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# TARGET REVALUATION AFTER FAILED TAKEOVER ATTEMPTS – CASH VERSUS STOCK

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**ABSTRACT**

Cash- and stock-financed takeover bids induce strikingly different target revaluations. We exploit detailed data on unsuccessful takeover bids between 1980 and 2008, and show that targets of cash offers are revalued on average by +15% after deal failure, whereas stock targets return to their pre-announcement levels. The differences in revaluation do not revert over longer horizons. We find no evidence that future takeover activities or operational changes explain these differences. While the targets of failed cash and stock offers are both more likely to be acquired over the following 8 years than matched control firms, there are no differences between cash and stock targets, neither in the timing nor in the value of future offers. Similarly, we cannot detect differential operational policies following the failed bid. Our results are most consistent with cash bids revealing prior undervaluation of the target. We reconcile our findings with the opposite conclusion in earlier literature (Bradley, Desai, and Kim, 1983) by identifying a "look-ahead" bias built into their sample construction.

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## 1. Introduction

Takeovers are among the largest and most disruptive events in a corporation's lifetime. The proper assessment of their value implications has been of foremost interest to policymakers and academic researchers alike. One set of stylized facts the literature has been wrestling to explain is the large difference in returns of cash- and stock-financed takeovers, as well as the different motives of acquirers for choosing one type of payment over the other (see, e.g., Shleifer and Vishny, 2003; Rhodes-Kropf and Viswanathan, 2004; Fishman, 1989). Announcement returns of cash deals are consistently found to be higher than those of stock deals, both in the short run (Huang and Walkling, 1987) and in the long run (Loughran and Vijh, 1997), and not only for the acquirer but also for the target (Andrade, Mitchell, and Stafford, 2001).[1]

The correct interpretation of the return differences between cash and stock bids depends on the underlying information the market responds to (see, for instance, Bradley, Desai, and Kim, 1983). A bid might reveal information about the value implications of the takeover, e.g., match-specific synergies reaped by the acquirer or the size of the premium paid to target shareholders. A bid might also reveal information about value implications that are independent of the specific takeover, e.g., information about the parties' stand-alone values or about the target firm's general attractiveness as a takeover target. As Bhagat, Dong, Hirshleifer, and Noah (2005) argue, disentangling these non-exclusive sources is a first-order building block in estimating the real value created by mergers and acquisitions.

Our paper contributes to this debate by identifying and quantifying the economic relevance of the different channels in the context of failed takeover attempts. The difference in valuations before bid announcement and after bid failure allows us to separate out the value implications of the takeover itself, and to estimate the extent to which firms are revalued in response to the bid, independently of the completion of the takeover. Focusing on the target, we then go one step further, and distinguish between revaluation due to expected future takeover activity (i.e., the target firm's general attractiveness as a takeover target) and revaluation of the target's stand-alone value.

For our empirical analysis, we collect a detailed data set on unsuccessful merger bids and tender offers in the U.S. between 1980 and 2008, including hand-collected information about the failure reasons. We show that, on average, targets of cash offers are revalued by +15%, but there is no revaluation of stock targets.[2] Specifically, after an initial announcement effect (including the 25-trading-day run-up) of +25% for cash targets and +15% for stock targets,[3] the value of the average cash target remains at +15%

[1] See also the overview paper by Betton, Eckbo, and Thorburn (2008).

[2] We also show that, consistent with previous studies, stock *acquirers* trade on average at significantly lower prices post failure (−17.6%), while cash acquirers remain at their pre-announcement level. See Rhodes-Kropf, Robinson, and Viswanathan (2005) and Dong, Hirshleifer, Richardson, and Teoh (2006) for similar findings on acquirers. Savor and Lu (2009) compare unsuccessful and successful bids, and find that unsuccessful stock acquirers perform worse.

[3] The estimates are almost identical to those found by Huang and Walkling (1987) in their sample

cumulative abnormal returns (CAR) at the time of deal failure, relative to the pre-announcement level, while the CAR of stock targets is statistically insignificant (with a slightly negative point estimate). These results hold controlling for a host of deal- and firm-level characteristics, including target size, relative deal size, offer premium, hostility, and deal form (tender offer versus merger). We also show that this difference persists in the long run. Over the next five years after failure of the bid, targets of cash and stock offers do not exhibit abnormal stock-market performance.

Since deal failure is not exogenous, we cannot easily generalize our findings beyond the sample of failed deals. The issue is not that deals that ultimately fail are different from deals that do not fail; *common* deal-failure bias would not affect the differential revaluation of cash and stock targets.[4] Instead, the concern is a more subtle selection bias, namely that selection into bid failure *differs* between cash and stock targets.

