings-to-price ratios are similar. Thus, consistent with our identifying assumption, both the market overall and experts evaluating the companies have similar expectations about the future profitability of winners and losers before the merger.

After the merger, however, losers significantly outperform winners. The estimated effect for U.S. mergers is economically large, a 23.3 to 33.2 percent difference in buy-and-hold abnormal returns over the next three years relative to winners, depending on the sample and type of abnormal return calculation. These differences in post-merger performance cannot

[5] We search for press mentions in the *Financial Times*, *Wall Street Journal*, and *Washington Post* and in some models we require that at least one article mentions the competing bids identified by Thomson.

be attributed to changes in the risk profile of winners relative to losers since the results are unaffected when we adjust for changes in risk exposure. We also show that they are not explained by pre-merger run-ups in stock price or valuation differences in terms of market-to-book ratio.

Outside the U.S. contested mergers generate less underperformance. We estimate a statistically significant effect of 13.6 percent in the international sample.

While our main goal is to estimate the *average* effect of mergers, in the second part of the paper we explore potential channels that may explain a negative effect. We focus on three potential mechanisms that are commonly discussed in the literature: merger-induced reductions in strategic flexibility, the post-merger cost of integration, and pre-existing target inefficiencies. These mechanisms are of course not necessarily mutually exclusive.

We find that the loss of strategic and financial flexibility may help to explain the underperformance of acquirors. The negative merger effect is estimated to be particularly large in acquirors who spend their cash or bump up their leverage to finance the merger. We also find that proxies for high costs of post-merger integration, based on industry differences or relative acquiror/target size, predict larger than average effects. Finally, stock-based, cash-flow based, and Q-based proxies point to the possibility that pre-existing target inefficiencies may also play a role.

Thus, all three channels may help to explain the underperformance of acquirors. We stress, however, that the estimation of these models is significantly more demanding than the estimation of our baseline models as it requires us to subsample along the dimension of the respective characteristic. As such these analyses yield estimates that are necessarily less precise and should be considered suggestive, rather than definitive.

In the last part of the paper, we use our empirical approach to evaluate the main existing methods that have been employed to measure returns to mergers. When we calculate announcement effects, alphas based on four-factor calendar-time regressions of winner-only

portfolios, and winners' long-run buy-and-hold abnormal returns in our sample, we find that none of these approaches capture the negative long-run return implications of contested mergers. Winners' announcement returns are insignificantly negative, both at the initial bid and at the losing bidder's withdrawal, and winners' four-factor alphas as well as winners' long-run buy-and-hold abnormal returns are insignificantly positive. Moreover, winners' announcement returns display an insignificantly negative correlation with our estimates, i.e., fail to predict the causal effect of contested mergers even directionally.

This paper relates to a large literature estimating the value created in corporate takeovers, which goes back at least to the 1970s (e.g., Mandelker (1974) and Dodd and Ruback (1977)). Early reviews of the empirical evidence include Jensen and Ruback (1983) and Roll (1986); more reviews are from Andrade, Mitchell, and Stafford (2001) and Betton, Eckbo, and Thorburn (2008). The assessment of the value effects of mergers has changed over the decades, not only due to time variation in the data but also due to new methodology and econometric techniques, as emphasized, for example, by Bhagat, Dong, Hirshleifer, and Noah (2005). Recent studies of acquiror *percentage* announcement returns find relatively small but statistically significant effects of 0.5-1% (Moeller, Schlingemann, and Stulz (2004); Betton, Eckbo, and Thorburn (2008)). The analysis of *dollar* announcement returns (Moeller, Schlingemann, and Stulz (2005)) reveals that a small number of large losses swamp the majority of profitable, but smaller, acquisitions. Studies of long-run post-merger performance suggest that stock mergers and mergers by highly valued acquirors are followed by poor performance (Loughran and Vijh (1997); Rau and Vermaelen (1998)). Industry-specific studies of the long-term consequences of mergers, such as recently Allen, Clark, and Houde (2014) for the mortgage industry and Gowrisankaran, Nevo, and Town (2015) for the hospital industry, tend to focus on the welfare implications for consumers rather than abnormal returns.

Few papers analyze bidder returns in bidding contests. Instead, most of the prior litera-

ture exploits failed merger bids and bidding contests to draw implications for the target, including for example Davidson, Dutia, and Cheng (1989), Fabozzi, Ferri, Fabozzi, and Tucker (1988), Officer (2003), and Dodd (1980). One notable exception is Boone and Mulherin (2008), who identify competing bidders in the private negotiation stage and test whether more bidding competition induces lower bidder returns. They find that this is not the case, consistent with our results for later-stage bidding. Differently from our paper, their analysis does not use a winner-loser comparison. Two other exceptions are the theoretical framework presented in Hietala, Kaplan, and Robinson (2003) who focus on short-term price movements around merger contests to draw inferences about synergies, information effects and overpayment, and the methodology developed in Barraclough, Robinson, Smith, and Whaley (2012) which uses option prices to disentangle synergies and information effects for the bidder and the target.

The winner-loser research design is, instead, motivated by Greenstone and Moretti (2004) and Greenstone, Hornbeck, and Moretti (2010), who analyze bids by local governments to attract "million-dollar plants" to their jurisdiction by comparing winners and losers. Relative to their county-level analysis, mergers allow for considerably more convincing controls of bidder heterogeneity. In contrast to measures such as firm productivity or labor earnings, stock prices incorporate not just current conditions but also expectations about future performance. Our identification strategy also relates to Savor and Lu (2009), who use a small sample of failed acquisitions to construct a counterfactual. On the theoretical side, Dimopoulos and Sacchetto (2014) features an auction model and structural estimation relating bidder behavior to subsequent abnormal returns.

1. Data

1.1. Sample Construction

Our data combine information on merger contests from the Thomson One Mergers and Acquisitions database with financial and accounting information from CRSP, Compustat, Compustat Global, Datastream as well as analyst forecast data from I/B/E/S.

Thomson One records public and binding acquisition bids.[6] We collect all contested bids made by public U.S.-listed firms between January 1, 1985 and December 31, 2012. We exclude privately held and government-owned firms, mutually owned companies, subsidiaries, and firms whose status Thomson cannot reliably identify. We also exclude white knights since they likely lack ex-ante similarity with other bidders in their success chances and hence do not provide a plausible hypothetical counterfactual. To identify contested mergers, we use Thomson One's competing bid flag. We verify that all bids for a given target that are flagged as contested in the Thomson One database were indeed valid in overlapping time periods. In other words, we require that the period between announcement of the winning bid and completion of the merger overlaps with the period between announcement and withdrawal of the losing bids. We classify the company that succeeds in completing the merger as the winner, and all other bidders as losers. In three cases, we find that Thomson erroneously assigns two winners, and we identify the unique winner using a news wire search. A detailed description of the sample construction and of all variables is contained in the Appendix.

For each contest and bidder, we merge the Thomson data with financial and accounting information from the CRSP Monthly Stock and the CRSP/Compustat Merged databases, using monthly data for stock returns, and both quarterly and yearly data for accounting

[6] We do not consider non-binding bids, which are often made during the initial, private stage of the takeover bidding process (see Boone and Mulherin (2007)), since bidders with a serious interest in the acquisition are more likely to be similar ex ante, consistent with our identification strategy. We note that the focus on the 'surviving' group of bidders with serious interest is also likely to help with identification and convergences in terms of the characteristics of the winner and loser.

items from three years before to three years after the contest. We eliminate observations that can or should not be matched, such as repeated bids of the same bidder for the same target, contests that are not completed, or firms without CRSP permno. At the same time, we take caution to reduce survivorship bias. When bidders disappear from CRSP in the three-year period after a merger due to delisting, we calculate the return implications of the delisting events for shareholders using all delisting information available in CRSP. (Details are in the Appendix). The final sample, which we denote as the *Full Sample*, contains 16,632 event-time observations from 231 bidders, 111 winners and 120 losers. Table A-1 summarizes the construction of our data set. Appendix-Figure A-1 shows the frequency distribution of contests over the sample period, and illustrates the spikes in merger activity during the mid-1980s and mid-1990s.

In addition, we construct a "refined" version of the *Full Sample*. We manually search for press mention of each bid reported in the Thomson data in the *Financial Times*, *Wall Street Journal*, and *Washington Post*. We are able to find press mentions for 194 out of the 231 bids in the *Full Sample*. For 180 bids (87 contests, with 87 winnings bids and 93 losing bids), at least one article mentions the competing bids identified by Thomson. We refer to this latter sample as the *Contest Coverage Sample*. We estimate our empirical model both on the *Full Sample* and on this smaller *Contest Coverage Sample*.

We supplement the merger and stock data with analyst forecasts from I/B/E/S. We extract two-years-ahead consensus (mean) earnings forecasts for the 36 months before (and, for completeness, the 36 months after) the merger from the I/B/E/S summary history file using the 8-digit CUSIP as identifier.[7] We construct the forecasted earnings-to-price ratios as the consensus forecast divided by the stock price at the end of the month. Our sample includes forecasts for 180 firms, 82.2% of our total sample.[8]

[7] In addition to the mean (MEANEST), we also extract the median forecast of all analysts covering the firm (MEDEST) as well as forecasts of various other horizons, all of which yield very similar results.

[8] Consensus forecasts are not necessarily available for every month within the +/- three years around

We also test whether the estimated merger effects are present in a broader international sample. We include bids from companies that are headquartered outside North America. The vast majority of contested mergers and acquisitions outside North America that are recorded in Thomson One come from ten additional countries: Australia, France, Germany, Italy, Japan, the Netherlands, Spain, Switzerland, Sweden, and the United Kingdom. Including the U.S. and Canada, these countries cover more than 90% of all contested bids recorded in Thomson One. For these international cases, we turn to Compustat Global and Datastream for the corresponding stock data, as described in more detail in the Appendix. Our final *International Sample* contains an additional 72 contests and 152 bids (see Appendix-Table A-2).

We acknowledge that the data for international bidders is significantly less reliable and coverage is much less comprehensive than that of North American firms, as already discussed in prior research on similar data (e.g., Ince and Porter (2006)), in particular for data prior to the year 2000. We attempt to address these concerns and minimize the effect of noise by winsorizing BHARs at the 1%-level when we use the *International Sample*. Moreover, we will generally focus our analysis on the two North American samples.

The contest focus of our analysis requires a specific data architecture. For all three data sets, we construct an event time variable t that counts the months relative to a contest. As illustrated in Figure 1(a), we set $t = 0$ at the end of the month preceding the start of the contest, i.e., preceding the earliest bid. The end of the month prior to that is $t = -1$; the end of the month before that is $t = -2$, etc. Going forward, we set $t = +1$ at the end of the month in which the contest ends, that is, in which the merger is completed. The end of the following month is $+2$, the end of the month after that is $+3$, etc. Hence, event-time periods before and after the merger contest are exactly one month long, but the period

a merger. On average, 171 firms are covered in each event-time period. As in previous literature, we drop observations with negative forecasted earnings (see, e.g., Richardson, Sloan, and You (2011)).

from $t = 0$ to $t = 1$ is of variable length, depending on the duration of the merger contest. Figure 1(b) provides a concrete example from our data, the merger contest between Westcott Communications and Automatic Data Processing for Sandy Corporation.

[Figure 1 approximately here]

1.2. Summary Statistics

Table 1 provides the summary statistics of our main data, the *Full Sample*. The bidder statistics (Panel A) are based on balance sheet and income data from the end of the fiscal year preceding the contest. The first three rows indicate that both winners and losers are large compared to the average Compustat firm.[9] This reflects the fact that acquiring (and public) firms tend to be larger than non-acquiring (and private) firms. The table also shows that winners tend to be larger than losers, though the size difference is only marginally significant, and small compared to the difference between the average acquiring and non-acquiring firm in Compustat. The differences between winners and losers in terms of firm characteristics such as Tobin's Q, PP&E, profitability, book leverage, and market leverage are very small and statistically insignificant. The similarity of winners and losers in observable characteristics is a first indication that losers may be a valid counterfactual for winners.

The last two rows of Panel A report the three-day announcement CARs, in percentage and in dollar terms. Percentage announcement returns are negative for winners and zero for losers, but the winner-loser differences are not significant at conventional levels. In contrast, the winner-loser difference in dollar announcement returns is weakly significant.

[Table 1 approximately here]

Panel B reports deal characteristics of the completed transactions. The first three rows show that the transaction values of contested mergers are large compared to the size of the

[9] Average total assets of Compustat firms during our sample period are \$5.3bn; the median is \$170.7m.

bidding firms involved, on average 39 percent of the losers' book assets and 24 percent of the winners' book assets. None of the completed deals in our sample results from a tender offer. This is more common in non-contested mergers. Deal attitude (hostile or friendly), and means of payment (stock, cash, or other means) do not differ markedly from those found in uncontested mergers (see, e.g. Betton, Eckbo, and Thorburn (2008)). 79 out of 111 cases involve two competing bidders, 25 cases involve three bidders, and seven contests involve more than three bidders.[10]

The average offer premium in our sample is 63 percent if expressed as a percentage of the target's market capitalization, and 10 percent if expressed as a percentage of the acquiror's market capitalization. We compute the takeover premium as a percentage of the acquiror's market capitalization to assess whether overpayment could potentially have a substantial effect on the acquiring shareholders' equity value. In our sample, takeover premia are larger than in a typical sample of non-contested bids (for example, 48 percent relative to target value in a sample of 4,889 bids for US targets during 1980-2002, analyzed by Betton, Eckbo, and Thorburn (2008)) and may indicate overbidding, or winner's curse, brought about by the competing offers. Below we explore this possibility in more detail.

1.3. Identifying Close Contests

In order to identify the causal effect of mergers, comparing the performance of companies who bid and won to companies who bid for the same target and lost is likely to be a better comparison than comparing winners to other companies in the market. Yet, not all takeover contests necessarily provide a good source of identification. After all, there is a reason why one of the competing companies eventually prevails.

[10] Note that our analysis employs a lower number of bidders than those that actually participate in the contest because not all bidders are public firms and, hence, no stock data is available. If we exclude all bidders with missing stock data, we have 104 contests with two bidders, seven contests with three bidders, and one contest with four bidders.

Within the group of contested mergers, we seek to identify those contests that most credibly allow for a causal interpretation. In an ideal empirical scenario, a "coin toss" would determine the winning bidder. In order to approximate this scenario, we distinguish between close takeover contests, where winning and losing bidders have a similar ex-ante chance of winning, and contests where the chances of different bidders differ substantially. By maximizing the similarity between bidders, including their likelihood of acquiring a specific target at a specific point in time, we aim to identify the most credible setting for a causal interpretation.

Our main approach to identifying "close contests" relies on contest duration. In our data, some contests are resolved swiftly, with the first bidder withdrawing the initial offer shortly after the competing bidder enters the picture. Other contests involve a longer back-and-forth of bids and counter-bids. As shown in Panel B of Table 1, the average contest duration in our sample, defined as the time between announcement of the first bid and deal completion, is 8.65 months. Merger contests with above-median duration last six months to over two years, with a mean of 12.79 months. Merger contests with below-median duration, instead, last on average 4.39 months, which is similar to the duration of non-contested mergers. We will identify close contests as those with above-median duration.[11] That is, we make the identifying assumption that, in protracted contests of above-median duration, the ex-ante chances of winning faced by winners and losers are more similar to each other than in shorter contests.

We provide both a theoretical underpinning and several pieces of empirical evidence for our approach. We first sketch the theoretical argument for why protracted contests tend to feature bidders that are more similar in terms of their ex-ante chances to win the contest. We use a simple model of costly sequential bidding with independent private values.[12] For

[11] We also run all of our empirical models using alternative sample splits, i.e., terciles and quartiles of contest duration. We find similar results on these subsamples (available upon request).

[12] More generally, takeovers naturally have both a common-value element, namely, the stand-alone value

simplicity, we consider the (typical) case of two bidders competing for a given target. Assume that both bidders know their own valuation but not the valuation their competitor assigns to the target. Instead, they only know an interval of possible valuations that contains the true valuation of their competitor. Intuitively, the interval represents a "realistic guess" about the range of values that might capture their competitor's willingness to pay. We will consider the case of fixed-width intervals.[13]

This setting allows us to consider two bidding scenarios. First, consider the case that valuations are dissimilar enough so that the competitor's range of plausible valuations does not contain the initial bidder's own valuation. Whenever this is the case, the initial bidder will drop out as soon as the second, higher-interval bidder enters, since the second bidder would ultimately outbid the initial bidder and in order to save on bidding costs.[14] For example, depending on further assumptions on target shareholders' outside option, the first bidder may have submitted a bid equal to the stand-alone value of the target. For the second bidder it suffices to enter the auction and increase the outstanding bid by some minimum increment to stop the initial bidder from continuing to bid. Hence, we predict bidding contests to end swiftly after the competitor submitted a bid when winner and loser differ in their valuation and thus their likelihood to win the contest.

Second, consider the case that valuations are similar enough for the valuation intervals to be overlapping. After the initial bid of the first bidder, the second bidder enters only if his true valuation lies above the lower bound of the first bidder's possible valuations. And the first bidder continues the contest only if his true valuation lies above the lower bound of the second bidder's possible valuations.[15] In this case, the contest lasts longer. Hence,

of the target, and a private-value element, mostly the synergies arising from the potential takeover. Here we focus on the latter element.

[13] An alternative route would be to allow for varying interval widths, where the width reflects uncertainty about the other bidder's valuation and will affect the length of the bidding contest.

[14] The first bidder is endogenously the lower-value bidder since a competing bidder would not enter the contest after a bid of a high-interval bidder whose lower bound lies above the own true valuation.

[15] The exact size of the bidding costs determines whether we will observe jump bidding and the length of

similarity in bidders' private values of the takeover target (overlapping value intervals) and thus similarity in target valuation leads to protracted bidding.

Of course, there are plausible alternative models in which longer contest duration is not correlated with bidder similarity and the ex-ante likelihood of winning the contest. For example, contest duration might reflect uncertainty about bidders' own target valuations. Yet other models generate a similar correlation between close bids and contest duration with a similar underlying intuition, but a different set of theoretical assumptions, such as models of preemptive high bids that deter competitors (see Fishman (1988)). Whether protracted contests feature more similar bidders or not is thus an empirical question. We provide preliminary evidence below, and show formal tests in Section 3.1.

To provide empirical underpinning for our approach, we start from an inspection of press accounts about long and short merger contests confirms our intuition. Using the *Financial Times*, *Wall Street Journal*, and *Washington Post* articles described above, we find that, in short-duration contests, one bidder typically withdraws the bid quickly after the competing bid comes in, suggesting that the withdrawing company does not see much of a chance to win. By contrast, long-duration contests often involve multiple bidding rounds in which both bidders raise their initial bid in response to the competitor's most recent offer, sometimes several times. In the long-duration half of our sample, 43% of the bids are withdrawn only after a higher bid by at least one of the potential acquirors. In the short-duration sample, this is the case in just 24% of the contests. Instead, in the short-duration sample, 62% of losers withdraw shortly after placement of the competing bid, but only 24% do so in the long-duration contests.[16] Thus, the longer duration appears to indicate that neither offer clearly dominates, and that target management or target shareholders take both bids seriously. It

the contest.

[16] We also check whether the contest duration is driven by the time between withdrawal of the last competing bid and the completion. Though the time from withdrawal of the last competing bid to the completion of the merger is longer for long-duration contests, long- and short-duration contests differ significantly in the time during which at least two bids are active (108 vs 45 days).

is precisely in these contests that we expect the maximum similarity between winners and losers, and hence the loser's performance to provide a valid counterfactual for the winner's performance.

In Section 3.1, we will provide more formal tests. We will show that, before the merger contest begins, winner and loser are more similar in terms of market returns and analyst expectations in long contests than in short contests. We will also report the estimation results of an alternative approach based on a different identification assumption. Here, we define a contest as "close" if at least one bidder has sweetened their offer in response to a competing bid. As we will show, these estimates of the merger effect are similar to those reported for our main measure.

2. Econometric Model

2.1. The Effect of Mergers on Acquiror Returns

We evaluate winner-loser differences in abnormal performance over the three years prior to and the three years after the merger contest using a controlled regression framework. We compute buy-and-hold abnormal returns (BHARs) for each month in the +/- three-year event window separately for each bidder. The BHAR is calculated as the difference between the cumulated bidder stock return and a cumulated benchmark return, starting from 0 at $t = 0$. Cumulating forward, this amounts to:

$$BHAR_{ijt} = \prod_{s=1}^{t}(1 + r_{ijs}) - \prod_{s=1}^{t}(1 + r_{ijs}^{bm}), \tag{1}$$

where i denotes the bidder, j the bidding contest, t and s index event time, r_{ijs} is the bidder's stock return earned in event period s, i.e., over the time interval from $s-1$ to s (including all distributions), and r_{ijs}^{bm} is the benchmark return in event period s.[17] Recall that event

[17] Cumulating backward, this corresponds to $BHAR_{ijt} = \prod_{s=0}^{t+1}(1 + r_{ijs})^{-1} - \prod_{s=0}^{t+1}(1 + r_{ijs}^{bm})^{-1}$ for $t < 0$.

time is defined such that $t = 0$ indicates the end of the month preceding the start of the merger contest, and $t = 1$ the end of the month of merger completion. Hence, the return at $t = 1$ captures the performance over the whole (variable-length) contest, collapsed into one event period. It includes the stock price reactions at bid announcements as well as at contest resolution. After $t = 1$ and before $t = 0$, event time proceeds in steps of calendar months and, hence, r_{ijt} corresponds to the respective calendar-month return.

We use three standard benchmarks for normal returns: (1) the value-weighted market return, r_{mt} (as our baseline benchmark); (2) the value-weighted industry return, r_{ikt}, where k is the industry of bidder i based on the Fama-French 12-industry classification; and (3) the CAPM expected return, $r_{ft} + \beta_{ij}(r_{mt} - r_{ft})$, where r_{ft} is the risk free rate and β_{ij} is bidder i's beta in the event window around contest j.[18] We call the adjusted performance measures market-adjusted, industry-adjusted, and risk-adjusted BHARs, respectively. Note that the BHARs account for calendar time-specific shocks since they net out the cumulated benchmark return realized over the same calendar period.

When calculating risk-adjusted returns we estimate each bidder's beta separately for the pre- and the post-merger periods based on monthly returns. By doing so, we adjust for the mechanical change of the winner's beta brought about by the merger – from that of the pre-merger, stand-alone company to the weighted average of the acquiror's and the target's beta after the merger.

We evaluate the winner-loser differences in abnormal performance using the following

[18] We have also used value-weighted, characteristics-matched portfolio returns as a benchmark (Daniel, Grinblatt, Titman, and Wermers (1997); Wermers (2004)), which generates even stronger results. We do not report them in the paper since Wermers' data on the size, book-to-market, and twelve-month momentum triple sort, ends in 2012, which further reduces the sample size. However, using characteristics-based returns of portfolios matched only on size and book-to-market (available from Ken French's webpage for the entire sample period), we also replicate the estimation and generate even stronger results, available upon request.

regression equation, akin to the approach in Greenstone, Hornbeck, and Moretti (2010):

$$BHAR_{ijt} = \sum_{t'=\underline{T}}^{\overline{T}} \pi_{t'}^{W} W_{ijt}^{t'} + \sum_{t'=\underline{T}}^{\overline{T}} \pi_{t'}^{L} L_{ijt}^{t'} + \eta_j + \varepsilon_{ijt}. \tag{2}$$

The key independent variables are the two sets of indicators $W_{ijt}^{t'}$ and $L_{ijt}^{t'}$. $W_{ijt}^{t'}$ equals 1 if event time t equals t' and bidder i is a winner in contest j, i.e., $W_{ijt}^{t'} = 1_{\{t=t' \text{ and } i \text{ wins contest } j\}}$. $L_{ijt}^{t'}$ is an equivalent set of loser-event time dummies, i.e., $L_{ijt}^{t'} = 1_{\{t=t' \text{ and } i \text{ loses contest } j\}}$. Thus, our specification allows the effects of the winner and of the loser status to vary with event time, and the coefficients $\pi_{t'}^{W}$ ($\pi_{t'}^{L}$) measure the average winner (loser) return at event time t'. For example, π_3^W is the conditional mean of the winners' BHARs three months after the end of the bidding contest, and π_3^L is the conditional mean of the loser BHARs three months after the merger.[19]

The vector η_j is a full set of contest fixed effects, i.e., of indicator variables for each merger contest, and hence absorbs heterogeneity in the level of the outcome variable across contests, and ε_{ijt} is a stochastic error term. The inclusion of contest fixed effects guarantees that the π-series is identified from comparisons within a winner-loser pair. Thus, we retain the intuitive appeal of pairwise differencing in a regression framework. We also note that the inclusion of calendar year-month fixed effects to account for time varying shocks is redundant when using abnormal returns because abnormal returns already account for period-specific shocks.[20]

Equation (2) yields 72 coefficients for winners and 72 for losers – one for each month in the

[19] Note that some firms are winners and/or losers more than once, and observations from these firms simultaneously identify multiple π's.

[20] Abnormal returns adjust in fact more finely than time fixed effects since they account for the firm's varying exposure, e.g., the firm's risk exposure in the case of risk-adjusted abnormal returns. For completeness, we have tested and confirmed that the inclusion of year-month dummies does not alter the economic or statistical significance of the coefficient estimates. The abnormal return calculations and normalization of the BHAR relative to $t = 0$ also limit the variation in the level of the outcome variable across contests. Any remaining variation is absorbed by the contest fixed effects; but the difference in results between specifications using and omitting contest fixed effects is negligible.

three years prior to and after the merger. This detailed information is useful for graphically assessing the evolution of winners' and losers' performance over time. However, in order to perform statistical tests of the merger effects, we need a more parsimonious version with few interpretable coefficients. We estimate the following piecewise-linear approximation:

$$\begin{aligned} BHAR_{ijt} = {} & \alpha_1 W_{ijt} + \alpha_2\, t_{ijt} + \alpha_3\, t_{ijt}\, W_{ijt} + \alpha_4\, Post_{ijt} + \alpha_5\, Post_{ijt}\, W_{ijt} \\ & + \alpha_6\, t_{ijt}\, Post_{ijt} + \alpha_7\, t_{ijt}\, Post_{ijt}\, W_{ijt} + \eta_j + \varepsilon_{ijt}. \end{aligned} \tag{3}$$

The independent variables in Equation 3 are a winner dummy $W_{ijt} = 1_{\{i \text{ wins contest } j\}}$, a dummy indicating the post-merger period, $Post_{ijt} = 1_{\{t \text{ lies in post-merger period}\}}$, the trend variable t, all interactions between these variables as well as contest fixed effects, η_j. The contest fixed effects estimate the performance of the losing firm at $t = -36$, and all other coefficients measure differences against that number. The specification allows for different levels of loser performance before and after the merger ($\alpha_4 Post$) as well as for winner-loser differences in performance levels pre- and post-merger ($\alpha_1 W$ and $\alpha_5 Post\, W$). It also accounts for two separate linear time trends in the pre-merger and post-merger periods ($\alpha_2 t$ and $\alpha_6 t\, Post$), and for winners deviating from these trends, separately in the pre-merger and in the post-merger periods ($\alpha_3 t\, W$ and $\alpha_7 t\, Post\, W$). We estimate the value effect of mergers as the long-run performance difference between winners and losers at $t = 36$, $\hat{\alpha}_1 + \hat{\alpha}_5 + 35 \cdot (\hat{\alpha}_3 + \hat{\alpha}_7)$. We account for possible serial correlation and correlation within winner-loser pairs by clustering standard errors by contest. Clustering by contest also accounts for heteroscedasticity in the error terms across contests. Such heteroscedasticity can arise in our setting because, being a cumulated return, the dependent variable tends to be larger in magnitude and variance for longer contests, and hence the variance of the error term also increases in contest duration.[21]

[21] Another type of heterscedasticity concerns the distance to the contest. Since our return variable is a buy-and-hold return, cumulated forward and backward starting at $t = 0$, its magnitude and variance increase

A major advantage of the regression approach above is that it fully leverages the underlying contest-specific matching of firms and, at the same time, flexibly allows for any remaining differences between matched firms to vary over time. The more standard approach in the literature, running calendar time portfolio regressions, cannot be implemented in our setting. The matching requirements leave us with too few firms to form calendar-month portfolios that are long in the winning bidders' stocks and short in the losing bidders' stocks. The long or short portfolios would often contain just one or otherwise very few stocks. As a result, the estimates would become unreliable and depend on minimum-portfolio requirements.

2.2. Is There an Effect of Mergers on Losers?

An important consideration in assessing our identification strategy is whether the merger affects the loser's profitability directly. For example, a positive winner-loser differential might (also) reflect that the merger has weakened the loser's market power. Or, if we estimate a negative winner-loser differential it might reflect that the merger transaction triggers changes inside the loser firm that improve the loser's profitability.[22] Any such "direct loser effects" are not of concern if losing the merger contest affects the loser in a similar manner to how it would have affected the winner had the winner (counterfactually) lost the contest. To the contrary, our empirical approach aims to capture all performance implications of the merger,

in the distance to $t = 0$, and hence the variance of the error term increases in the same manner. This type of heteroscedasticity can be addressed by weighted least squares, where the weights are the inverse of the distance to contest. We have implemented this approach, retaining the clustering by contest. The results using weighted least squares show only negligible differences in magnitude and significance of the merger effect compared to the unweighted regressions of Table 3.

[22] As an example of positive loser effects consider the Continental-United and Delta-Northwest airline mergers which are both expected to benefit the non-merging airlines. Theoretically, in both a Cournot oligopoly model and a differentiated-products Bertrand model, the non-merging firm could benefit if the synergy or efficiency effects of the merger are not very large. Salant, Switzer, and Reynolds (1983) conclude that in general, a merger is not profitable in a Cournot oligopoly, with the exception of two duopolists that become a monopoly. Subsequent literature has identified some limits of this result. Deneckere and Davidson (1984) argue that the existence of product differentiation can result in the merged firm producing all the output of its pre-merger parts. Perry and Porter (1985) identify many circumstances in which an incentive to merge exists, even though the product is homogeneous.

including those that reflect the rest of the industry re-optimizing in response to the merger.

Hypothetical direct loser effects are a concern only if they were to differ for winners had they counterfactually lost the contest. (This concern mirrors our discussion of the identification of abnormal returns and winner-loser comparability, here with an emphasis on the loser post-merger.) First, consider the possibility of a loser-specific disadvantage. There are two versions, (i) the scenario that a merger hurts the performance of the loser, and that it hurts it more than it would have hurt the performance of the winner had the winner lost the contest; and (ii) that losing improves returns, but the (actual) loser's performance improves less than the winner's performance would have improved had he lost the contest. If either version were true, it would strengthen our main finding, namely the estimation of a negative merger effect. The measured merger effect would have been even more negative in the absence of the loser effect and, hence, our estimates would provide a conservative lower bound.

Second, consider the opposite concern, a loser-specific advantage. There are again two versions, (i) the possibility that mergers hamper the performance of the loser but less than it would hamper the winner, and (ii) the possibility that losing improves the loser's performance more than it would have affected the winner's performance had the winner lost the contest. Case (ii) is the empirically relevant one as we will estimate positive abnormal loser performance. However, while there are models predicting positive returns to *not* participating in a merger of competing companies (cf. Stigler (1950)), it is hard to see how this class of models could apply in our case. The bidders in our sample engage in deliberate and protracted battles to prevail in the merger, which is inconsistent with systematic loser-specific advantages to losing.[23] We conclude that in our context, asymmetric loser effects in either

[23] Consider for example the scenario discussed by Stigler (1950), in which the acquiror reduces post-merger output to a level below the combined output of its pre-merger parts and industry prices increase. Firms that did not merge may then expand output and profit from the higher prices. As Stigler argues "the major difficulty in forming [such] a merger is that it is more profitable to be outside ... than to be a participant." Hence, he argues the firm pursuing the merger will need to get "much encouragement from each firm—almost

direction are unlikely to be a concern.

3. Empirical Results

3.1. Are Winners and Losers Comparable Ex-Ante?

Table 1 indicated that winners and losers have similar observable characteristics, with the exception of a marginal difference in size. In this subsection, we go further in testing whether losers are a valid counterfactual for winners, and test for pre-merger similarities in their stock-market performance, in analyst forecasts of their future performance, and in their operating performance.

Stock prices are particularly suitable for probing the validity of our identification assumption because they capture pre-merger market expectations about future profitability. Unlike the variables in Table 1, which are only about current characteristics, stock valuations are forward looking. Thus, they can help us determine whether winner and loser profitability are comparable not just at the time of the announcement, but also in the foreseeable future. Any difference between winners and losers would indicate that the market expects future profitability to be different even without the merger contest.

Figure 2 plots the series of winner and loser π-coefficients from regression equation (2), estimated on the sample of close (long-duration) contests for the three measures of abnormal stock returns in Panels (a) to (c) and, for completeness, using raw returns in Panel (d).

[Figure 2 approximately here]

The graphs indicate that winning and losing firms display very similar performance paths in the three years before the contest, irrespective of the measure of performance. It is only after ther merger that winning and losing firms diverge. Statistical tests fail to reject the

every encouragement, in fact, except participation."

hypothesis that the pre-merger trends for winners and losers are equal. (The t-statistics are -0.19, -0.08, 0.37, and -0.42, respectively.)

Next we test for pre-merger differences using analyst forecasts. Analyst forecasts capture expectations about future profitability by well informed professional experts. While highly correlated with stock performance, they are not identical.

Figure 3 shows analyst earnings forecasts separately for winners and losers around the contest. We use the two-year consensus forecast scaled by the stock price at the end of the forecast month, computed using quarterly data from I/B/E/S. Just as the BHARs in Figure 2 above, the forecasted earnings-to-price ratios are normalized to zero in $t = 0$. The figure shows, very similarly to the evidence on stock-market performance, that the paths of forecasted earnings of winners and losers are closely aligned in the three years before the contest.

[Figure 3 approximately here]

Finally, we turn to direct accounting measures of operating performance. We compute operating cash flows following Moeller, Schlingemann, and Stulz (2004) from quarterly Compustat data as the ratio of net sales minus cost of goods sold and selling, general and administrative expenses over lagged total assets. We substitute lagged total assets (in the denominator) with current total assets to avoid inflating the ratio in the quarter just before the merger,[24] though we remark that the results are virtually identical under either measure.

Figure 4 shows the evolution of operating cash-flows for winners and losers in long-duration contests over the three years around the merger. Winners and losers are fairly aligned and show no significant differences in operating performance before the merger. We

[24] The cash-flow (CF) ratio changes from CF(Acquiror)/Assets(Acquiror) before the merger to (CF(Acquiror)+CF(Target)) / (Assets(Acquiror)+Assets(Target)) after the merger. The one-quarter lag in the denominator could potentially bias this ratio for up to three months after the merger. In these cases, the denominator would be too small as it uses the aquirors assets before the merger, and hence does not include the targets assets.

observe a bit of run-up in the winner's cash flows prior to the merger, and will return to this pattern when investigating the channel for our main results in Section 4. For now, we re-iterate that our alternate definition of cash flows, using current assets in the denominator, rules out a merely mechanical reason for the run-up. Moreover, we observe the same pattern for a range of alternative measures of operating performance around mergers, following Healy, Palepu, and Ruback (1992). We also note that, after the merger, both the winners' and the losers' operating performance drop sharply, and their subsequent performance is even more closely aligned. We will return to the pattern around and after the merger in Section 4.

[Figure 4 approximately here]

The close alignment in the paths of these variables for winners and losers, and in particular in the forward-looking variables, lends further support to the identifying assumption. We can go one step further and conduct an even sharper test by analyzing the *pair-specific* alignment in paths. In Table 2 we correlate winners' abnormal performance trends prior to the merger (their pre-merger alphas) with their matched losers' abnormal performance trends over the same period. We estimate bidder-specific pre-merger alphas by regressing the pre-merger abnormal returns of each bidder on a constant, and then regress the winner alphas on matched-loser alphas.