We address the concern of differential sample-selection bias following the approach of Savor and Lu (2009). We classify failure reasons for failed deals into categories such as regulatory intervention, negative shocks to the bidder, or uncovering of new information about the target post announcement of the bid. Based on our hand-collected news-search analysis and detailed categorization of failure reasons, we replicate our analysis for each of the 12 identified failure categories. We find that the cash-stock revaluation difference for targets is positive in every category other than failure due to "market-wide shocks," although the statistical significance naturally varies given the small subsamples.

Such categorization involves an inevitable amount of judgment, and none of the failure categories can be definitely established as exogenous to target value as convincingly as in a randomized experiment. However, endogeneity concerns (with respect to target value) should be less relevant for deals that failed due to regulatory intervention or negative shocks to the bidder compared to bids that failed because of negative shocks to the target, such as the uncovering of accounting fraud in the due-diligence process. Based on the extent of such endogeneity concerns, we then aggregate deal-failure categories into larger subsamples. Our results are robust, regardless of variations in how exactly we form these subsamples.

To investigate the source of revaluation, we first test whether future takeover bids explain our findings. Do targets of (failed) cash bids receive significantly more or significantly higher future bids than stock targets? For each target of a failed deal, we measure the time from the date of deal failure until the arrival of a future successful offer (or the censoring event determined by the data sample). Employing hazard-rate models, we find that *both* cash and stock targets are significantly more likely to be targeted in subsequent offers, compared to a sample of matched control firms. For example, after 5 years, 50% of the targets with failed bids have received a successful bid, compared to 20% in the

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of successful and unsuccessful deals, +29.3% for cash and +14.4% for stock deals, suggesting that the market assesses (eventually) failed deals to be quite similar ex ante.

[4] If, hypothetically, revaluations were lower in failed than in completed transactions by a common percentage for both cash and stock deals, the revaluation difference between cash and stock deals in the sample of failed bids would be representative of that in the full sample.

control group. The abnormally high subsequent takeover activity persists until year 8 after deal failure. However, comparing targets of unsuccessful cash and stock bids with each other, we do not detect any differences in frequency. Similarly, we do not detect any differences in future takeover premia in the subsample of targets that receive a successful follow-up bid, though the statistical power of the latter analysis is low.

Another plausible explanation is that bids induce value-increasing operational-policy changes. This *catalyst channel* could explain our results if targets of failed cash bids responded more than targets of failed stock bids. However, employing operational outcome variables used by Safieddine and Titman (1999), we do not find any robust evidence for differential post-failure policies.

In a nutshell, our paper, first, documents significant differential revaluation of cash and stock targets in failed takeover bids. By construction, such revaluation is independent of the value created by the intended takeover. Second, the differential revaluation is neither explained by future takeovers nor by common proxies of operational change. Our evidence is most consistent with a differential reassessment of cash and stock targets, i.e., a pure informational channel. At the same time, our results do not indicate the absence of synergies or operational improvements as a result of takeover bids. Instead, our empirical approach of comparing cash and stock targets of failed bids is specifically designed to isolate information effects: we exploit that cash and stock targets are similar in that they are both exposed to a failed takeover, allowing us to difference out any associated real effects while identifying the differential information content embedded in the medium of exchange. Thus, our empirical results should be interpreted as ruling in the possibility of significant information effects, which contrasts with the previous literature.

**Contribution to literature.** Our paper relates to an earlier literature on mergers and acquisitions exploiting bid failure. Dodd and Ruback (1977) document a large revaluation of targets after failed *tender offers*. Dodd (1980) compares revaluations of target firms that vetoed the bid to those that did not. He finds that only target firms whose management opposed the bid are positively revalued. Bradley, Desai, and Kim (1983) analyze the role of future bids. They compare target firms that are taken over within five years following the initial (failed) tender offer to firms that are not. They report that firms without a subsequent takeover offer exhibit negative abnormal returns after deal failure, and return to pre-offer valuations: the CAR point estimate from 1 month prior to announcement of the original bid until 5 years after the announcement is virtually zero, namely $-1.07\%$. They conclude that "the gains to the stockholders of unsuccessful targets stem from the anticipation of a future successful acquisition and not simply from the revelation of new information regarding the 'true' value of the target resources."