Panel A shows the results for long-duration contests (column 1) and, for comparison, for the short-duration contests (column 2) in the *Full Sample*. We find that the correlation between pre-merger alphas of winners and losers is high and statistically significant. It is more than twice as large in long-duration contests as in the short-duration contests. A similar pattern can be observed for R-squared. In Panel B, we report the analogous results estimated on the *Contest Coverage Sample*, for which press reports confirm the contested nature of each deal. The results are even stronger. Going from long to short contests, the correlation in alphas and R-squared drops by even more than in the full sample. Thus, the results

indicate that winning bidders who experience abnormal run-ups (declines) during the three years preceding the merger are typically challenged by rival bidders who have experienced a similar run-up (decline), and that this similarity is most pronounced in contests of long duration.

These findings speak to the concern that contestants differ in their acquisition motives or prospects. Specifically, one may worry that bidders who are motivated by overvaluation of their own stock, possibly following a pre-merger run-up, systematically differ in their post-merger performance from bidders who did not experience a recent run-up. We find that pre-merger trends of both sets of bidders are closely aligned in the sample of long-duration contests.

[Table 2 approximately here]

The results for the *International Sample* in Panel C are less strong. Winners' performance is significantly correlated with the performance of their matched loser, though somewhat less strongly than in the two U.S. samples. This is perhaps not surprising since the *International Sample* contains bidder pairs with companies from different countries. We therefore consider our estimates of the effect of mergers to be most accurate on the two U.S. samples.

Overall, the evidence presented in this subsection indicates that losers represent a plausible counterfactual for winners in long-duration contests. Before the contest, the market expects them to perform similarly in the future. In addition, winners and losers are similar in terms of accounting measures of profitability and other firm characteristics.

3.2. Estimates of the Effect of Mergers

Figure 2 provides a visual description of our main finding. In the months leading up to the contest, winner and loser performance is quite similar. After the contest, however, winner and loser returns begin to diverge. Losers of a close bidding contest display positive abnormal

performance, with an initial jump between the beginning and the end of the contest, and a continued upward trend in the three years after the merger. In contrast, winners display no or negative abnormal performance after the merger.[25] In other words, the shareholders of the acquiring company would have been better off under the hypothetical counterfactual in which their company lost the merger contest.

Table 3 quantifies the magnitude of the effect, separately for close contests in the *Full Sample*, the *Contest Coverage Sample*, and the *International Sample*. The table reports the coefficient estimates of equation (3) as well as the estimated cumulative merger effect at $t = 36$, or three years after the contest, at the bottom of the table.

[Table 3 approximately here]

Table 3 reports that the coefficient estimate α_3, which measures the pre-merger trend difference in winner and loser performance, is insignificant in all three samples. Consistent with the evidence in Figure 2 and Table 2, winner and loser returns are statistically indistinguishable at conventional levels during the 36 months leading up to the merger.

The estimated merger effect, instead, is significantly negative. In the *Full Sample*, reported in column (1), the cumulative underperformance of winners from the beginning of the contest to the end of three years after merger completion is 23.7% percent. That is, consistent with the visual evidence, the estimates in the table suggest that winners fare significantly worse than losers and that the difference is statistically significant. In the *Contest Coverage Sample*, which includes only contests validated by the newspapers articles, the effect is even larger, -32.1%. Despite the reduction in sample size, the effect is statistically highly significant. We also note that, in both samples the coefficient α_7 is insignificantly negative, indicating that the underperformance of winners relative to losers does not decrease

[25] We note that the positive performance of both winners and losers shown in Panel (d) for period 1 reflects that Panel (d) shows *unadjusted* returns over several months, from the beginning to the end of the contest.

over time. Finally, in the *International Sample*, analyzed in column (3), the estimate of the merger effect is smaller, −13.6%, but still statistically significant (p-value = 0.05).

The results are economically large also when expressed in dollar terms. The average dollar decline in market capitalization of winners relative to losers is $2,034m for the *Full Sample*, $2,711m for the *Contest Coverage Sample*, and $1,127m for the *International Sample*.

Our results are robust to employing alternative return adjustments. In Table 4, we use the definitions of stock performance and abnormal returns from the variations in Panels (b), (c), and (d) of Figure 2, both for the *Full Sample* and for the *Contest Coverage Sample*. As the table reveals, our results are insensitive to employing alternative return adjustments. In the remainder of the paper, we focus on market-adjusted returns.

[Table 4 approximately here]

As another robustness check, we have also estimated models that use an alternative definition of contested mergers: We include contests of any duration and define a fight as "close" if at least one bidder has sweetened their offer in response to a competing bid. Our estimates are consistent with those reported in Table 3. In particular, the cumulative underperformance of winners from the beginning of the contest to the end of three years after merger completion is −30.13 (14.87), −29.38 (16.41), −36.72 (18.42), and −23.75 (16.89) for market-adjusted, industry-adjusted, risk-adjusted, and raw returns (and their standard errors), respectively.[26]

We conclude that, on average, acquirors significantly and robustly underperform relative to similar companies who attempted but failed to acquire the same target.

[26] For completeness, we report the estimates for the short-duration subsample in Appendix Table A-3. We find no significant differences in post-merger performance between winners and losers. As discussed above, it is unclear how to draw inferences about the causal effect of mergers from this sample of contests. We have also estimated the incremental effect of increasing contest duration by one year. In pooled regressions, we interact all variables with contest duration. We find that an increase in contest duration by one year is associated with additional value destruction of 46.20 percent. While these estimates are consistent with our estimates in the long-duration subsample, they lack a causal interpretation due to differential sorting of winning and losing bidders in the short-duration subsample, and are reported only for completeness.

3.3. Additional Estimates

Our approach to estimating long-run abnormal returns to mergers as winner-loser differences allows us to address concerns about unobserved determinants that affect estimates in the prior literature, where the comparison firms are not pursuing similar mergers. This improvement comes at the cost of sample specificity—our results apply to contested mergers, and we cannot easily generalize to a broader sample of acquisitions.

In this section, we first focus on two questions that are commonly explored in existing merger studies: Are the return estimates affected by prior over- or undervaluation of the acquiror? And, are the return estimates different for private versus public targets? We then explore the role of sample specificity, i.e., of the more competitive setting of contested mergers, and analyze how the estimated merger effect varies with the degree of competitiveness: How do the return estimates vary as the number of bidders increases and when the acquisition premium is particularly high?

Acquiror Q. Our approach to estimating long-run abnormal returns to mergers allows us to address concerns about the prior finding that highly-valued acquirors tend to underperform in the long run, relative to a characteristics-matched firm portfolio (see., e.g., Rau and Vermaelen (1998)). The concern is that the subsequent reversal in acquirors' market valuation might not be not caused by the merger, but would have occurred even in the absence of the takeover. For example, temporarily overvalued firms might choose to acquire less highly valued targets, possibly to attenuate the reversal in their (over-)valuation (Shleifer and Vishny (2003), Rhodes-Kropf and Viswanathan (2004)). Our setting alleviates such selection concerns since we benchmark winners against losing bidders who show close similarity in Tobin's Q before the merger (see Table 1), in abnormal pre-merger run-ups or declines in stock price (see Table 2), in pre-merger analyst forecasts (see Figure 3), and in pre-merger operating performance (see Figure 4).

We now test whether, in such a controlled setting, the acquiror's Q still predicts the returns to mergers. We extend our baseline regression model from equation (3) to include a full set of interaction terms with a dummy variable indicating the subsample of acquirors with above-median market-to-book ratio.[27] The estimation results are shown in column (1) of Table 5. At the bottom of the table, we report the merger effect for acquirors with below-median Q, above-median Q, as well as the difference.

We find no difference in the merger effects estimated for highly and less-highly valued acquirors, compared to the post-merger performance of similarly valued competing bidders. The difference in performance between high-Q and low-Q acquirors is small and insignificant (6.25%, p-value: 0.77). We also fail to reject the null hypothesis of no significant merger effects within the high-Q subsample. That is, mergers of high-Q acquirors in our sample do not appear to be value-destroying when benchmarked against the close-bidder counterfactual.

We note that, if we employ instead a more standard estimation approach, we *do* replicate the underperformance of high-Q acquirors. We implement the double-sort methodology of the original Rau and Vermaelen (1998) study by dynamically matching each acquiring firm-month to a Fama-French portfolio formed on size and book-to-market (5×5 portfolios). Relative to these benchmark returns, the long-run BHARs of high-Q acquirors in our sample are more negative than those of low-Q acquirors. The difference is 38.30 percent over the three years after the merger, which is broadly in line with Rau and Vermaelen (1998).

This methodological comparison indicates that prior estimation results might be affected by the lack of a proper counterfactual and may have to be interpreted with caution. That is, in prior estimations, acquisitions of high-Q firms might appear to be value-destroying since they are not benchmarked against the right counterfactual. We caution that our sample does not allow to speak directly to the merger effects estimated in prior studies as the data

[27] Note that the direct effect of the above-median subsample dummy (α_8) is subsumed in the contest fixed effect. Hence the variable is not included in the regression.

employed is different.

Public versus private acquisitions. In the same spirit, our estimation approach allows us to distinguish the return implications of public and private acquisitions, which is a common robustness check in prior studies estimating the returns to mergers. Existing large-sample studies show that announcement returns are significantly lower in acquisitions of public targets (Fuller, Netter, and Stegemoller, 2002; Betton, Eckbo, and Thorburn, 2008; Spalt and Schneider, 2016), and attribute this finding to private information about private targets (Makadok and Barney (2001)) or liquidity discounts for private targets (Fuller, Netter, and Stegemoller (2002)).

In column (2) of Table 5, we re-estimate merger effects separately for public and private companies using the same winner-loser methodology with interactions. The estimation results indicates that acquisitions of public firms appear to destroy value. The merger effect is statistically insignificant and positive (15.39%, p-value: 0.59) for acquisitions of private firms, but significantly negative (-34.03%, p-value: 0.00) for acquisitions of public firms. The estimated difference of -49.41 is economically large but has a p-value of 0.11.

These results are consistent with the prior literature but avoid its selection confounds, namely, that acquirors taking over public firms may differ in various aspects from firms that acquire private firms. In our setting, this selection problem is not present since winner and loser attempt to acquire the same firm—either both attempt to acquire a public firm or both attempt to acquire a private firm.

We now turn to analyzing the role of our specific, more competitive setting of contested mergers, and analyze how the estimated merger effect varies with the degree of competitiveness. We consider two measures of competitiveness, the number of bidders and the ultimate acquisition premium.

Number of Bidders. First, we re-estimate our model allowing for a differential effect

of merger completion in contests with a high number of bidders. We distinguish between contests with exactly two bidders and those with more bidders, and include the usual interaction effects with an indicator variable for "more than two bidders" into our estimating equation. The results are shown in column (3) of Table 5. We estimate a merger effect of -25.20% (p-value: 0.06) for contests with exactly two bidders, and -18.76% (p-value: 0.29) for contests with more than two bidders. The difference is insignificant. Hence, the estimated negative average merger effect in close contests does not appear to increase as bidding competition becomes more intense, alleviating somewhat the concern that our results might be exclusively driven by bidding competition in our specific sample. We also note that this result is consistent with prior large-sample studies that find no evidence that bidding competition decreases bidder returns (Moeller, Schlingemann, and Stulz, 2004; Spalt and Schneider, 2016).[28]

Acquisition premium. Another possible correlate of intense bidding competition is a high acquisition premium. High premia should mechanically induce underperformance of the acquiror to the extent that the premium exceeds the target's stand-alone value plus the expected synergies from the merger. Empirically, though, it is difficult to measure the expected-synergies component and thus true overpayment. Our empirical analysis is therefore limited to the role of the acquisition premium, defined as the difference between offer price and stand-alone value of the target, without consideration of synergies.

We calculate the offer premium as the run-up in the target's stock price from 40 trading days prior to the beginning of the contest until one day after completion. In column (4) of Table 5 we include a dummy variable indicating above-medium premia in the interaction terms. We estimate a large but insignificant merger effect of -24.86% for low-premium contests and an even larger and significant merger effect of -41.46% for high-premium contests.

[28] There is even evidence to the contrary for earlier-stage bidding in private negotiations (Boone and Mulherin, 2008).

The difference fails to be significant.

Overall, we fail to find significantly stronger underperformance in more competitive bidding contests. However, these results do not rule out that systematic differences between contested and uncontested mergers are significant in generating the abnormal negative effect.

4. Possible Mechanisms

In the previous section, we have found that the post-merger returns of winners and losers differ substantially. On average, losing appears to be better than winning from the perspective of acquiring-company shareholders. We now ask which mergers are particularly likely to generate negative abnormal returns. That is, can we identify the channels, features, or mechanisms determining the returns to mergers?

We will proceed in two steps. First, we demonstrate that the estimated negative average effect masks significant heterogeneity in the magnitude of the losses. Second, we develop and test hypotheses that might explain the estimated return implications. We also discuss to what extent the potential explanations might imply *positive* abnormal returns to the loser, rather than negative abnormal returns to the winner, as Figure 2 seems to indicate.

Heterogeneity. Figure 5 shows the dispersion of the estimated merger effects in our sample. In this figure, each contest is an observation, and the histogram shows the distribution of the estimated long-run winner-loser differences in BHARs across contests.

[Figure 5 approximately here]

The mass of the distribution is visibly shifted to the left of zero. This is not surprising, since we have shown that the effect for the mean contest is negative. But it is also clear that not all mergers result in an equal destruction of value. In our sample, 66% of mergers have a negative effect, and 34% have a positive effect. The 25%, 50% (median), and 75% percentiles are -68%, -19%, and 13%, respectively. Of course part of this dispersion likely

reflects small-sample noise in our estimates. Intuitively, this noise comes from the fact that we do not know the "true" merger effects, but instead can only estimate them. A weighted version of this distribution—with weights equal to the combined firm value of acquiror and target—is similar, though, indicating that not all the dispersion is noise.

What explains this heterogeneity then? To shed light on the economic channels that drive the negative merger-effect estimates, we consider three commonly raised hypotheses for the post-merger underperformance of acquirors. First, we consider the role of strategic flexibility. Strategic flexibility describes a firm's ability to commit the resources necessary to pursue competitive strategies. We test whether acquirors who have exhausted their access to liquidity because they cash-financed the merger or emerged with a high leverage ratio appear to be particularly hampered by the negative return implications of mergers, or appear to generate positive profit opportunities to their competitors.

A second common explanation for merger underperformance is that management (or the stock market) initially underestimates the integration costs entailed by the merger. The cost of integration likely depends on factors such as how distant the acquiror and the target are in terms of products, technologies, and corporate culture. While we do not have direct measures of integrations costs, we consider several factors that may be correlated with these costs, such as whether the target and the acquiror belong to the same industry; the size of the target relative to the the aquiror; and whether the bid is hostile or friendly.

A third hypothesis is that the underperformance of the merger reflects some pre-existing inefficiency in the target firm, which continues to be a drag on the merged firm's performance going forward, and which might have been underestimated by the acquiror ex ante. While we do not have direct measures of pre-existing target inefficiencies, we will utilize information on targets' operating and stock price performance and its pre-merger Q to test this hypothesis.

We stress that the analysis in this section should be interpreted as suggestive, rather

than definitive. For once, the empirical estimations are significantly more demanding than our baseline models as they require estimating a merger effect for specific sub-samples. Due to sample size, the estimates will necessarily be less precise than our baseline estimates.

Moreover, we do not *directly* observe strategic flexibility, integration costs or inefficiencies. Given the available data, we necessarily have to rely on *indirect* tests. At the same time, our tests are effective in that they also capture almost all of the characteristics associated with long-term post-merger performance in prior literature. As such, our analysis also serves to test whether any of these commonly-analyzed characteristics are systematically correlated with the size of the estimated winner-loser difference and thus help explain our findings, regardless of the interpretation attached to them.

We note that this part of the analysis implicit depends on some measure of market inefficiency. Indeed, any estimation of long-run abnormal returns caused by a one-time event implicitly assumes a certain degree of market inefficiency. This reliance on market inefficiency was present already in our baseline estimation: At the announcement, the market appears to systematically overestimate the returns to mergers; otherwise the winner price would fully adjust at announcement, on average, and we would not have estimated significant abnormal performance over the following three years. The analysis further assumes that any channel or mechanism we might detect was not anticipated by the market at the announcement.

A. Strategic Flexibility

Strategic flexibility includes both the ability to pursue *proactive* strategic choices, such as aggressive pricing, targeted advertising against rivals, or poaching of productive workers, and the ability to be *reactive* and respond to external competitive threats (Sushil (2015)). If acquirors lose flexibility relative to losers, this may help explain why losers in our sample tend to outperform the market, while winners lag behind. Winners may be sacrificing future financial flexibility when they complete the merger, in particular if they pay in cash and

lever up their company. As a result, they may then have to miss out on future investment opportunities, while the losing bidders are able to take advantage of them.

If loss of flexibility explains the underperformance of winners relative to losers, we predict that the estimated negative average merger effect is most pronounced in those cases were the loss in flexibility is particularly steep. To test this hypothesis, we examine acquirors' form of payment and post-merger leverage.

Form of Payment. First, we look at the form of payment—cash, stock, or other. Cash holdings offer more flexibility than any other less liquid asset, and may influence the product-market choices of a firm and its competitors for several reasons. A cash-rich firm can use its resources to finance competitive strategies. For instance, it can challenge rivals through aggressive pricing (Bolton and Scharfstein (1990)) and fund competitive choices such as the location of stores or plants, the construction of efficient distribution networks, targeted advertising against rivals, or the employment of more productive workers (Campello (2006)). A firm's stock of cash can also *signal* the possibility of aggressive behavior, thereby distorting competitors' actions in the product market. Accordingly, it functions as a preemptive device that affects rivals' entry or capacity expansion decisions (cf. Benoît (1984)).

We hypothesize, then, that if loss of flexibility plays a role in explaining our findings, winners that acquire a target through all-cash offers lose more flexibility than stock acquirors as they end up with lower cash reserves and therefore more binding credit constraints. We test whether cash acquirors therefore tend to under-perform more than otherwise identical winners that acquire a target through an all-stock offer.

Column (1) of Table 6 shows the estimated merger effect for all-cash offers versus those that use at least some stock. It is helpful for our analysis that the form of payment is very similar across winners and losers in our sample. The mean difference between winners and losers in the percentage of the transaction value offered in cash is only 0.49% in long-duration (close) contests. (For comparison, it is 14.40% in short contests.)