The conclusion of this seminal paper has remained the presumed status quo in the literature (see Davidson, Dutia, and Cheng, 1989, and Fabozzi, Ferri, Fabozzi, and Tucker, 1988, for follow-up studies on merger bids and tender offers, respectively). Our findings suggest that the evidence in these studies needs to be reinterpreted. We argue that forward-looking sample construction biases the returns of firms without future takeover activity downwards. The magnitude of this bias is economically significant. We show

that matched control firms are taken over about 20% of the time over the span of 5 years. This amount of future takeover activity should be priced in the stock-market valuation, also in the case of *actual* targets prior to the announcement of the (initial) takeover offer. Conditioning on the absence of takeover activity for 5 years after deal failure therefore induces a negative look-ahead bias of about 20% of the typical takeover premium. Using the average historical premium of 46.2% (cf. Panel A of Table 1), a back-of-the-envelope calculation (ignoring discounting) suggests that the magnitude of this bias is roughly $46.2\% \cdot 0.2 \approx 9\%$. Since Bradley, Desai, and Kim (1983) find that targets in their sample (almost exactly) return to pre-announcement levels despite this bias, their empirical results are consistent with a positive (offsetting) informational effect of the failed bid. Therefore, their conclusion of ruling out information effects may not be warranted based on their own evidence.

The issue of forward-looking sample selection implies that the *point estimate* of the long-run CAR is a (downward) biased measure of the informational effect of a bid, since the estimation conditions on the absence of future bids. A separate, more general concern is the *precision* of long-run CAR estimates, which affects all studies calculating long-run returns: since the standard error of the CAR grows with the square root of the return horizon (e.g., Fama, 1998), a one-time event triggering a significant short-run effect in the range of $10-20\%$ is unlikely to be detected over a 5-year horizon. Concretely, in the sample of Bradley, Desai, and Kim (1983), the standard error of the CAR estimate for the window from 1 month before the bid until 60 months after the bid is economically large at 15.37%.[5] As a result, even after correcting the CAR estimate of $-1.07\%$ for a "look-ahead" bias of 9%, a long-run-returns analysis would not allow us to reject the existence of any informational effect of a takeover bid between $[-22\%, +38\%]$ at the 5% level. We conclude that the noise inherent in long-run CARs clouds the ability to make precise quantitative assessments of the informational effect.[6]

The empirical approach proposed in this paper, i.e., the comparison of cash and stock targets at announcement and failure, addresses both issues. First, forming comparison groups based on the medium of exchange does not introduce a look-ahead bias, as the choice of cash versus stock is already publicly known at the time of the announcement. Second, our main CAR estimates are orders of magnitude more precise than above-mentioned long-run-returns calculations, since the average time from announcement to failure is 60 days in our sample. Concerns about noise in long-run-returns calculations do affect our additional analysis of the value of future bids due to the longer period between the initial bid and the subsequent bid. However, in the complementary analysis of the likelihood of a future bid, a longer observation horizon allows us to estimate hazard rates more precisely.

[5] The monthly standard error of 1.952% (see Table 3 in Bradley, Desai, and Kim, 1983) implies that the standard error of the long-run CAR from 1 month before the bid until 60 months after the bid is $\sqrt{62} \cdot 1.952\% = 15.37\%$.

[6] In the Online Appendix, we calculate long-run returns in the spirit of Bradley, Desai, and Kim (1983) using our larger sample and employing a calendar-time-portfolio approach. Our results are subject to noise concerns of similar magnitude.

The results in our paper are consistent with earlier evidence by Sullivan, Jensen, and Hudson (1994).[7] Our paper is also closely related to Savor and Lu (2009), whose classification of failure reasons we implement. Different from our approach, they use this classification to compare successful and failed bids. In a similar spirit, Malmendier, Moretti, and Peters (2010) assess the long-run returns to takeovers by comparing the returns of competing bidders in contested takeovers. They find that winners of (long-duration) bidding contests underperform losers in the long run.

The remainder of the paper is organized as follows. In Section 2, we describe our data, and explain our sample selection. Section 3 presents all empirical results. Section 4 concludes.

## 2. Data

We collect data on failed merger bids and tender offers in the U.S. between 1980 and 2008 from the SDC Mergers and Acquisitions database, and merge the data with stock-market and accounting data from CRSP and Compustat. To research failure reasons, we run a news search in LexisNexis, and use the deal synopses provided by SDC.

Our initial sample contains all bids with sufficient information for a basic analysis of the relation between the medium of exchange and target revaluation. That is, first, we require a valid announcement date and a valid completion or failure date within 5 to 250 trading days after the announcement.[8] Second, we exclude bids with competing offers, i.e., offers that are announced before failure of the initial bid, to avoid capturing returns to the competing offer.[9] Third, to ensure meaningful ownership changes, we drop targets of which less than 50% was publicly traded before the takeover bid. Fourth, we require a match in the merged CRSP/Compustat database.[10] Fifth, our analysis requires information about the medium of exchange (cash, stock, or other) and the deal premium. We extrapolate missing deal premia by regressing the available SDC premia on transaction values divided by the target's market capitalization 25 days prior to the bid, and predict out-of-sample premia based on transaction value and market capitalization (where available). Following Officer (2003), we truncate deal premia below 0 and above 200%. Sixth, target stock-market data need to be available 25 days prior to announcement until 25 days post failure. The use of 25 days for the run-up period is motivated by the finding of Schwert (1996) that run-ups do not occur until 21 days before the announcement. The

[7] Sullivan, Jensen, and Hudson (1994) is based on a very small sample (36 observations, 66 without controls), lacks essential control variables (such as hostility, offer premium, market-to-book ratio or other valuation measures), and does not include tender offers.

[8] We impose an upper bound on the days post announcement to avoid capturing information that is unrelated to the offer. None of our results depend on this bound, which affects 5% of the sample.

[9] We correct some of the competing-bid information in SDC based on our news search in LexisNexis. As a robustness check, we have included targets with competing bids in a previous version of the paper. The results with regard to the medium of exchange are robust.

[10] We use the six-digit CUSIP provided in the SDC database to merge the data. When matched with more than one CRSP CUSIP, we choose the CUSIP with the lowest 7th digit (typically 1).

resulting initial data set consists of 969 failed bids.

For our main analysis, we impose three further restrictions. First, we eliminate leveraged buyouts (LBOs), most notably buyouts by target management, given that the information revealed naturally differs from transactions with third-party bidders, leaving us with 809 deals.[11] Second, to rule out other factors that are correlated with the choice of cash versus stock, we require information about hostile bids, deal form (tender offer versus merger), market value of equity, and target $q$ ratios (market value of equity plus assets minus the book value of equity, all divided by assets). This reduces our sample to 675 deals of which 518 deals are pure-cash or pure-stock deals (henceforth pure deals). This constitutes our *large sample*. Third, we account for the fact that only public acquirers have a meaningful choice between cash and stock financing and, thus, restrict our sample to public-to-public transactions. The latter restriction also allows us to control for the relative deal size, i.e., the ratio of the transaction value over the market value of the acquirer's equity, as well as the acquirer's $q$ ratio. The resulting data set constitutes our *main sample*. It consists of 236 unsuccessful takeover bids (183 pure deals). The corresponding sample of successful bids amounts to 1,846 observations (1,268 of which are pure), implying that roughly one-eighth of all deals are unsuccessful. The last restriction focuses our sample on larger and economically important acquisitions, similar to the sample of Healy, Palepu, and Ruback (1992), which is also a prerequisite for obtaining detailed information on failure reasons in our news-search analysis. However, our main results also hold in the *large sample* of 675 deals.[12]

**Summary statistics.** The summary statistics for the *main sample* are in Panel A of Table 1, separately for successful and unsuccessful bids. Successful and unsuccessful deals in our sample are remarkably similar along many dimensions, including the percentage of cash payment offered, transaction value, relative deal size, and offer premium. They differ in the percentage of stock and other payment offered, with less stock and more other payments in successful deals. We also see that deals take longer to be completed than to be withdrawn or rejected. Successful bids are less likely to be hostile, and more likely to be tender offers, than unsuccessful bids. They also feature a higher fraction of experienced bidders. Finally, there is a marginally significant difference in the $q$ ratio of the target, which is higher among successful bids. There are no significant differences, however, in the $q$ ratio of the acquirer nor in the proportion of bids for which the acquirer's $q$ is greater than the target's $q$.

Panel B of Table 1 provides more details on pure deals within the subsample of unsuccessful bids.[13] These deals make up roughly four-fifths of the main sample. There are only few significant differences between cash and stock transactions. Cash deals are more likely to be hostile or tender offers, and both bidders and targets have lower $q$ ratios. There are no significant differences in the acquirer-to-target $q$ ratios.

[11] We thank an anonymous referee for pointing this out. All results are robust to including LBOs.

[12] See, in particular, Appendix-Table B.3.

[13] For completeness, we also show the characteristics of successful pure deals in Appendix-Table B.1.