Consistent with our hypothesis, we find that, in the subset of deals that are at least partially financed with stock, the merger effect is small and marginally significant (−20.45%, p-value: 0.08), while it is almost three times as large and statistically more significant for all-cash deals (−56.46%, p-value: 0.01). The difference, however, fails to be significant at conventional levels (p-value: 0.15).

This result is noteworthy since much of the previous literature focuses on the (seeming) under-performance of stock mergers.[29] Our approach to identifying the merger effect reveals that, at least for the specific sample of contested mergers, this is not the case when acquirors are benchmarked against more comparable firms.

The large negative merger effect estimated for cash deals is also consistent with two alternative interpretations. One is the more negative view of cash deals that is commonly voiced among practitioners. Practitioners are often wary that, in deals settled in cash, the target simply seeks to cash out at the highest possible price, without long-term involvement and irrespective of the strategic fit of the merged entity. Under that scenario, many of these deals will result in poor long-run performance. By contrast, in deals settled in stock, the target has an economic interest in the subsequent performance of the merged company.

Another, complementary interpretation is proposed in the behavioral literature on mergers and acquisitions. As shown by Malmendier and Tate (2008), managers with overoptimistic expectations appear to engage in more merger transactions than their peers. Crucially, overconfidence is particularly likely to induce more and lower-performing mergers when the acquiror is cash-rich and can cash-finance the merger. As formalized and tested in Mal-

[29] Loughran and Vijh (1997) find that stock acquirors exhibit poor long-run abnormal returns relative to size and market-to-book matched firms, while cash acquirors outperform the matched firms. For short-run announcement returns, the evidence is less clear. Betton, Eckbo, and Thorburn (2008) find only slightly higher announcement returns for cash bids than for stock bids, and Spalt and Schneider (2016) find more negative announcement returns for stock bids than cash bids only in the subsample of public targets. We also caution that the Loughran and Vijh (1997) findings do not replicate in our data. We replicate their methodology by dynamically matching each acquiring firm-month to a Fama-French portfolio formed on size and book-to-market (5×5 portfolios). We find that the long-run BHARs of stock and of cash acquirors, relative to these benchmark returns, are not significantly different.

mendier and Tate (2005) and Malmendier, Tate, and Yan (2011), overconfidence implies a difference in opinion about the value of the firm between the overconfident managers and outside capital-providers, inducing a reluctance to raise stock financing. Both the practitioner view and the behavioral view are consistent with our finding that transactions performed at high cash prices appear to be drivers of the post-merger underperformance, as well as with the leverage results presented below.

Leverage. Another proxy for low strategic and financial flexibility is high leverage. Financial flexibility has been identified as a key driver of firms' capital structure decisions both theoretically (DeAngelo, DeAngelo, and Whited (2011)) and empirically (Lang, Ofek, and Stulz (1996), Marchica and Mura (2010)), including in surveys of CFOs (Bancel and Mittoo (2004), Brounen, De Jong, and Koedijk (2006), Graham and Harvey (2001)). Acquisitions that imply a sharp increase in acquiring firm's leverage may thus reduce the winner's future growth. Indeed, Penman, Richardson, and Tuna (2007), for example, find that leverage is negatively associated with future stock returns. We note that an increase in leverage may be connected to the method of payment, i.e., a heavier burden in cash-financed deals that rely on increasing debt obligations, but may also be driven by the leverage of the target. In the case of cash deals, acquirors may have to draw down their cash holdings and take on additional debt in order to finance the deal. We also note that increased net leverage may be viewed as potentially harmful by the market for other reasons. For example, a disregard of high leverage would also be consistent with both the practitioner view and the behavioral view on cash mergers outlined above.

We first compare winners' and losers' net leverage ratios both before and after the merger. Using quarterly data, we compute net market leverage as the ratio of net debt (short-term plus long-term debt minus cash and short-term investments) to market value (total assets minus book equity plus market equity) of the firm. Alternatively, we use net book leverage and industry-adjusted net book or market leverage; all measures yield very similar results.

Figure 6 shows the evolution of net market leverage for winners and losers. The graph indicates that winners' net leverage ratios sharply increase from pre- to post-merger. In the pre-merger period, the winners' leverage ratio is declining, and winners have a somewhat lower leverage ratio than losers just before the merger. The merger brings about a sharp increase (from 12 to 20 percent) in the winners' leverage ratio while losers' leverage remains stable. This difference persists until three years after the merger.

[Figure 6 approximately here]

However, when estimating regressions analogous to those of Table 6 – using a dummy interaction variable that equals one if the increase in the winner's leverage ratio from the pre-merger quarter to the post-merger quarter is above median – the difference of the effect is not significant at conventional levels. The merger effect is -29.95% (p-value: 0.08) for below-median leverage increase, -17.86% (p-value: 0.19) for above-median leverage increase, and the difference has a p-value of 0.58. Thus, we do not have definitive evidence on the effect of capital structure changes. The correlational evidence points to the possibility that high leverage ratios necessary to finance and implement the merger might be constraining the acquiror post-merger.[30] Alternatively, since the spike in leverage at the time of the merger is similar in all-cash and all-stock mergers, it might reflect leverage of the target which is assumed once the merger is completed.

We stress that neither form of payment, nor leverage are perfect predictors of flexibility. But taken together, the results are consistent with the hypothesis that strategic flexibility may play a role in explaining our findings.

B. Integration Costs

[30] Similar to the idea that the increased leverage allows bidders to win the bidding contest, Safieddine and Titman (1999) argue that, increased leverage allows targets of failed merger attempts to remain independent, though in this case possibly resulting from operational improvements.

The cost of post-merger integration is often cited as a key reason for poor post-merger performance, and the underestimation of these costs as one of the top mistakes companies make in acquisitions (Dean (2014), Finkelstein (2016)). The most commonly-mentioned underlying factor is "cultural differences," and Weber and Camerer (2003) have illustrated experimentally how different organizational cultures introduce merger costs. If integration costs and their underestimation are an important explanation for the merger effect, mergers where post-merger integration issues are more likely to arise should experience stronger underperformance.

We consider several possible factors contributing to the cost of integration: relatedness (in terms of industry), relative sizes, and hostility of the takeover. We emphasize again that these proxies are only indirect measures of integration costs. (At the same time, all of these proxies are commonly studied determinants of merger outcomes, and provide robustness checks of independent interest.)

Relatedness. A large strand of management and corporate-finance literature has shown that mergers between related firms tend to generate higher value than diversifying mergers (Chatterjee (1986), Singh and Montgomery (1987), Morck, Shleifer, and Vishny (1990), Cartwright and Cooper (1993)). Their definitions of relatedness are mainly concerned with similarities in production technology, scientific research, products and industries. More recent studies, however, find different results (Spalt and Schneider (2016), Akbulut and Matsusaka (2010)), which point to identification as an important issue: Firms that participate in diversifying mergers are different from firms engaging in concentrating acquisitions.

Our approach provides a novel opportunity to address this issue as winners and losers in our sample are very similar in their tendency to diversify or concentrate. We use the Fama-French 12-industry classification to distinguish related versus diversifying mergers. An acquisition bid is related if the bidder is in the same industry as the target, and it is diversifying otherwise. In the full sample, the winner and loser in a contest have different

diversification status (winner is diversifying, loser is concentrating or vice versa) in only 24 out of 112 contest. Moreover, in the subsample of close contests, diversification status between winners and losers differs in only eight out of 56 cases.

Column (2) of Table 6 shows the estimated merger effect for related offers versus diversifying offers. Consistent with most of the prior literature, our estimates indicate that, on average, diversifying mergers appear to destroy value. The merger effect is -69.79% (p-value: 0.06), while it is only -18.10% (p-value: 0.11) in mergers with related targets. However, the difference in the effect is not precisely estimated, making the evidence for a role of acquiror-target similarities and (possibly) their correlation with integration costs only suggestive.

Size. As a second correlate of the cost of integration, we look at size. Integration costs tend to be more severe the larger the target is relative to the acquiror, and the harder it is to transform the target's corporate culture. At the extreme end of the spectrum, a small firm acquiring a large target will incur significant costs training the target firm's employees to adhere to the acquiror's business practices. Indeed, large relative target size has been associated with significantly lower returns to mergers, at least in the short-run.[31] Here too, causal interpretation is complicated by the fact that acquirors of large target firms may be different from acquirors of small firms. For instance, mature firms with declining profits may choose to acquire large firms while young growth firms may tend to acquire small firms. In our setting, both the winning and the losing bidders try to acquire the same firm. Hence, there is no difference in absolute target size and there are only small differences in relative target size.

We hypothesize that, if integration costs play a role in explaining our findings, winners

[31] Alexandridis, Fuller, Terhaar, and Travlos (2013) find that the three-day bidder returns are 2.4% worse for large deals than for small deals. Spalt and Schneider (2016) find that target size is negatively related to three-day acquiror announcement returns if the target is public, but positively related to three-day bidder announcement returns if the target is private.

underperform more when they are relatively small compared to the target. We start from absolute size of the bidder. In column (3) of Table 6, we estimate the merger separately for acquirors with above-and below-median market capitalization. As shown at the bottom of the table, we find that the long-run merger performance of small acquirors is -26.59% and marginally significant (p-value = 0.09), while it is -20.87% and statistically insignificant (p-value = 0.18) for large acquirors. Hence, while small acquirors tend to perform worse, directionally, absolute acquiror size fails to predict abnormal post-merger performance.

A more direct test is based not on absolute acquiror size, but on the *relative* sizes of target and acquiror. We calculate relative target size as the ratio of the transaction value (or the offered purchase price in the case of the losing bidder) and the bidder's market capitalization. Column (4) of Table 6 shows the results of the corresponding estimation, where we split the sample in above-median and below-median relative target sizes. Consistent with the integration-cost hypothesis, we find that deals involving relatively large targets induce larger negative merger effects, though the difference is not significant: When acquirors are benchmarked against firms that bid for the same target, the merger effect amounts to -35.63% and is marginally significant (p-value: 0.07) for large targets, while it is only -17.23% (p-value: 0.08) for small targets. The role of relative target size is even more striking when we define target size as "large" relative to the acquiror if the transaction price paid is at least 50% of the acquirors firm value. Under this classification scheme, the merger effect for small targets is -17.53% (p-value: 0.13) and the effect for large targets is -61.24% (p-value: 0.02).

In summary, whether we consider relative target size as a proxy for the costs of integration or have alternative interpretations in mind, our empirical approach reveals that relatively large targets tend to predict large negative abnormal performance in the long run.

Hostile vs. Friendly. Another predictor of higher costs of integration is the hostility of a takeover bid. The corporate cultures of the target and acquiror and the respective groups

of employees will likely converge more slowly in cases where the takeover process was met with resistance on the side of the target company. Greenwood, Hinings, and Brown (1994) and Larsson and Finkelstein (1999), for example, argue that employee antipathy or hostility can negatively affect the post-merger outcomes. Hostile bids may also induce lower returns for other reasons, for example, because less information was ex-ante shared by the target with the acquiror, or because hostile bidders might have to bid higher than they would in a friendly takeover. We note, though, that there is no clear evidence of hostility-induced overbidding in existing large-sample studies (Betton, Eckbo, and Thorburn, 2008; Spalt and Schneider, 2016).

The results of the estimation are reported in column (5). We find that the merger effect for hostile bids is large (-100.70 with a p-value of 0.13) while friendly bids underperform much less (-19.45 with a p-value of 0.07). Despite the large difference in economic magnitude, the difference remains insignificant. One caveat here is that the number of hostile bids in our sample is low. As the summary statistics in Table 1.B reveal, hostile bids amount to less than one tenth of our *Full Sample* (ten out of 119 bids), and they are even less common in the subsample of close contests. One reason for this may be that hostile bids often induce targets to produce a white knight, and we exclude white knights from our sample due to their lack of comparability to other bidders.

C. Target Inefficiencies

Finally, we turn to preexisting underlying inefficencies within the target firm as a potential explanation for the estimated negative effect of mergers. We consider several proxies, including the target's operating performance, stock performance, and market-to-book ratio (see, e.g., Jensen and Ruback (1983) for a discussion of these proxies). We test for lingering effects of such inefficiencies, i.e., whether mergers involving more inefficient targets result in stronger underperformance than otherwise identical mergers that involve less inefficient

targets. We compare takeovers of targets that have been struggling for the *previous* three years with takovers of targets that have been doing well.

Stock performance. First, we classify pre-merger inefficiencies based on targets' abnormal stock price performance in the three years leading up to the merger. We distinguish between targets with below-median and above-median market-adjusted abnormal performance. The results are shown in column (6) of Table 6. We find that bidders acquiring underperforming targets do worse than bidders that acquire outperforming targets. The merger effect is large and highly significant for acquirors of low-performing targets, -53.36% (p-value: 0.01), but smaller and insignificant (-13.38%, p-value: 0.35) for targets with above-median stock performance of the three-year pre-merger window. The difference of 39.98% is marginally significant, with a p-value of 0.09.

Operating Performance. When we use measures of operating performance we do not find similar evidence. Here, we calculate pre-merger operating cash flow of the target similarly to Moeller, Schlingemann, and Stulz (2004) as net sales minus cost of goods sold and selling, general and administrative expenses, and express it as a percentage of total assets.[32] As shown in column (7), the estimated merger effect is -29.74% (p-value: 0.09) for targets of below-median profitability, and -31.06% (p-value: 0.12) for targets of above-median profitability.

We also refer back to Figure 4 on the acquiror's operating performance around the merger. We observed a sharp drop around the merger, which was however mirrored by the performance of the loser. Also over the 36 months after the merger, the winner's and the loser's operating performance appears to be closely aligned. One piece of suggestive evidence, however, is favor of pre-existing target inefficiencies might be the somewhat more pronounced run-up in cash flows in winner firms than in loser firms before the merger, combined with

[32] Moeller, Schlingemann, and Stulz (2004) further subtract the change in working capital to compute operating cash flow. We do not subtract this item as it is not available on a quarterly frequency, but we note that it represents only a small fraction of cash flows.

the somewhat more pronounced drop in cash flows after the merger. This could suggest that winners tend to acquire targets with relatively weak cash flow, and that these lower target cash flows drag down the profitability of the combined entity.

Target Q. Finally, in column (8), we estimate the merger effects for above and below-median target Q. Here, the merger effect is -50.14% (p-value: 0.02) for low-Q targets, and -11.89% (p-value: 0.33) for high-Q targets. The difference fails to be significant. Still, both directionally and in terms of economic magnitude, the results suggest that highly valued targets generate less negative post-merger performance.

We conclude that all channels, and in particular strategic flexibility and prior target inefficiencies, remain plausible candidate explanations for our estimation results. A larger sample would be required and exogenous sources of variation would be required to estimate their role more precisely.

We can also relate these channels to our empirical observation that, losers appear to earn positive abnormal returns, rather than winners earning negative abnormal returns, at least if we consider standard measures of abnormal returns to be valid benchmarks around times of mergers. This might be surprising since the *direct* implication of most channels seems to be winner underperformance. That is, whether we consider strategic flexibility, cost of integration, or pre-existing target inefficiencies, a likely immediate implication would be that the winner displays negative abnormal returns while the loser does not earn abnormal returns relative to standard benchmarks. However, it is also possible that, *indirectly*, there are positive long-term return implications for the loser. Consider for example the loss of strategic and financial flexibility. The merger and the ensuing loss in flexibility might be not have an immediate impact on the winner's and loser's operations. But, as new opportunities and competitive challenges arise, the loser will be able to take advantage of them, while the winner will not be able to do so. As a result, the loser's strategic opportunities as well as

their return implications might be better than they would have been without the merger.

[Table 6 approximately here]

5. Comparison with Existing Methodologies

As a last step in our analysis, we use our findings to evaluate existing empirical approaches to estimating the returns to mergers. While our empirical approach to identification is limited to contested mergers and cannot easily be generalized, we can still apply existing approaches to our sample and compare the resulting estimates to our winner-loser estimates. Such an analysis allows us to assess possible biases in the existing approaches, at least to the extent that they materialize in our sample. We consider announcement returns, alpha estimates based on calendar-time portfolio regressions of winner-only portfolios, and winners' buy-and-hold abnormal returns relative to the market portfolio. We present the corresponding estimates in Panel A of Table 7.

The first row of Panel A reports winners' three-day announcement returns. Announcement effects are commonly viewed as the most credible measure of the causal effect of mergers on acquiring-company stock returns, given the difficulty of identifying a valid benchmark for long-run performance. We find that the three-day announcement returns are negative but not statistically significant in closely contested mergers. Thus, compared to our winner-loser estimates, announcement effects underestimate the loss of value generated by mergers and the market is, on average, incorrect in its initial assessment of the causal effect of contested mergers.

[Table 7 approximately here]

The second row of Panel A shows the winners' announcement returns around the date of the losers' withdrawal. We add these statistics since, compared to single-bidder acquisitions, the market may perceive the probability of deal success as lower at the announcement of the ultimate winner's initial bid, and therefore not react strongly at that point in time. When the losing bidder drops out of the contest, the takeover probability for the winner increases and the market may react more strongly. In our sample, we do not find statistically or economically significant market reactions at the time of the losers' withdrawal.

In the third row, we report the losers' announcement returns around the date of their bid withdrawal. If the market views the acquisition as value-destroying, we expect a positive market reaction around the withdrawal date. We indeed find a strong, positive market reaction of +1.8% in losers at the withdrawal of their bid. These results are consistent with the findings based on our winner-loser methodology, and they support the notion that losing bidders' performance is informative about the value created in mergers.

The fourth row shows four-factor abnormal returns of winners, using an equally-weighted calendar-time portfolio methodology for the contest period and post-acquisition returns of the winner. (The monthly *alpha* is scaled by the duration of the contest period and the three-year post-merger period so that the magnitude of the estimate is comparable to our long-run winner-loser estimates.) We estimate a positive and insignificant alpha of 10.3%, instead of a negative and significant effect as in the winner-loser method.