**Failure reasons.** We categorize the reasons for bid failure based on a detailed news search in LexisNexis and on the deal synopses in SDC. Table 2 shows the main categories. The first five categories summarize cases in which the deal failed due to a negative response of the target to the bid, or due to (typically negative) news about the target. Specifically, "Price too low" indicates that the parties could not agree on the transaction price. "Management rejection" indicates that the target management prevented the takeover, for example by adopting poison pills, by repurchasing shares from the bidder (greenmail), or by deliberately breaching merger covenants. "Shareholder rejection" indicates rejection by shareholders, e.g., leading to an insufficient number of shares being tendered. "Target news (public)" refers to failed deals associated with (typically bad) public news about the target.[14] "Target problems (private)" refers to failed deals where the acquirer discovered bad information in the due-diligence process.

The next two categories summarize reasons that likely affect both the target and the acquirer. "Market problems" summarizes failures due to market-wide downturns, mostly the October 1987 crash, September 11, and the subprime crisis. "Industry problems" are industry-wide shocks such as adverse oil-price developments for oil companies.

The next four categories are all cases in which the endogeneity of failure with respect to target value should be less of a concern. "Regulator" refers to lack of regulatory approval as revealed by our news search or the SDC data. For example, General Electric's proposed acquisition of Honeywell in October 2000 was blocked by the European Commission, in a decision that deviated from the U.S. Department of Justice's view. "Management terms" refers to cases where target management and acquirer management could not agree on organizational issues, such as the nomination of a CEO of the future company. "Bidder problems" summarizes failures due to financing problems on the part of the bidder or other negative news about its business. "Bidder acquired" are sudden cancelations because the bidder itself became the target of an acquisition.

Lastly, the stand-alone category "Alliance" denotes cases in which the bidder and the target entered into another type of cooperation, instead of the takeover. We were unable to retrieve any information about the failure reason for 35 of 236 deals, and had no information beyond which party canceled the takeover for another 51 deals.

We will use this categorization of failure reasons to form two subsamples for which endogenous selection (with respect to target value) should be less of a concern. First, we denote as sample $N$ the subset of deals excluding bids whose failure was clearly endogenous to the target's value or related to extreme market volatility, namely the categories "Target news (public)," "Target problems (private)," "Market problems," and "Industry problems." Second, we consider a more conservative sample $C$, which contains *only* those deals for which we have identified a failure reason that is most likely unrelated to the target's value: regulatory intervention ("Regulator"), unexpected issues on the side of the bidder ("Bidder problems" and "Bidder acquired"), and disagreement on

[14] One deal in our sample failed due to positive news about the target: in August 1996, U.S. Diagnostic Labs called off the acquisition of Alliance Imaging because of a run-up in Alliance's stock price.

management terms or positions ("Management terms").

## 3. Empirical analysis

Our empirical analysis proceeds in two steps. First, we establish the revaluation difference between cash and stock deals – both in the raw data and in a controlled regression framework – and show its long-run persistence. Second, we test whether the differential revaluation of cash and stock targets can be explained by differences in future takeover activity or differences in subsequent operational policies.

### *3.1. Revaluation*

To evaluate revaluation differences in the short run, we examine target returns from 25 trading days pre announcement to 25 trading days post failure. The choice of 25 days is motivated by the findings of Schwert (1996) on pre-bid run-ups. We calculate cumulative abnormal returns ($CAR$) as follows:

$$CAR_{it} = \sum_{j=1}^{t} \left( r_{ij} - r_{mj} \right), \tag{1}$$

where $r_{ij}$ and $r_{mj}$ denote firm $i$'s equity return and the CRSP value-weighted market return at time $j$, respectively.[15] Note that cumulative abnormal returns can be compared across deals with different window lengths from bid to failure as long as the underlying equilibrium asset-pricing model is correctly specified. Moreover, in our analysis, any such model misspecification is likely a second-order concern due to the relatively short length of the event window.[16]

**Univariate results.** Figure 1 previews our first key empirical result. It plots the evolution of cumulative abnormal returns from 25 trading days prior to the announcement to 25 trading days after failure, separately for pure-cash and pure-stock bids and both for targets and for acquirers. The graph illustrates three sets of raw empirical findings. The left part of the graph, from $B-25$ to $B+1$, indicates the average announcement return to the bid, including the run-up period (as in Asquith, 1983). The right part of the graph, from $F-1$ to $F+25$, captures the announcement return to bid failure. The middle part, from $B+1$ to $F-1$, captures the returns during the intermediate period between announcement and failure. Note that, for the graphical illustration of the intermediate period, we normalize trading days (in percent) since the time from bid announcement to failure differs across deals.[17] For example, 50% refers to trading day

[15] We follow the literature in using equity-market values. Ideally, one would use enterprise values, i.e., include the market values of debt, but it is difficult to obtain daily market values of debt. Our approximation mistakes are likely of second order since debt is less sensitive to information.

[16] See, among others, Barber and Lyon (1997), Fama (1998), and Brav (2000) on the statistical concerns affecting the calculation of long-run abnormal returns.

[17] We linearly interpolate between trading days if needed. See Appendix A.1 for details.

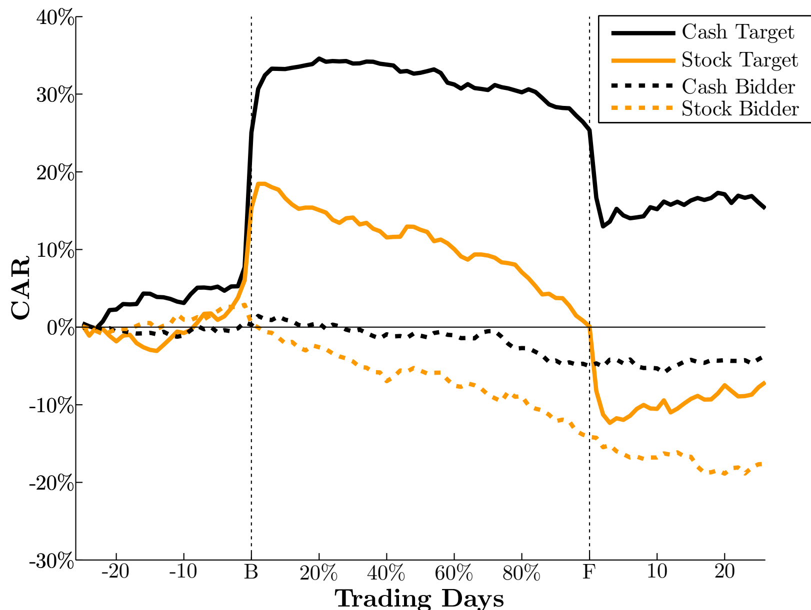


**Fig. 1. Announcement Effects at Bid and at Failure (+/− 25 days).** Cumulative abnormal returns (CARs) from 25 trading days before announcement of the initial bid (B) to 25 trading days after deal failure (F). The sample consists of 81 pure-cash and 102 pure-stock deals (see summary statistics in Panel B of Table 1).

50 if a bid fails after 100 trading days, but refers to trading day 20 if a bid fails after 40 trading days. We note that the intermediate returns reflect continuous updating about the probability of failure, in addition to changes of the valuation conditional on success and failure, respectively.

We observe strong cumulative announcement returns to targets of both cash and stock offers, of 25% and 15% on average, respectively. These magnitudes are very similar to those estimated by Huang and Walkling (1987) in their earlier sample of cash and stock bids, which also included successful bids. Thus, the market assesses (eventually) failed deals to be similar ex ante. At the time of deal failure, however, the value of stock targets falls slightly below the pre-announcement level, to which it ultimately returns. The value of cash targets, instead, remains significantly higher. The typical cash target features cumulative abnormal returns of 15% relative to pre-announcement. Despite a small upward trend for both cash and stock targets after deal failure, stock targets remain on average more than 15% below cash targets.

For completeness, the graph also plots the corresponding acquirer returns. Stock acquirers trade on average at significantly lower prices post failure (−17.6%), whereas the typical cash acquirer is not revalued significantly.

Next, we test whether the 15% revaluation difference between cash and stock targets reverts, or whether it persists over longer horizons. We estimate the long-run abnormal performance of targets in the post-failure period over various horizons up to five years.

Due to the significantly longer horizon, we can no longer rely on the simple abnormal-return calculations shown in equation (1). Instead, we adopt the calendar-time-portfolio approach advocated by Fama (1998) (see also earlier work by Jaffe, 1974, and Mandelker, 1974) to account for the cross-sectional correlation between target firms. For each month from January 1980 until December 2008, we form an equal-weight portfolio of all firms that received an unsuccessful cash or stock offer in the previous $m$ months, where $m \in \{12, 24, 36, 48, 60\}$. For example, medical-device company Cyberonics received a cash offer by St. Jude Medical in April 1996 that was withdrawn in October 1996. Upon deal failure, Cyberonics was in the 60-month cash portfolio between November 1996 and October 2001. We then calculate the alphas of the respective cash and stock portfolios, as well as the long-short (cash minus stock) portfolio in time-series regressions using the CAPM as the underlying asset-pricing model.[18] Since the number of firms in the portfolios is time-varying, we efficiently estimate the coefficients by weighted least squares (see Appendix A.2 for details). We note that one would not expect to see abnormal post-failure returns in an efficient market, as this portfolio strategy can be implemented without forward-looking information.