Finally, in row five, we calculate the long-run abnormal buy-and-hold returns of winners relative to the market portfolio. This is similar to our main estimates, which also use long-run BHARs, except that here the market return is used as a benchmark instead of the loser's performance. We find results that are quite different from the winner-loser approach. The winner-only effect is close to zero (+0.1% on the *Full Sample*, and +1.1% on the *Contest Coverage Sample*). To check the robustness of these winner-only estimates, we also compute long-run winner BHARs using alternative risk adjustments. We obtain −13.5 percent and

−3.2 percent for industry-adjusted and risk-adjusted BHARs, respectively. Although they are also negative, these estimates are much smaller in magnitude than the corresponding winner-loser estimates, and none of them are statistically significant.

In summary, none of the traditional methods appear to capture the negative causal effect of mergers in our sample *on average*.

In Table 7, Panel B, we go one step further. Instead of simply comparing the respective overall estimates of the merger effect, we correlate the estimates case by case. Specifically, we regress the long-run winner-loser difference in BHARs on the three announcement returns calculated above and on the winner's long-run market-adjusted BHAR.[33] This additional analysis tests which of the traditional methods generates the same relative assessment of mergers, i.e., the same ordering of mergers with respect to the returns they produce, as our winner-loser methodology, even if the methodologies disagree on the absolute level of returns and possibly even on the sign of merger-induced abnormal returns.

For the winner's announcement return, we find an insignificantly negative correlation. In other words, in deals for which our estimates point to a more negative effect of mergers, the announcement effect tends to be more positive (less negative), while in deals for which our estimates point to a more positive (less negative) effect of mergers, the announcement effect tends to be more negative. Hence, the announcement effect fails to predict the causal effect of mergers even directionally.

The winner's three-day CAR at the loser's withdrawal, reported in the second row of Panel B, shows a positive correlation with our long-run winner-loser estimates, but it is not statistically significant. The loser's CAR around the withdrawal date, shown in the third row, has an even weaker correlation with our estimates.

Turning to the correlation with the winners' long-run BHARs, the picture is more en-

[33] We cannot regress our case-by-case winner-loser estimates on the winner's alpha, since alpha is a single number. It is not estimated case by case.

couraging. The correlation is positive and significant. Partly, this result may reflect the fact that the winners' long-run BHARs are part of the winner-loser difference in long-run BHARs. However, it is not entirely mechanical as the benchmarking against the loser can (and does) turn negative returns positive and vice versa. Quantitatively, the correlation amounts to about 65 percent, and the R-squared is about 40 percent.

Overall, these estimation results indicate that researchers should be cautious when using announcement returns to measure the expected returns to mergers. At least in the subsample of merger contests, the winner's announcement effect appears to be generally uninformative about the returns generated by the merger in the long-run. Existing methodologies focusing on long-run abnormal returns are better in terms of their relative assessment, but fail to capture the extent of value destruction (or creation) in our sample of mergers.

6. Conclusion

This paper makes two contributions. Methodologically, we exploit bidding contests to address the identification issue in estimating the returns to mergers. We argue that, in contests where at least two bidders have a significant ex-ante chance of winning, the post-merger performance of the losing bidder permits the calculation of the counterfactual performance of the winner without the merger. This logic applies to protracted merger fights, in which all participating bidders have, ex ante, a similar chance to win.

Substantively, this paper provides credible estimates of the effect of contested mergers on stock prices. We find that the stock returns of bidders in close contests are not significantly different before the merger contest, corroborating the validity of our methodology. Post-merger, however, bidder returns diverge significantly, providing us with the measure of a causal effect of mergers on acquiring-company stock. In the case of close contests, losers outperform winners by 24 to 37 percent over the three years following the merger.

In interpreting our results, it is important to keep two points in mind. First, while we argue that losers provide a good counterfactual for winners in long contests, we cannot rule out the presence of additional unobserved factors correlated with merger activity that affect abnormal returns. In the paper, we discuss possible omitted variables and show that the empirical evidence is generally inconsistent with the alternative explanations of our main result. Ultimately, though, the credibility of our estimates rests on the identification assumption, which of course cannot be tested directly.

Second, the external validity of our findings is unclear. Our estimates are based on contested mergers, which are not representative of the entire population of mergers. While a non-trivial fraction of mergers are contested (and the empirical assessment of merger contests is interesting in and of itself), the size and even the direction of the effect may not generalize to mergers more broadly. Indeed, our additional results linking acquiror underperformance to large cash transactions suggest that closely contested mergers maybe particularly likely to exhibiting long-term negative effects in the acquiring firm.

At the same time, the empirical estimates do allow us to evaluate existing methodologies at least in this specific context, which suggests caution in interpreting announcement effects as measures of the returns to mergers. Thus, despite the inherent difficulties in assessing long run abnormal returns, our results imply a renewed emphasis on developing long-term approaches along the lines of Lyon, Barber, and Tsai (1999), possibly coupled with matching on pre-merger stock returns.

References

Akbulut, Mehmet E., and John G. Matsusaka, 2010, 50+ years of diversification announcements, *The Financial Review* 45, 231–262.

Alexandridis, George, Kathleen P. Fuller, Lars Terhaar, and Nickolaos G. Travlos, 2013, Deal size, acquisition premia and shareholder gains, *Journal of Corporate Finance* 20, 1–13.

Allen, Jason, Robert Clark, and Jean-Francois Houde, 2014, The effect of mergers in search markets: Evidence from the canadian mortgage industry, *American Economic Review* 104, 3365–3396.

Andrade, Gregor, Mark Mitchell, and Erik Stafford, 2001, New evidence and perspectives on mergers, *Journal of Economic Perspectives* 15, 103–120.

Asquith, Paul, Robert F. Bruner, and David W. Mullins Jr, 1987, Merger returns and the form of financing, *Journal of Financial Economics* 11, 121–139.

Bancel, Franck, and Usha R. Mittoo, 2004, Cross-country determinants of capital structure: a survey of european firms, *Financial Management* 33, 103–132.

Barraclough, Kathryn, David T. Robinson, Tom Smith, and Robert E. Whaley, 2012, Using option prices to infer overpayments and synergies in m&a transactions, *The Review of Financial Studies* 26, 695–722.

Benoît, Jean-Pierre, 1984, Financially constrained entry in a game with incomplete information, *Rand Journal of Economics* 15, 490–499.

Betton, Sandra, B. Espen Eckbo, and Karin Thorburn, 2008, Corporate takeovers, in *Handbook of Corporate Finance: Empirical Corporate Finance, volume 2, chapter 15* (North Holland/Elsevier).

Bhagat, S., M. Dong, D. Hirshleifer, and R. Noah, 2005, Do tender offers create value? new methods and evidence, *Journal of Financial Economics* 76, 3–60.

Bolton, Patrick, and David S. Scharfstein, 1990, A theory of predation based on agency problems in financial contracting, *American Economic Review* 80, 93–106.

Boone, Audra L., and J. Harold Mulherin, 2007, How are firms sold?, *The Journal of Finance* 62, 847–875.

——— , 2008, Do auctions induce a winner's curse? New evidence from the corporate takeover market, *Journal of Financial Economics* 89, 1–19.

Brounen, Dirk, Abe De Jong, and Kees Koedijk, 2006, Capital structure policies in europe: Survey evidence, *Journal of Banking and Finance* 30, 1409–1442.

Campello, Murillo, 2006, Debt financing: Does it boost or hurt firm performance in product markets?, *Journal of Financial Economics* 82, 135–172.

Cartwright, Susan, and Cary L. Cooper, 1993, The role of culture compatibility in successful organizational marriage, *The Academy of Management Executive* 7, 57–70.

Chatterjee, Sayan, 1986, Types of synergy and economic value: The impact of acquisitions on merging and rival firms, *Strategic Management Journal* 7, 119–139.

Daniel, Kent, Mark Grinblatt, Sheridan Titman, and Russ Wermers, 1997, Measuring mutual fund performance with characteristic-based benchmarks, *The Journal of Finance* 52, 1035–1058.

Davidson, Wallace N., Dipa Dutia, and Louis Cheng, 1989, A re-examination of the market reaction to failed mergers, *The Journal of Finance* 44, 1077–1083.

Dean, Carol, 2014, Companies shouldn't scrimp on M&A integration budgets, *Euromoney*.

DeAngelo, Harry, Linda DeAngelo, and Toni M. Whited, 2011, Capital structure dynamics and transitory debt, *Journal of Financial Economics* 99, 235–261.

Deneckere, Raymond, and Carl Davidson, 1984, Horizontal mergers and collusive behavior, *The International Journal of Industrial Organization* 2, 117–132.

Dimopoulos, Theodosios, and Stefano Sacchetto, 2014, Preemptive bidding, target resistance, and takeover premiums, *Journal of Financial Economics* 114, 444–470.

Dodd, Peter, 1980, Merger proposals, managerial discretion and stockholder wealth, *Journal of Financial Economics* 5, 351–373.

——— , and Richard Ruback, 1977, Tender offers and stockholder returns: an empirical analysis, *Journal of Financial Economics* 8, 105–138.

Dong, M., D. Hirshleifer, S. Richardson, and S.H. Teoh, 2006, Does investor misevaluation drive the takeover market?, *The Journal of Finance* 61, 725–762.

Fabozzi, F.J., M.G. Ferri, T.D. Fabozzi, and J. Tucker, 1988, A note on unsuccessful tender offers and stockholder returns, *The Journal of Finance* 43, 1275–1283.

Finkelstein, Sydney, 2016, How to avoid four common problems companies make with mergers and acquisitions – and how to get it right., *Wall Street Journal*.

Fishman, Michael J., 1988, A theory of preemptive takeover bidding, *The RAND Journal of Economics* 19, 88–101.

Fuller, Kathleen, Jeffry Netter, and Mike Stegemoller, 2002, What do returns to acquiring firms tell us? Evidence from firms that make many acquisitions, *The Journal of Finance* 57, 1763–1793.

Gowrisankaran, Gautam, Aviv Nevo, and Robert Town, 2015, Mergers when prices are negotiated: Evidence from the hospital industry, *American Economic Review* 105, 172–203.

Graham, John R., and Campbell R. Harvey, 2001, The theory and practice of corporate finance: Evidence from the field, *Journal of Financial Economics* 60, 187–243.

Greenstone, Michael, Richard Hornbeck, and Enrico Moretti, 2010, Identifying agglomeration spillovers: Evidence from winners and losers of large plant openings, *Journal of Political Economy* 118, 536–598.

Greenstone, Michael, and Enrico Moretti, 2004, Bidding for industrial plants: Does winning a 'million dollar plant' increase welfare?, *Working Paper*.

Greenwood, Royston, CR Hinings, and John Brown, 1994, Merging professional service firms, *Organization Science* 5, 239–257.

Healy, Paul M., Krishna G. Palepu, and Richard S. Ruback, 1992, Does corporate performance improve after mergers?, *Journal of Financial Economics* 31, 135–175.

Hietala, Pekka, Steven N. Kaplan, and David T. Robinson, 2003, What is the price of hubris? Using takeover battles to infer overpayments and synergies, *Financial Management* pp. 5–31.

Ince, Ozgur S., and R. Burt Porter, 2006, Individual equity return data from Thomson Datastream: Handle with care!, *Journal of Financial Research* 29, 463–479.

Jensen, Michael C., 1986, Agency cost of free cash flow, corporate finance, and takeovers, *American Economic Review Papers & Proceedings* 76, 323–329.

——— , and Richard S. Ruback, 1983, The market for corporate control, *Journal of Financial Economics* 11, 5–50.

Lang, Larry, Eli Ofek, and Rene M. Stulz, 1996, Leverage, investment, and firm growth, *Journal of Financial Economics* 40, 3–29.

Larsson, Rikard, and Sydney Finkelstein, 1999, Integrating strategic, organizational, and human resource perspectives on mergers and acquisitions: A case survey of synergy realization, *Organization Science* 10, 1–26.

Loughran, Tim, and Anand M. Vijh, 1997, Do long-term shareholders benefit from corporate acquisitions?, *Journal of Finance* 52, 1765–1790.

Lyon, John D., Brad M. Barber, and Chih-Ling Tsai, 1999, Improved methods for tests of long-run abnormal stock returns, *The Journal of Finance* 54, 165–201.

Makadok, Richard, and Jay B Barney, 2001, Strategic factor market intelligence: An application of information economics to strategy formulation and competitor intelligence, *Management Science* 47, 1621–1638.

Malmendier, Ulrike, and Geoffrey Tate, 2005, CEO overconfidence and corporate investment, *The Journal of Finance* 60, 2661–2700.

——— , 2008, Who makes acquisitions? CEO overconfidence and the market's reaction, *Journal of Financial Economics* 89, 20–43.

——— , and Jun Yan, 2011, Overconfidence and early-life experiences: The effect of managerial traits on corporate financial policies, *Journal of Finance* 66, 1687–1733.

Mandelker, Gershon, 1974, Risk and return: the case of merging frms, *Journal of Financial Economics* 1, 303–335.

Marchica, Maria-Teresa, and Roberto Mura, 2010, Financial flexibility, investment ability, and firm value: evidence from firms with spare debt capacity, *Financial Management* 39, 1339–1365.

Mitchell, Mark, Todd Pulvino, and Erik Stafford, 2004, Price pressure around mergers, *Journal of Finance* 59, 31–63.

Moeller, Sara B., Frederik P. Schlingemann, and René M. Stulz, 2004, Firm size and the gains from acquisitions, *Journal of Financial Economics* 73, 201–228.

——— , 2005, Wealth destruction on a massive scale? A study of acquiring-firm returns in the recent merger wave, *Journal of Finance* 60, 757–782.

Morck, Randall, Andrei Shleifer, and Robert Vishny, 1990, Do managerial objectives drive bad acquisitions?, *The Journal of Finance* 45, 31–48.

Officer, Micah S., 2003, Termination fees in mergers and acquisitions, *Journal of Financial Economics* 69, 431–467.

Penman, Stephen H., Scott Richardson, and Irem Tuna, 2007, The book-to-price effect in stock returns: Accounting for leverage, *Journal of Accounting Research* 45, 427–467.

Perry, Martin K., and Robert H. Porter, 1985, Oligopoly and the incentive for horizontal merger, *The American Economic Review* 75, 219–227.

Rau, Raghavendra, and Theo Vermaelen, 1998, Glamour, value, and the post-acquisition performance of acquiring firms, *Journal of Financial Economics* 49, 223–253.

Rhodes-Kropf, Matthew, David Robinson, and S. Viswanathan, 2005, Valuation waves and merger activity: the empirical evidence, *Journal of Financial Economics* 77, 561–603.

Rhodes-Kropf, Matthew, and S. Viswanathan, 2004, Market valuation and merger waves, *The Journal of Finance* 59, 2685–2718.

Richardson, Scott, Richard Sloan, and Haifeng You, 2011, What makes stock prices move? Fundamentals vs. investor recognition, *Working Paper*.

Roll, Richard, 1986, The hubris hypothesis of corporate takeovers, *Journal of Business* 59, 197–216.

Safieddine, A., and S. Titman, 1999, Leverage and corporate performance: evidence from unsuccessful takeovers, *The Journal of Finance* 54, 547–580.

Salant, Stephen W., Sheldon Switzer, and Robert J. Reynolds, 1983, Losses from horizontal merger: The effects of an exogenous change in industry structure on cournot-nash equilibrium, *Quarterly Journal of Economics* 98, 185–99.

Savor, Pavel G., and Qi Lu, 2009, Do stock mergers create value for aquirers?, *The Journal of Finance* 64, 1061–1097.

Shleifer, Andrei, and Robert W. Vishny, 2003, Stock market driven acquisitions, *Journal of Financial Economics* 70, 295–311.

Singh, Harbir, and Cynthia A Montgomery, 1987, Corporate acquisition strategies and economic performance, *Strategic Management Journal* 8, 377–386.

Spalt, Oliver, and Christoph Schneider, 2016, Why does size matter so much for bidder announcement returns?, *Working Paper*.

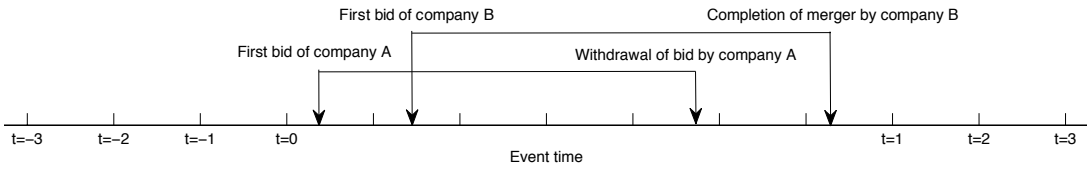
Stigler, George J., 1950, Monopoly and oligopoly by merger, *American Economic Review Papers & Proceedings* 40, 23–34.

Sushil, 2015, Strategic flexibility: The evolving paradigm of strategic management, *Global Journal of Flexible Systems Management* 16, 113–114.

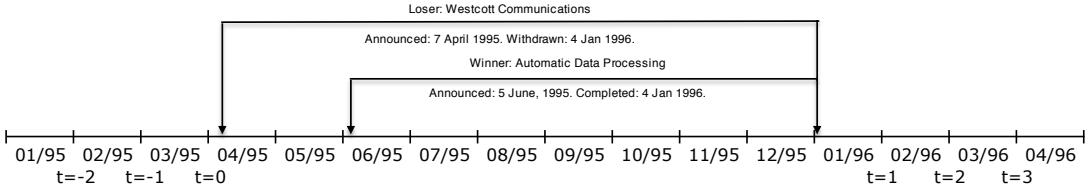
Weber, Roberto A., and Colin F. Camerer, 2003, Cultural conflict and merger failure: An experimental approach, *Management Science* 49, 400–415.

Wermers, Russ, 2004, Is money really 'smart'? New evidence on the relation between mutual fund flows, manager behavior, and performance persistence, *Working Paper, University of Maryland*.