Table 3 presents our results on long-run post-failure returns. Note that the alpha estimate of the long-short portfolio is generally not the difference between the respective cash and stock alphas, since a monthly observation is included only if both the cash and the stock portfolio are non-empty. We find that, for both the main sample and the large sample, all portfolio alphas (cash, stock, long-short) are insignificant for each horizon $m$. Hence, we do not find evidence that the revaluation difference between cash and stock targets reverses in the long run.

**Multivariate-regression analysis.** Next, we return to our chosen event-study window $(B - 25, F + 25)$, and estimate revaluation differences in a controlled regression framework. In this manner, we test whether revaluation differences between cash and stock targets reflect other observable deal- or entity-specific characteristics such as hostility, tender offers, or relative deal size.

The multivariate-regression analysis is presented in Table 4. As a benchmark, we first regress the target CAR on the fraction of cash offered without further controls. This replicates the graphical evidence, and provides robust standard errors. We estimate a cash coefficient of 22.4% in the main sample (column 1) and 22.1% in the pure-deals sample (not shown in the table). Both estimates are significant at the 1% level.

In columns 2 and 3, we add control variables for deal- and entity-specific characteristics that are correlated with the medium of exchange and might reflect the target's stand-alone value: relative deal size, acquirer $q$ and target $q$ (which correlate negatively with the use of cash), as well as dummies for hostile and tender bids (which both correlate positively with the use of cash).[19] Including such controls is important since Jensen and Ruback (1983) find that only targets in unsuccessful tender offers are positively revalued.

[18] All of our results are robust to using the Fama and French (1993) three-factor model.

[19] See Appendix-Table B.2 for an analysis of the correlates of the medium of exchange.

In addition, we control for target size, offer premium, industry fixed effects, and year fixed effects. Note that the industry and year fixed effects should also mitigate confounds with accounting-rule changes, such as Statement 142 of the Financial Accounting Standards Board (FASB) abolishing the pooling-of-interests method in 2001.[20]

After including these controls, the cash coefficient remains similar, 19.9% in the main sample (column 2) and 22.6% in the pure-deals sample (column 3). The analysis also reveals that, in addition to the medium of exchange, deal premia correlate strongly with target CARs. Intuitively, the more the bidder is willing to pay, the higher is the market revaluation. We also find that smaller targets are revalued more, possibly reflecting that a bid conveys more new information to the market if the target is small.

The cash effect is also present in the large sample of 675 bids, which includes non-public acquirers (see Appendix-Table B.3). The point estimates become somewhat smaller, but are still statistically and economically significant. The smaller magnitudes may be explained by the fact that private acquirers have less of a choice between cash and stock, making a cash offer a weaker signal of target value.

**Endogenous selection into deal failure.** So far, we have shown that variables known at the time of deal announcements do not explain the differential revaluation of cash targets. A different concern is selection into deal failure, as the choice of cash versus stock payment might be correlated with deal failure. The summary statistics in Panel A of Table 1 reveal that the fraction of the total payment offered in cash does not correlate with deal failure, i.e., the fraction of cash offered in successful deals (45.93%) is very similar to that in unsuccessful deals (44.03%). However, it is still possible that revaluation differences between cash and stock targets are driven by differential sorting of cash and stock deals into failure. For example, good news about the target might make cash deals, but not stock deals, more likely to fail because a financially constrained bidder is unable to increase the bid in cash, leading to overproportional failure of cash bids for targets with high (re-)valuation.

We have previously addressed this specific concern in the example above by controlling for financial constraints of the acquirer (as measured by the Kaplan and Zingales, 1997, index). In all specifications based on Table 4, the coefficients on the KZ index as well as its interactions with cash are insignificant.[21] However, even if this specific concern

[20] FASB Statement 142 requires acquirers to record net target assets at their fair value (purchase method) for fiscal years beginning after December 15, 2001. The difference between purchase price and asset value is allocated to goodwill and amortized over a maximum period of 40 years. Until 2001, stock acquirers (but not cash acquirers) could also use the pooling-of-interests method and combine the balance sheets of the merging entities to a consolidated balance sheet. Acquirers often preferred the pooling-of-interests method to avoid the amortization of goodwill and, thus, future reductions in reportable earnings, see Aboody, Kasznik, and Williams (2000) and Jennings, LeClere, and II (2001), possibly tilting the medium of exchange towards stock. Lys and Vincent (1995) describe an extreme case – AT&T's acquisition of NCR – of the bidder's interest in having the acquisition qualify as a pooling of interests. In additional regressions (unreported), we include an interaction term between cash and a dummy for the pre-2002 period, but fail to find a significant effect.