Figures and Tables

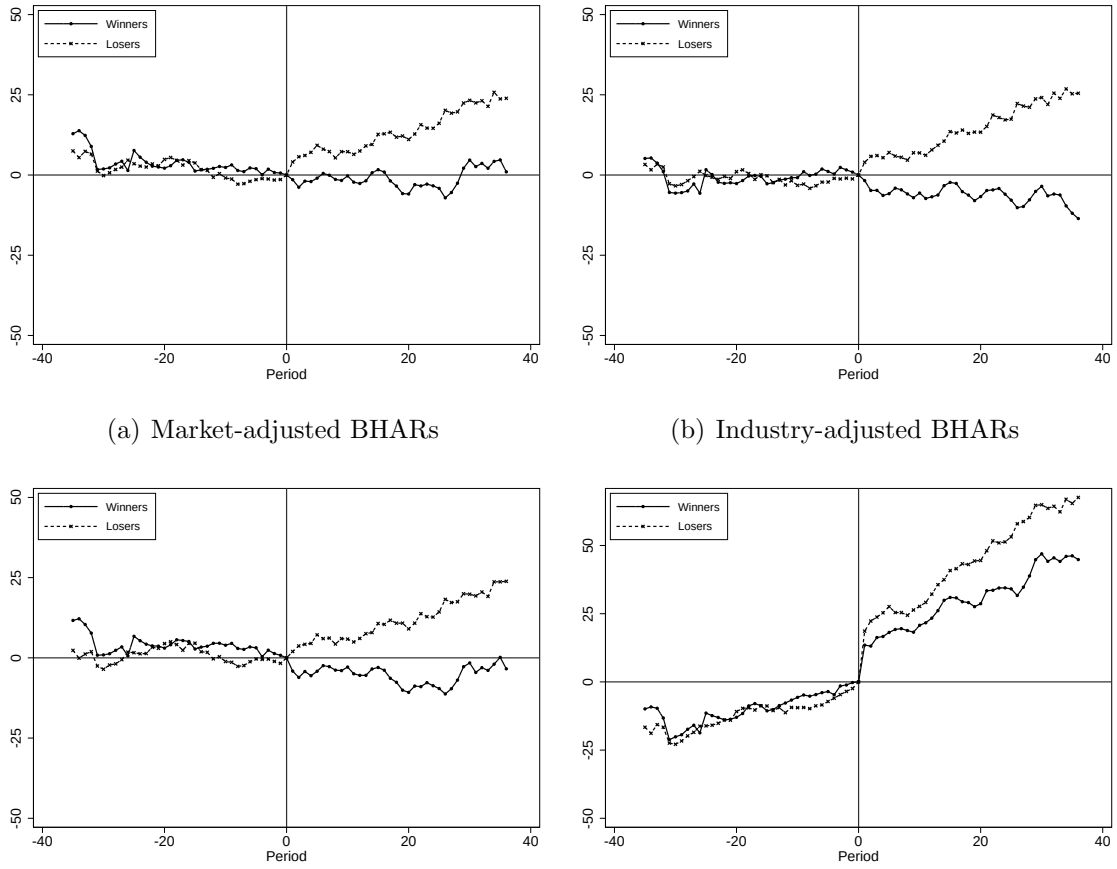


(a) Stylized Example



(b) Data Example

Figure 1
Construction of Event Time



(a) Market-adjusted BHARs

(b) Industry-adjusted BHARs

(c) Risk-adjusted BHARs

(d) Unadjusted buy-and-hold returns

Figure 2
Winner and Loser Performance

The graphs show the average stock price performance of winners and losers in close contests, i.e., in the long-duration subsample. The top graphs show market-adjusted and industry-adjusted buy-and-hold abnormal returns (BHARs). The bottom graphs show risk-adjusted BHARs and buy-and-hold raw returns. BHARs are calculated as described in Table 3. Cumulative raw returns are calculated using the same formula, but setting the benchmark return to zero. The circles correspond to the average winner returns, the crosses to the average loser returns.

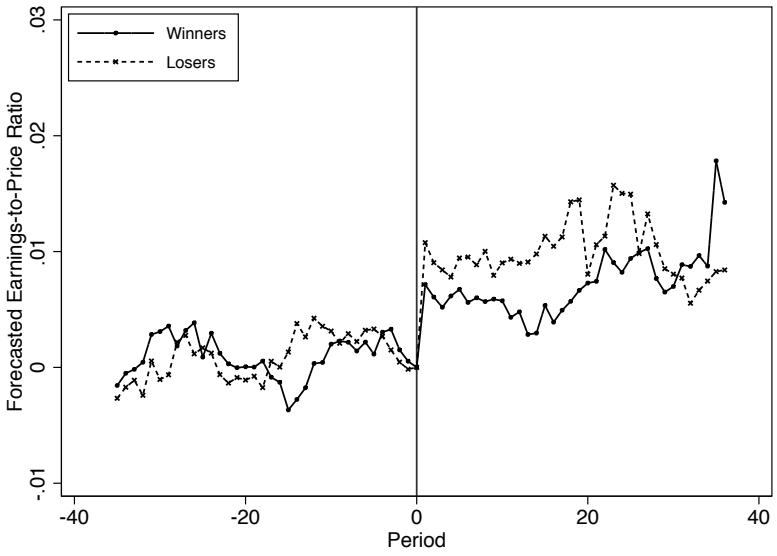


Figure 3
Forecasted Earnings-to-Price Ratio

The figure shows the forecasted earnings-to-price ratio (FE/P ratio) of winners and losers around merger contests in the long-duration sample. The FE/P ratio is computed as analysts' two-year consensus (mean) forecast divided by the stock price at the end of the forecast month. It is computed using quarterly data from I/B/E/S. The FE/P ratio is normalized to zero in $t = 0$ and expressed as a decimal. The circles correspond to the event-time specific mean FE/P ratios of winners, the crosses to those of losers.

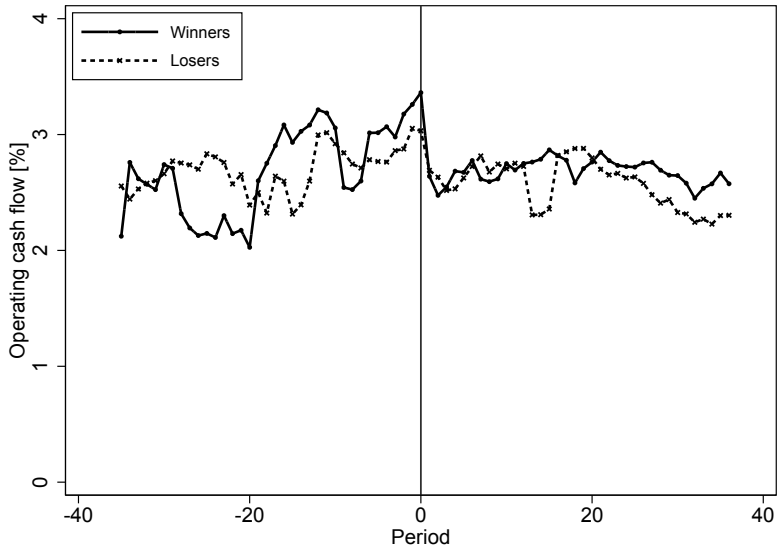


Figure 4
Operating Performance

The figure shows operating cash flow of winners and losers around merger contests in the subsample of long contests. Operating cash flow is calculated as the ratio of net sales minus cost of goods sold and selling, general and administrative expenses over total assets. It is based on quarterly accounting data and is expressed as a percentage. The circles correspond to the event-time specific mean oeprating cash flow of winners, the crosses to those of losers.

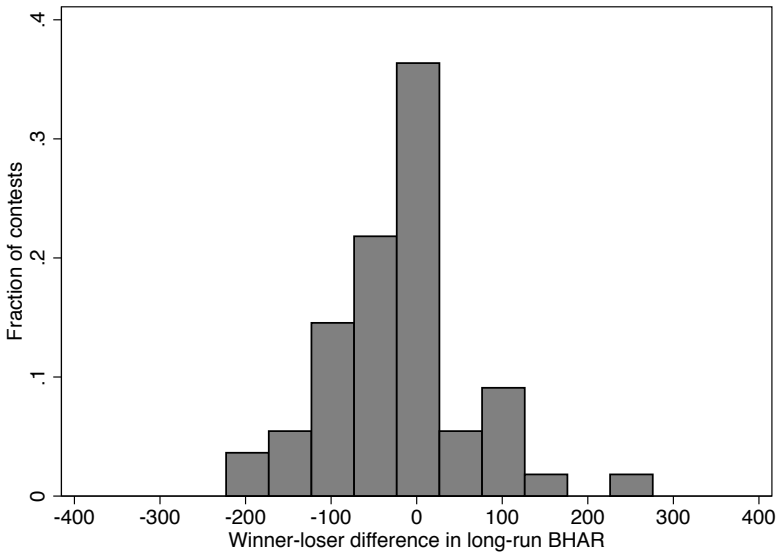


Figure 5
Distribution of Merger Effect Estimates

The histogram shows the distribution of the estimated merger effect for all 56 contests of above-median duration in our Full Sample. The merger effect is the winner-loser difference in the market-adjusted BHAR three years after the merger.

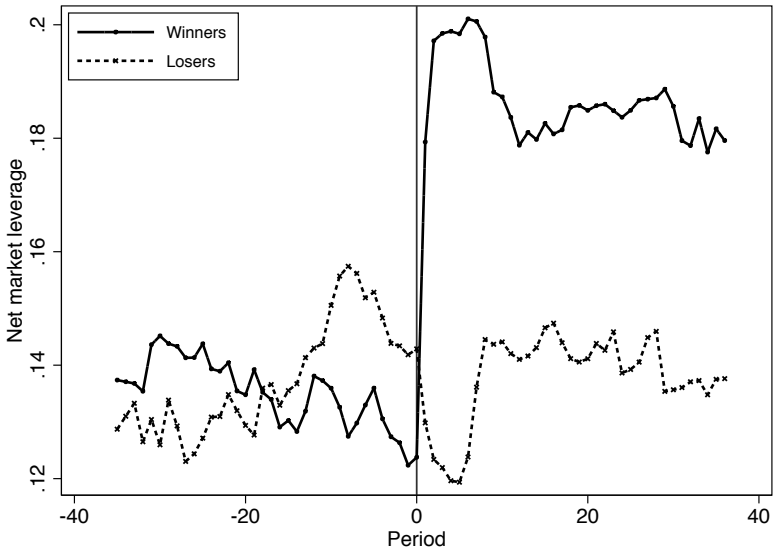


Figure 6
Net Market Leverage

The figure shows net market leverage of winners and losers around merger contests in long contests. Net market leverage is defined as debt in current liabilities plus long term debt minus cash and short-term investments, divided by total assets minus book equity plus market equity. The variable is computed using quarterly data. The circles correspond to the event-time specific mean leverage of winners, the crosses to those of losers.

Table 1
Descriptive Statistics

Panel A: Bidder Characteristics

	Winners				Losers				p-value of
	Mean	Median	Std	N	Mean	Median	Std	N	mean diff.
Total assets [$m]	12649.91	3107.34	26912.24	111	7202.58	2495.59	10750.64	120	0.04
Market value [$m]	19320.16	5351.05	39464.55	111	11628.67	3258.72	21887.65	120	0.07
Sales [$m]	6936.18	1709.6	16697.29	111	4063.24	1112.70	8460.92	119	0.10
Tobin's Q	1.88	1.41	1.37	111	1.82	1.36	1.29	120	0.74
PP&E	0.29	0.22	0.25	108	0.30	0.22	0.26	116	0.86
Profitability	0.12	0.11	0.08	108	0.12	0.12	0.08	116	0.63
Book leverage	0.26	0.19	0.21	111	0.23	0.19	0.17	116	0.20
Market leverage	0.18	0.15	0.16	111	0.17	0.14	0.14	116	0.52
Ann. CAR [%]	-1.51	-0.61	7.41	111	-0.44	-0.93	6.66	119	0.25
Ann. CAR [$m]	-336.28	-6.76	1967.72	111	12.48	-4.97	881.73	118	0.08

Panel B: Deal Characteristics

	P25	Median	P75	Mean	Std	N
Deal value [$m]	90.41	501.95	1,186	3016.52	10805.97	109
Tender offer	0.00	0.00	1.00	0.37	0.48	111
Hostile	0.00	0.00	0.00	0.09	0.29	111
Percentage paid in stock	0.00	0.00	68.74	31.19	40.74	111
Percentage paid in cash	0.00	35.00	100.00	46.71	44.96	111
Number of bidders	2.00	2.00	3.00	2.41	0.91	111
Offer premium [% of target]	30.71	51.56	79.28	63.24	63.24	79
Offer premium [% of acquiror]	1.59	6.43	18.66	10.08	20.55	79
Contest duration [months]	4.00	7.00	10.00	8.65	6.66	111

In Panel A, Total assets are the book value of total assets. Market value is Total assets plus market value of equity (common shares outstanding times fiscal-year closing price) minus book value of equity (book value of shareholders' equity, plus balance sheet deferred taxes and investment tax credit [if available], minus book value of preferred stock, where, depending on availability, we use redemption, liquidation, or par value (in that order) to estimate the book value of preferred stock). Tobin's Q is the ratio of Market value to book value of assets. PP&E is book value of property, plant and equipment divided by total assets. Profitability is operating income before depreciation divided by total assets. Leverage is debt in current liabilities plus long term debt, either divided by Total assets (Book leverage) or by Market value (Market leverage). All of the above variables are based on balance sheet and income data from the end of the fiscal year preceding the contest. Announcement CAR [%] is the three-day cumulative market-adjusted return around the announcement date of the winner's or loser's first bid in a given contest. Announcement CARs [$m] are three-day cumulative dollar abnormal returns of the first bid, i.e., percentage announcement CARs multiplied by the bidder's pre-merger market value of equity. p-value of mean difference refers to the difference in means between winners and losers. In Panel B, Deal value is the dollar value of the winning bid. Tender offer is a dummy indicating a tender offer. Hostile is a dummy indicating whether the deal attitude of the winning bid was hostile. Percentage paid in stock is the percentage of the winning bid that is paid in stock. Percentage paid in cash is the percentage paid in cash. Number of bidders is the total number of bidders involved in the merger contest. Offer premium [% of target] is the run-up in the target's stock price from 40 days prior to the announcement of the initial bid until completion of the merger contest. Offer premium [% of acquiror] is Offer premium [% of target] times target equity value divided by acquiror equity value. Contest duration is the number of months from the month-end preceding the first bid until the end of the month of the completion of the merger.

Table 2
Winner-Loser Similarities: Correlation in Pre-Merger Abnormal Returns

Panel A: Full sample			
	Pre-merger		Pre-merger
	Long	Short	Full Sample
Coefficient	0.586***	0.274**	0.370***
SE	(0.186)	(0.105)	(0.096)
R-squared	0.16	0.11	0.12
Observations	56	55	111
Panel B: Contest coverage			
	Pre-merger		Pre-merger
	Long	Short	Full Sample
Coefficient	0.619**	0.235*	0.343***
SE	(0.237)	(0.122)	(0.116)
R-squared	0.14	0.08	0.09
Observations	44	43	87
Panel C: International sample			
	Pre-merger		Pre-merger
	Long	Short	Full Sample
Coefficient	0.307**	0.290***	0.297***
SE	(0.136)	(0.082)	(0.075)
R-squared	0.05	0.12	0.08
Observations	92	91	183

Winner-loser similarities in abnormal returns are estimated in two steps: In the first step (unreported), we estimate market-adjusted abnormal performance trends for each bidder by regressing each bidder's monthly market-adjusted returns in the three-year pre-contest period on a constant. In the second step, we regress the abnormal performance trends of the winners on those of the losers in the same merger contest. The table reports the resulting coefficients of this univariate regression, separately for the different samples (Panel A to C). We show the pre-merger period results split up into above-median and below-median subsamples of contest duration (column 1 and 2) as well as the full sample (column 3) for the pre-merger period. The intercept is omitted.

Table 3
Winner-Loser Differences in Long-Run Abnormal Returns

Sample:	Full Sample (1)	Contest Coverage (2)	Intl. Sample (3)
Winner (α_1)	1.049	0.101	-0.012
	(0.266)	(0.020)	(2.514)
t (α_2)	-0.182	-0.131	0.111
	(-0.840)	(-0.485)	(0.137)
Winner × t (α_3)	-0.018	-0.251	0.011
	(-0.075)	(-0.900)	(0.177)
Post (α_4)	6.756	3.661	5.384
	(0.834)	(0.413)	(5.820)
Winner × Post (α_5)	-8.927	-5.791	-4.472
	(-1.153)	(-0.622)	(5.605)
t × Post (α_6)	0.693*	0.854*	-0.043
	(1.896)	(1.923)	(0.232)
Winner × Post × t (α_7)	-0.433	-0.503	-0.273
	(-1.027)	(-0.979)	(0.241)
Contest fixed effects	Yes	Yes	Yes
Merger effect	-23.69**	-32.07**	-13.64**
Merger effect: p-value	0.03	0.02	0.05
Observations	8,568	6,624	14,040
R-squared	0.279	0.267	0.28
Number of contests	56	44	92

The regression equation in all columns is: $BHAR_{ijt} = \alpha_0 + \alpha_1\, W_{ijt} + \alpha_2\, t_{ijt} + \alpha_3\, t_{ijt} \cdot W_{ijt} + \alpha_4\, Post_{ijt} + \alpha_5\, Post_{ijt} \cdot W_{ijt} + \alpha_6\, t_{ijt} \cdot Post_{ijt} + \alpha_7\, t_{ijt} \cdot Post_{ijt} \cdot W_{ijt} + \eta_j + \varepsilon_{ijt}$ (equation (3)). The regressions are run on the subsample of long (above-median) contest duration of the sample indicated in the first row. The dependent variable is the market-adjusted buy-and-hold abnormal return, normalized to zero in the month preceding the start of the contest and computed as $BHAR_{ijt} = \prod_{s=1}^{t}(1 + r_{ijs}) - \prod_{s=1}^{t}(1 + r_s^{mkt})$ going forward in event time, and as $BHAR_{ijt} = \prod_{s=0}^{t+1}(1 + r_{ijs})^{-1} - \prod_{s=0}^{t+1}(1 + r_s^{mkt})^{-1}$ going backward, where i denotes the bidder, j the contest and t the event month. The market return is the CRSP value-weighted market return. Winner (W_{ijt}) is a dummy indicating whether bidder i is a winner in merger contest j. t is a variable counting event time. The dummy variable $Post_{ijt}$ indicates whether period t is in the post-merger window of contest j. The lower part of the table between the two solid lines reports tests for the long-run winner-loser differences in BHARs at $t = +36$. Standard errors (in parentheses) are clustered by contest.

Table 4
Winner-Loser Differences in Long-Run Abnormal Returns - Alternative Return Adjustments

Dept. variable:	BHAR (ind.-adjusted)		BHAR (risk-adjusted)		BHR (unadjusted)	
Sample:	Full Sample	Contest Coverage	Full Sample	Contest Coverage	Full Sample	Contest Coverage
	(1)	(2)	(3)	(4)	(5)	(6)
Winner (α_1)	1.480	0.552	1.351	0.775	0.811	-0.267
	(4.199)	(5.239)	(4.008)	(4.957)	(3.845)	(4.772)
t (α_2)	-0.059	-0.099	-0.020	-0.002	0.517**	0.556*
	(0.251)	(0.303)	(0.190)	(0.215)	(0.247)	(0.313)
Winner × t (α_3)	0.125	-0.072	-0.101	-0.304	-0.045	-0.270
	(0.335)	(0.411)	(0.241)	(0.272)	(0.241)	(0.277)
Post (α_4)	6.696	2.860	3.302	-0.853	22.612***	21.492**
	(7.612)	(8.110)	(8.755)	(9.007)	(8.153)	(8.829)
Winner × Post (α_5)	-9.535	-6.060	-9.625	-7.003	-7.957	-5.502
	(7.630)	(9.154)	(7.896)	(9.432)	(7.595)	(9.236)
t × Post (α_6)	0.641*	0.905**	0.524	0.744*	0.844*	0.987*
	(0.367)	(0.447)	(0.340)	(0.382)	(0.466)	(0.578)
Winner × Post × t (α_7)	-0.864*	-1.090*	-0.408	-0.525	-0.421	-0.452
	(0.481)	(0.589)	(0.440)	(0.528)	(0.427)	(0.517)
Contest fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Merger effect at $t = 36$	-33.90***	-46.18***	-26.10**	-35.23**	-23.45**	-31.03**
Merger effect: p-value	0.00	0.00	0.05	0.03	0.04	0.02
Observations	8,419	6,509	8,568	6,624	8,568	6,624
R-squared	0.27	0.23	0.26	0.26	0.36	0.36
Number of contests	56	44	56	44	56	44

The regression equation in all columns is: $BHAR_{ijt} = \alpha_0 + \alpha_1\ W_{ijt} + \alpha_2\ t_{ijt} + \alpha_3\ t_{ijt} \cdot W_{ijt} + \alpha_4\ Post_{ijt} + \alpha_5\ Post_{ijt} \cdot W_{ijt} + \alpha_6\ t_{ijt} \cdot Post_{ijt} + \alpha_7\ t_{ijt} \cdot Post_{ijt} \cdot W_{ijt} + \eta_j + \varepsilon_{ijt}$ (equation (3)). The regressions are run on the subsample of long (above-median) contest duration of the sample indicated in the first row. The dependent variable is the buy-and-hold cumulative abnormal return, normalized to zero in the month preceding the start of the contest and computed as $BHAR_{ijt} = \prod_{s=1}^{t}(1 + r_{ijs}) - \prod_{s=1}^{t}(1 + r_{ijs}^{bm})$ going forward in event time, and as $BHAR_{ijt} = \prod_{s=0}^{t+1}(1 + r_{ijs})^{-1} - \prod_{s=0}^{t+1}(1 + r_{ijs}^{bm})^{-1}$ going backward, where i denotes the bidder, j the contest, t the event month, and bm references a benchmark portfolio. In column 1 and 2, the benchmark return is the value-weighted industry return using the Fama-French 12-industry classification. The benchmark return in columns 3 and 4 is the CAPM required return, $r_{ft} + \beta_i(r_{mt} - r_{ft})$. Columns 5 and 6 use the raw, unadjusted buy-and-hold return, i.e. the benchmark return is set to zero. Winner (W_{ijt}) is a dummy indicating whether bidder i is a winner in merger contest j. t is a variable counting event time. The dummy variable $Post_{ijt}$ indicates whether period t is in the post-merger window of contest j. The lower part of of the table between the two solid lines reports tests for the long-run winner-loser differences in BHARs at $t = +36$. Standard errors (in parentheses) are clustered by contest.

Table 5
Winner-Loser Differences in Long-Run Abnormal Returns - Robustness

Interaction variable:	Acq. Q (1)	Publ./priv. (2)	No. bidders (3)	Acq. premium (4)
Winner (α_1)	-3.591	5.622	-0.440	-0.644
	(6.811)	(4.733)	(5.687)	(9.380)
t (α_2)	-0.224	0.252	-0.374	-0.247
	(0.258)	(0.231)	(0.335)	(0.381)
Winner × t (α_3)	-0.472	0.087	0.207	-0.472
	(0.365)	(0.309)	(0.333)	(0.492)
Post merger (α_4)	11.366	2.624	3.155	-3.295
	(9.935)	(12.746)	(10.992)	(15.230)
Winner × Post (α_5)	-9.173	-17.261*	-12.433	1.077
	(8.351)	(9.675)	(10.605)	(16.292)
t × Post (α_6)	0.956**	0.613	0.842	0.115
	(0.382)	(0.682)	(0.535)	(0.580)
Winner × Post × t (α_7)	0.089	0.685	-0.559	-0.251
	(0.464)	(0.794)	(0.557)	(0.734)
Interaction × Winner (α_9)	8.476	-5.829	3.351	0.204
	(7.670)	(6.822)	(6.792)	(9.821)
Interaction × t (α_{10})	0.091	-0.569	0.483	-0.136
	(0.446)	(0.360)	(0.387)	(0.552)
Interaction × Winner × t (α_{11})	0.900*	-0.117	-0.588	0.877
	(0.497)	(0.432)	(0.424)	(0.575)
Interaction × Post (α_{12})	-10.015	5.423	9.075	21.776
	(16.401)	(16.150)	(15.986)	(19.570)
Interaction × Post × Winner (α_{13})	1.287	10.443	13.036	-14.883
	(15.736)	(13.657)	(14.987)	(19.395)
Interaction × Post × t (α_{14})	-0.572	0.105	-0.375	1.158
	(0.764)	(0.807)	(0.682)	(0.849)
Interaction × Post × t × Winner (α_{15})	-1.000	-1.426	0.304	-0.932
	(0.864)	(0.933)	(0.814)	(0.969)
Merger effect for interaction=0 (p-value)	-26.15** (0.01)	15.39 (0.59)	-25.20* (0.06)	-24.86 (0.12)
Merger effect for interaction=1 (p-value)	-19.90 (0.31)	-34.02*** (0.01)	-18.76 (0.29)	-41.46*** (0.01)
Difference in merger effect (p-value)	6.25 (0.77)	-49.41 (0.11)	6.43 (0.77)	-16.60 (0.46)
Observations	8,568	8,568	8,568	6,624
R-squared	0.28	0.29	0.29	0.29
Number of contests	56	56	0.56	44

The regression equation is $BHAR_{ijt} = \alpha_0 + \alpha_1 W_{ijt} + \alpha_2 t_{ijt} + \alpha_3 t_{ijt} W_{ijt} + \alpha_4 Post_{ijt} + \alpha_5 Post_{ijt} W_{ijt} + \alpha_6 t_{ijt} Post_{ijt} + \alpha_7 t_{ijt} Post_{ijt} W_{ijt} + Interactions_{ijt} + \eta_j + \varepsilon_{ijt}$, where i denotes the bidder, j the contest and t the event month. The dependent variable is the market-adjusted buy-and-hold abnormal return, normalized to zero in the month preceding the start of the contest. The market return is the CRSP value-weighted market return. W_{ijt} indicates whether bidder i is a winner in merger contest j; t counts event time; and $Post_{ijt}$ indicates whether period t is in the post-merger window of contest j. For columns (1) and (4), $Interaction$ indicates the above-median subsample in terms of acquiror Q and acquisition premium, for column (2), it indicates a public target, and for (3) contests with more than two bidders. Acquiror Q is the ratio of the acquiror's market value to book value of assets. The takeover premium is computed as the runup in the target's stock price from 40 trading days prior to the beginning of the contest until one day after completion. The lower part of the table between the two solid lines reports tests for the long-run winner-loser differences in BHARs at $t = +36$. Standard errors (in parentheses) are clustered by contest.

Table 6
Winner-Loser Differences in Long-Run Abnormal Returns - Interaction Effects

Dependent variable:	BHAR (market-adjusted)							
Interaction variable:	All-cash	Diversif.	Acq size	Tgt size	Hostile	Tgt α	Tgt profit	Tgt Q
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Winner (α_1)	0.286	1.499	-2.689	0.318	1.210	-7.719	4.831	5.052
	(4.286)	(4.296)	(7.278)	(7.176)	(4.135)	(9.780)	(3.402)	(3.144)
t (α_2)	-0.054	-0.246	-0.443	-0.329	-0.186	-0.506	-0.295	-0.675
	(0.182)	(0.241)	(0.408)	(0.400)	(0.228)	(0.394)	(0.436)	(0.437)
Winner × t (α_3)	-0.060	0.015	-0.109	0.034	-0.010	-0.667	0.114	0.200
	(0.240)	(0.272)	(0.453)	(0.418)	(0.258)	(0.490)	(0.383)	(0.353)
Post merger (α_4)	6.720	10.772	-8.460	6.269	7.515	-5.282	-2.825	14.995
	(8.788)	(8.631)	(11.784)	(13.066)	(8.288)	(15.128)	(11.547)	(12.128)
Winner × Post (α_5)	-9.763	-10.854	1.928	-7.871	-10.117	7.501	3.186	-21.086**
	(8.277)	(8.641)	(11.307)	(10.969)	(8.125)	(14.003)	(9.852)	(9.767)
t × Post (α_6)	0.534	0.670*	1.237*	0.683	0.659*	1.581***	1.439**	1.650**
	(0.345)	(0.380)	(0.629)	(0.559)	(0.379)	(0.525)	(0.564)	(0.677)
Winner × Post × t (α_7)	-0.254	-0.265	-0.628	-0.311	-0.291	-0.851	-1.193	-1.174
	(0.429)	(0.448)	(0.701)	(0.579)	(0.424)	(0.822)	(0.723)	(0.749)
Interaction × Winner (α_9)	10.350	-3.452	7.564	0.702	-3.778	12.416	-10.102	-11.252
	(8.772)	(9.583)	(7.844)	(7.973)	(10.432)	(10.564)	(11.288)	(10.698)
Interaction × t (α_{10})	-1.613	0.575*	0.530	0.289	0.081	0.764*	-0.235	0.664
	(1.619)	(0.318)	(0.423)	(0.442)	(0.306)	(0.439)	(0.601)	(0.596)
Interaction × Winner × t (α_{11})	0.641	-0.292	0.174	-0.100	-0.166	1.032*	-0.306	-0.532
	(1.397)	(0.362)	(0.487)	(0.496)	(0.424)	(0.581)	(0.636)	(0.637)
Interaction × Post (α_{12})	0.456	-36.144*	30.923*	1.238	-15.941	19.717	3.631	-26.707
	(13.720)	(20.883)	(15.733)	(16.532)	(40.454)	(21.587)	(19.266)	(18.856)
Interaction × Post × Winner (α_{13})	9.316	16.647	-22.201	-2.185	23.993*	-21.659	-10.990	33.475
	(20.322)	(12.407)	(15.434)	(15.854)	(12.256)	(20.210)	(21.004)	(20.058)
Interaction × Post × t (α_{14})	2.005	0.212	-1.106	0.024	0.713	-1.897**	-0.922	-1.645*
	(2.125)	(1.339)	(0.726)	(0.746)	(1.284)	(0.715)	(0.876)	(0.872)
Interaction × Post × t × Winner (α_{15})	-2.231	-1.562	0.408	-0.384	-2.732	0.374	0.871	0.990
	(1.843)	(1.224)	(0.846)	(0.851)	(2.154)	(0.993)	(1.065)	(1.061)

(Table continued on next page)

Table 6 - Continued

Dependent variable:	BHAR (market-adjusted)							
Interaction variable:	All-cash	Diversif.	Acq size	Tgt size	Hostile	Tgt α	Tgt profit	Tgt Q
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Merger effect for interaction=0	-20.45*	-18.10	-26.59*	-17.23*	-19.45*	-53.36***	-29.74*	-50.14**
p-value	0.08	0.11	0.09	0.08	0.07	0.01	0.09	0.02
Merger effect for interaction=1	-56.46**	-69.79*	-20.87	-35.63*	-100.70	-13.38	-31.06	-11.89
p-value	0.01	0.06	0.18	0.07	0.13	0.35	0.12	0.33
Difference in merger effect	-36.01	-51.69	5.72	-18.41	-81.21	39.98*	-1.32	38.25
p-value	0.15	0.17	0.79	0.39	0.22	0.09	0.96	0.11
Observations	8,568	8,568	8,568	8,424	8,568	5,904	5,904	6,048
R-squared	0.28	0.28	0.29	0.28	0.28	0.26	0.26	0.26
Number of contests	56	56	56	55	56	39	39	40

The regression equation in all columns is $BHAR_{ijt} = \alpha_0 + \alpha_1 W_{ijt} + \alpha_2 t_{ijt} + \alpha_3 t_{ijt} W_{ijt} + \alpha_4 Post_{ijt} + \alpha_5 Post_{ijt} W_{ijt} + \alpha_6 t_{ijt} Post_{ijt} + \alpha_7 t_{ijt} Post_{ijt} W_{ijt} + Interactions_{ijt} + \eta_j + \varepsilon_{ijt}$. The dependent variable is the market-adjusted buy-and-hold abnormal return, normalized to zero in the month preceding the start of the contest and computed as $BHAR_{ijt} = \prod_{s=1}^{t}(1 + r_{ijs}) - \prod_{s=1}^{t}(1 + r_s^{mkt})$ going forward in event time, and as $BHAR_{ijt} = \prod_{s=0}^{t+1}(1 + r_{ijs})^{-1} - \prod_{s=0}^{t+1}(1 + r_s^{mkt})^{-1}$ going backward, where i denotes the bidder, j the contest and t the event month. The market return is the CRSP value-weighted market return. W_{ijt} is a dummy indicating whether bidder i is a winner in merger contest j; t is a variable counting event time; and $Post_{ijt}$ is a dummy indicating whether period t is in the post-merger window of contest j. *Interaction* is a placeholder indicating the respective subsample analyzed in each of the columns. For columns (1), (2), and (5), *Interaction* indicates the subsamples of all-cash acquisitions, diversifying mergers, and hostile bids, respectively. For columns (3), (4), (6), (7), and (8), *Interaction* indicates the above-median subsample in terms of acquiror size, relative target size, target pre-merger abnormal performance, target pre-merger profitability, and target pre-merger Q. Acquiror size is the book value of debt plus market value of equity. (Relative) target size is the ratio of the transaction value (or the offered purchase price in the case of the losing bidder) and the bidder's market capitalization. A bid is diversifying if the bidder is in a Fama-French 12-industry classification that is different from that of the target. Target α is the target's market model alpha estimated using monthly stock returns over the three years preceding the contest. The definitions of profitability and Tobin's Q for the target are are The lower part of the table between the two solid lines reports tests for the long-run winner-loser differences in BHARs at $t = +36$. Standard errors (in parentheses) are clustered by contest.

Table 7
Alternative Methods to Assess the Returns to Mergers – Comparison

Panel A: Estimates from traditional methods			
	Mean	SE	N
Winners' announcement CAR [%]	-1.097	(0.799)	56
Winners' CAR at loser's withdrawal [%]	-0.095	(0.824)	55
Losers' CAR at loser's withdrawal [%]	1.798**	(0.715)	62
Winners' 4-factor alpha × (36+avg contest duration)	10.335	(10.467)	56
Winners' market-adjusted BHAR [%]	0.097	(13.315)	56
Panel B: Correlation with estimates from traditional methods			
	Coefficient	SE	N
Winners' announcement CAR [%]	-3.496	(2.288)	56
Winners' CAR at loser's withdrawal [%]	3.821	(2.262)	56
Losers' CAR at loser's withdrawal [%]	0.504	(2.243)	62
Winners' market-adjusted BHAR [%]	0.649***	(0.108)	56

Panel A reports the three-day market-adjusted return to the aquiror at announcement of the acquiror's initial bid (first row) and at the announcement of the loser's withdrawal (second row), the return to the loser at withdrawal (third row), the four-factor alpha of equally-weighted calendar-month portfolios of post-acquisition acquiror returns (fourth row), and the long-run buy-and-hold market-adjusted return of acquirors (fifth row). The four-factor alpha is the intercept of a time-series regression of the equally weighted excess return of an acquiror portfolio on the excess market return, the Fama-French factors, and the momentum factor. We use all acquiror stock returns that occur during the contest period or the following 36 months. The four-factor alpha from the monthly return regression is multiplied by 36 plus the average contest duration to make it comparable to the performance measurement period used for the estimates in Table 3. Panel B reports univariate regressions of the long-run winner-loser difference in BHARs on the return at announcement of the acquiror's initial bid (first row) and at the announcement of the loser's withdrawal (second row), the return to the loser at withdrawal (third row), and the long-run buy-and-hold market-adjusted return of acquirors (last row). Standard errors are in parentheses.

Appendix: Sample Construction

In this Appendix, we provide more details on the construction of our data set from Thomson One Mergers and Acquisitions, CRSP, Compustat, Compustat Global, Datastream, and I/B/E/S.

The initial data collection from Thomson One for the two North American samples described in the main paper includes the following variables: deal number of each bid, acquiror's and target's company identifiers (CIDGEN), six-digit CUSIP, nation, SIC code, announcement date of the bid, its effective or withdrawal date, dollar value of the offer, percentage of the transaction value offered in cash, in stock, or in other means of payment, deal attitude (friendly or hostile), number of bidders involved in the bidding process, and competing bid flag. We collect those data items for the period from 1/1/1985 to 12/31/2012.

We collect the following data for the longer period (including +/- three years) of 1/1/1982 to 12/31/2015: From the CRSP Monthly Stock Database, we obtain holding period stock return (RET), distribution event code (DISTCD), delisting code (DLSTCD), date of delisting payment (DLPDT), amount after delisting (DLAMT), delisting return (DLRET), and CRSP value-weighted index returns (VWRETD). From the Fama-French data library, we obtain the monthly Fama-French factor returns (MKTRF, SMB, HML), the risk-free rate, and value-weighted industry returns (12-industry classification). From the CRSP-COMPUSTAT Fundamentals Annual Database we obtain yearly accounting data, including total assets (AT), net sales (SALE), total debt (LT), shareholders' equity (SEQ), deferred taxes and investment tax credit (TXDITC), preferres stock liquidating, redemption and carrying value (PSTKL, PSTKRV, UPSTK, respectively), common shares outstanding (CSHO), fiscal year closing price (PRCC_F), and operating income (OIBDP). From the CRSP-COMPUSTAT Fundamentals Quarterly Database, we obtain quarterly data on total assets (ATQ), debt in current liabilities (DLCQ), long-term debt (DLTTQ), cash and short-term investments

(CHEQ), common shares outstanding (CSHOQ), fiscal quarter closing price (PRCCQ), book value of shareholders' equity (SEQQ), balance sheet deferred taxes and investment tax credit (TXDITCQ), book value of preferred stock (PSTKQ), net sales (SALEQ), cost of goods sold (COGSQ), and selling, general and administrative expenses (XSGAQ). We use the CRSP-COMPUSTAT quarterly data to construct the time series of operating cash flow and various measures of leverage (book and market leverage, as well as net book and net market leverage) for each bidder.

We merge the Thomson One and CRSP data using the 6-digit CUSIPs. Specifically, we match the 6-digit CUSIP provided by Thomson One with the first six digits of CRSP's historical CUSIP (NCUSIP). Since the CUSIP of a firm can change over time, and reassignment of CUSIPs is particularly common following a merger, we match Thomson One's bidder CUSIP with CRSP's NCUSIP for the month preceding the announcement of the bid. We manually check that the company names recorded by Thomson correspond to the matched CRSP company names. If a firm has multiple equity securities outstanding, we use (1) the common stock if common and other types of stock are traded; (2) Class A shares if the company has Class A and Class B outstanding; and (3) the stock with the longest available time series of data if there are multiple types of common stock traded.

We use the stock return data to construct the monthly time series of bidder returns for a window of $+/-$ three years around the merger contest (from event time $t = -35$ to event time $t = +36$). Depending on the duration of the bidding contest, the length of the return time series vary. The CRSP holding period return is adjusted for stock splits, exchanges, and cash distributions. (This adjustment is important since these events are particularly common around mergers.) We then construct the corresponding time series of CRSP value-weighted market returns and of value-weighted industry returns. From these return series we construct buy-and-hold abnormal returns as described in Section 2.

For publicly listed target firms, we also construct time series of daily returns to calculate

the offer premium, computed as the percentage run-up in stock price from 40 trading days prior to the beginning of the contest until the day of delisting. Alternatively, we express the offer premium as a percentage of the acquirors' equity value (percentage run-up in the target stock price $\times$ target market capitalization / acquiror market capitalization).

In the three-year period after a merger, many bidders disappear from CRSP due to delisting. To reduce survivorship bias, we calculate the return implications of the delisting events for shareholders using all delisting information available in CRSP. The delisting code (DLSTCD) classifies delistings into mergers, exchanges for other stock, liquidations, and several other categories of dropped firms; the distribution information (DIVAMT) reports to what extent shareholders were paid in cash or stock; and the delisting return (DLRET) provides the shareholder returns from the last day the stock was traded to the earliest post-delisting date for which CRSP could ascertain the stock's value. We round the delisting return period to full months and track the performance of a delisted firm from the perspective of a buy-and-hold investor, mirroring our approach when tracking the performance of bidders who are listed. Specifically, we assume that stock payments in takeovers are held in the stock of the acquiring firm and exchanges for other stock are held in the new stock. When shareholders receive cash payments (in mergers, liquidations, and bankruptcies) or CRSP cannot identify or does not cover the security in which payments are made, we track performance as if all proceeds were invested in the market portfolio, using the value-weighted CRSP index.

We merge the resulting monthly panel with the annual and the quarterly accounting data from CRSP-COMPUSTAT by assigning, to each monthly observation, the data pertaining to the most recent preceding fiscal-year (fiscal-quarter) end.

Our initial sample contains 623 bids and 293 takeover contests. We drop repeated bids by the same bidder, and keep the date of the first bid as the announcement date. This eliminates 20 bids. Next, we drop 57 contests that had not been completed by March 31, 2012. We further drop 99 bidders that could not be matched to a CRSP PERMNO. We then

delete 14 contests in which the winner is the ultimate parent company of the target. Next, we balance the sample by requiring non-missing stock return data from $t = -35$ to $t = +36$ (i.e., from three years before to three years after the contest), which reduces the number of contests to 178. We also eliminate one bidder which exhibits extreme stock returns and return volatility in the pre-merger period due to idiosyncratic factors.[34] Finally, we keep only those contests for which we have stock price data for both the winner and the loser(s) in a given contest. This reduces the sample by another 66 contests, resulting in a final sample of 112 contests with bids placed by 233 bidders, 112 winners and 121 losers (*Full Sample*).

[Figure A-1 approximately here]

[Table A-1 approximately here]

We add analyst forecast data from I/B/E/S. Specifically, we download the 2-year-ahead consensus forecast (i.e. the median (MEDEST) and mean (MEANEST) forecast of all analysts covering the firm) of earnings per share for the entire -3/+3-year window around the merger from the I/B/E/S summary history file using the 8-digit CUSIP as identifier. As in previous literature, we drop observations with negative forecasted earnings (see, e.g., Richardson, Sloan, and You (2011)). We also download forecasts of various other horizons which yield very similar results.

We extract two-years-ahead earnings forecasts for the 36 months before (and, for completeness, for the 36 months after) the merger from the I/B/E/S summary history file. We construct the forecasted-earnings-to-price ratios (FE/P ratios) as the two-year consensus

[34] This is the contest between Yahoo! Inc and TMP Worldwide Inc for HotJobs.com Ltd initiated by TMP Worldwide Inc on 29 June 2001 and won by Yahoo! Inc on February 13, 2002. Yahoo! Inc shows a 1000% stock price runup and reversal in the 36 months leading up to the merger, TMP Worldwide Inc shows a 100% runup and reversal in the same period. Idiosyncratic factors determining the run-ups make these bidders a poor benchmark for each other. Keeping the observation in the sample does not affect the magnitude of our findings; it only increases the standard errors.

(mean) forecast divided by the stock price at the end of the month.[35] Our sample includes forecasts for 180 firms, 77.2% of our total sample.[36]

For the *International Sample*, we obtain the stock returns of non-North American firms in a three-step procedure. In the first step, we are able to match 59 non-North American firms with Compustat Global via the SEDOL identifier provided by Thomson One. The firms that cannot be matched with Compustat Global are then merged with Datastream, also using the SEDOL. We are able to match 13 firms in this second step. For the remaining firms, we search annual reports and company profiles for their company identifiers. If no SEDOL but the International Securities Identification Number (ISIN) can be found, the ISIN is used as identifier. Finally, we merge these firms with Datastream again, successfully matching another 18 firms. Daily Compustat Global and Datastream stock prices are compounded to monthly returns and adjusted for stock splits and dividends. Eventually, we are able to add 152 bids and 72 contests to our North American sample.[37]

[Table A-2 approximately here]

[35] As in previous literature, we drop observations with negative forecasted earnings (see, e.g., Richardson, Sloan, and You (2011)). Alternatively, we use the median forecast and different forecast horizons with very similar results.

[36] Since firms are covered by analysts at different points in time and for different periods of time, the number of available consensus forecasts varies across periods. The average number of available consensus forecasts is 171, which constitutes 73.4% of our total sample.

[37] We drop the contest between Volkswagen and General Motors for Skoda. According to Thomson One, the contest starts on 12th September 1990 and ended on the 12th December 1995. However, the corporate history states that a decision for Volkswagen was been made as early as December 1990. The discrepancy between Thomson One and the corporate history is possibly due to the structure of the merger. Skoda remained a joint venture between Volkswagen and the Czech Republic, and Volkswagen only increased its ownership share of the joint venture (above 50%) on Dec 11, 1995. Hence, uncertainty about the outcome of the contest was resolved significantly before the official completion date.

Appendix: Figures and Tables

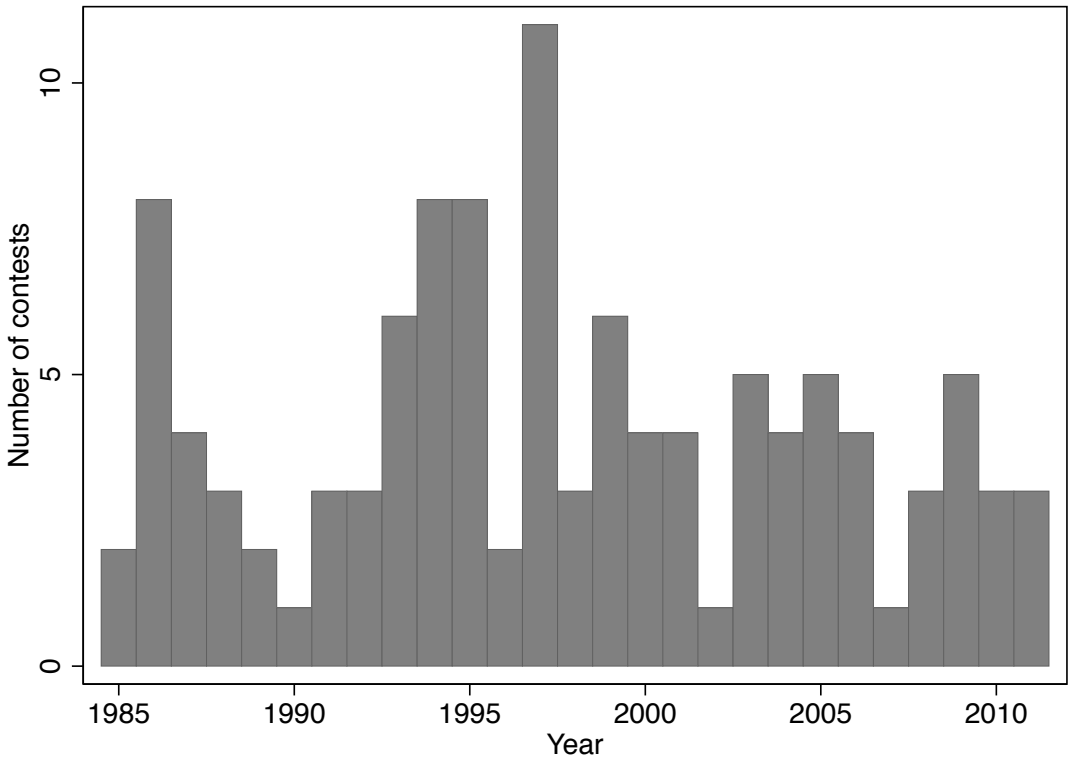
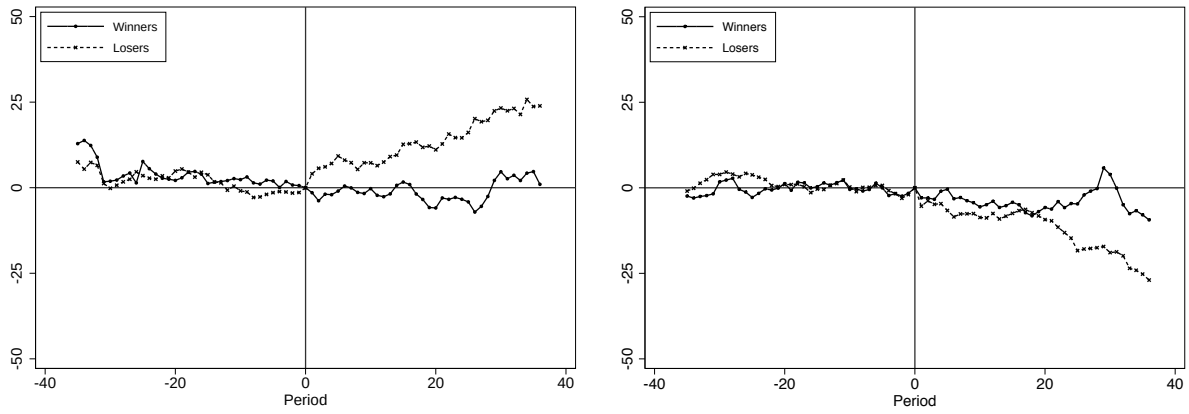


Figure A-1
Merger Contests over Time

This figure shows the frequency distribution of merger contests over the sample period. Years are the calendar years in which the contests started.



(a) Contest duration: Upper half (b) Contest duration: Lower half

Figure A-2

Winner and Loser Abnormal Performance - Long and Short Contests

The two graphs show the stock price performance of winners and losers around merger contests of long (left panel) and short (right panel) duration. The performance measure is the market-adjusted buy-and-hold return (BHAR), calculated as described in Table 3. The circles correspond to the average winner BHARs, the crosses to the average loser BHARs.

Table A-1
Sample Construction

Sample selection criterion	Bids	Bidders	Winners	Losers	Contests
Initial sample	623	604	239	365	293
less repeated bids of same bidder for same target	603	603	238	365	292
less contests that were not completed	488	488	238	250	235
less bidders without CRSP PERMNO	389	389	191	198	208
less contests where winner was parent company	362	362	176	186	194
less bidders with missing return data in the +/- three-year event window	304	304	154	150	178
less bidders with extreme return volatility	303	303	153	150	178
less bidders in contests with missing winner or loser	233	233	112	121	112
less contests with financial bidder	231	231	111	120	111

The initial sample consists of all bids in contested mergers submitted by public U.S-listed companies between January 1, 1985 and March 31, 2012 and recorded in the Thomson One Mergers and Acquisitions database, excluding bids by White Knights. We apply the criteria below to obtain a balanced and matched sample, that is, a sample with complete data on stock returns for both the winner and the loser(s) in the three-year periods before and after the merger contest.

Table A-2
Sample Construction - International Sample

Sample selection criterion	Bids	Bidders	Winners	Losers	Contests
Initial sample	1154	997	421	689	539
less repeated bids of same bidder for same target	1129	994	418	689	536
less contests that were not completed	898	815	418	459	427
less bidders for which no stock data could be found	847	772	392	435	404
less contests where winner was parent company	615	551	283	316	343
less bidders with missing return data in the +/- three-year event window	526	470	244	269	318
less bidders with extreme return volatility	523	468	243	268	317
less bidders in contests with missing winner or loser	385	357	180	201	184
Nation of bidder, final sample					
United States	247	225	112	113	
Canada	40	39	15	24	
United Kingdom	37	34	17	17	
Australia	23	19	8	11	
France	10	10	3	7	
Germany	9	9	4	5	
Japan	5	5	3	2	
Italy	5	4	3	1	
Spain	3	3	1	2	
Netherlands	1	1	1	0	
Sweden	1	1	1	0	
Switzerland	1	1	0	1	

The initial sample consists of all bids in contested mergers submitted by publicly listed companies in the United States, Canada, the United Kingdom, Australia, France, Germany, Japan, Italy, Spain, Netherlands, Sweden, and Switzerland between January 1, 1985 and March 31, 2012 and recorded in the Thomson One Mergers and Acquisitions database, excluding bids by White Knights. We apply the criteria below to obtain a balanced and matched sample, that is, a sample with complete data on stock returns for both the winner and the loser(s) in the three-year periods before and after the merger contest.

Table A-3
Winner-Loser Differences in Long-Run Abnormal Returns, Short Contests

Sample:	Full Sample (1)	Contest Coverage (2)	Intl. Sample (3)
Winner (α_1)	1.110	2.767	2.549
	(2.423)	(2.753)	(1.990)
t (α_2)	-0.117	-0.151	-0.177
	(0.177)	(0.211)	(0.155)
Winner × t (α_3)	0.137	0.155	0.363**
	(0.216)	(0.265)	(0.164)
Post (α_4)	-0.513	1.262	2.750
	(4.798)	(5.922)	(3.950)
Winner × Post (α_5)	-3.095	-7.319	-1.180
	(5.842)	(7.016)	(3.889)
t × Post (α_6)	-0.457	-0.491	0.292
	(0.377)	(0.428)	(0.270)
Winner × Post × t (α_7)	0.415	0.577	-0.571*
	(0.501)	(0.618)	(0.293)
Contest fixed effects	Yes	Yes	Yes
Merger effect at $t = 36$	17.32	21.07	-5.89
Merger effect: p-value	0.27	0.28	0.49
Observations	8,208	6,480	13,680
R-squared	0.28	0.27	0.25
Number of contests	56	44	92

The regression equation in all columns is: $BHAR_{ijt} = \alpha_0 + \alpha_1\, W_{ijt} + \alpha_2\, t_{ijt} + \alpha_3\, t_{ijt} \cdot W_{ijt} + \alpha_4\, Post_{ijt} + \alpha_5\, Post_{ijt} \cdot W_{ijt} + \alpha_6\, t_{ijt} \cdot Post_{ijt} + \alpha_7\, t_{ijt} \cdot Post_{ijt} \cdot W_{ijt} + \eta_j + \varepsilon_{ijt}$ (equation (3)). The regressions are run on the subsample of short (below-median) contest duration of the sample indicated in the first row. The dependent variable is the market-adjusted buy-and-hold abnormal return, normalized to zero in the month preceding the start of the contest and computed as $BHAR_{ijt} = \prod_{s=1}^{t}(1 + r_{ijs}) - \prod_{s=1}^{t}(1 + r_s^{mkt})$ going forward in event time, and as $BHAR_{ijt} = \prod_{s=0}^{t+1}(1 + r_{ijs})^{-1} - \prod_{s=0}^{t+1}(1 + r_s^{mkt})^{-1}$ going backward, where i denotes the bidder, j the contest and t the event month. The market return is the CRSP value-weighted market return. Winner (W_{ijt}) is a dummy indicating whether bidder i is a winner in merger contest j. t is a variable counting event time. The dummy variable $Post_{ijt}$ indicates whether period t is in the post-merger window of contest j. The lower part of the table between the two solid lines reports tests for the long-run winner-loser differences in BHARs at $t = +36$. Standard errors (in parentheses) are clustered by contest.