[21] We omit these results, which are available upon request, in the paper for the sake of brevity.

does not apply, the more general argument remains, and can be illustrated as follows. Suppose that there are only two failure reasons, $A$ and $B$. Failure reason $A$ is associated with targets that will be revalued by 30%, and failure reason $B$ is associated with targets that will be revalued by $-10\%$, which holds for both cash and stock targets. If 75% of all cash deals, but only 25% of all stock deals, occur in category $A$, then we should observe an overall revaluation effect of 20% for cash deals and 0% for stock deals, even though, within each category, there is no differential revaluation effect of cash and stock deals.

To address the concern that our differential revaluation estimates may be driven by specific deal-failure categories, we make use of our hand-collected sample of failure reasons. We re-estimate the cash coefficient for the sample $N$, described in Section 2, which excludes bids whose failure was endogenous to the target's value or caused by market- or industry-wide problems, which (also) affect the target. The results are shown in columns 4 to 6 of Table 4. In all regression specifications, we continue to estimate a positive cash coefficient, statistically significant and very similar in size to those estimated for the main sample in columns 1 to 3.

We also re-estimate the cash coefficient for the more conservative sample $C$. As outlined in Section 2, sample $C$ consists only of deals that failed due to regulatory intervention, news about the bidder, or disagreement on management terms. The results are shown in the second-to-last row of Table 2. Here, the small sample size confines us to a univariate regression, mirroring columns 1 and 4 of Table 4. It is interesting to see that we estimate a very similar cash coefficient of 19.2%, which is significant at the 5% level. Moreover, as shown in the second column of Table 2, we also re-estimate the cash coefficient separately for every single failure category identified by our news search. We find a positive cash coefficient for all of the 12 categories except "Market problems." The latter reflects that revaluation estimates for individual companies during market crashes like "September 11" or "October 1987" are extremely volatile, even after adjusting for market returns.

Subject to the caveat that we can only address selection based on publicly available information, the robustness of our results across failure categories suggests that selection into deal failure is unlikely to drive the (differential) cash effect on target CARs.

### *3.2. Possible channels*

To understand the source of the revaluation difference between cash and stock targets, we consider two channels that the literature has deemed important. First, a failed offer identifies a firm as a likely target of further takeover bids, and the anticipated future takeover premia lead to revaluation (see Bradley, Desai, and Kim, 1983, and, more recently, Edmans, Goldstein, and Jiang, 2012). Second, a failed takeover bid might induce the target management to make operational improvements (see, e.g., Safieddine and Titman, 1999). For our purposes, the relevant question is whether cash and stock deals are differentially exposed to these channels. Do failed cash bids induce higher future takeover premia than failed stock bids? Do they prompt better operational changes?

### 3.2.1. *Future takeover activity*

To assess the empirical significance of future takeovers for the cash-stock revaluation difference, we test whether the likelihood and timing of subsequent offers as well as their value are related to the medium of exchange used in the prior (failed) takeover attempt. We benchmark the analysis against matched firms that have similar characteristics as the target firms in our sample but did not receive a bid. We identify these control firms employing propensity-score matching. Within the universe of Compustat firms, we estimate a fixed-effects (conditional) logit model for the event that firm $i$ receives a takeover offer (successful or not) in year $t$:

$$\Pr\{takeover_{it} = 1 | x_{i,t-1}\} = G(\beta' x_{i,t-1}), \tag{2}$$

where $G(\cdot)$ denotes the cumulative logistic distribution and $x_{i,t-1}$ is a vector of control variables that includes firm $i$'s $q$ ratio, market capitalization (in 2010 \$), book value of total assets (in 2010 \$), and return on equity (net income over book value of equity), all measured at the end of year $t-1$, as well as industry-year fixed effects (according to two-digit SIC codes).

We use the predicted probabilities, or propensity scores, from the logit regression to match each target firm in the sample of failed bids to the control firm with the closest propensity score in the year of the respective failed bid. In doing so, we limit the set of potential control firms to those (i) operating in the same industry (according to two-digit SIC codes) as the matched target firm and (ii) not having received any publicly disclosed takeover offer in the previous five years.

For each failed bid in our sample, we measure the time from deal failure to the announcement date of an eventually successful bid or, in case the target never receives a successful takeover bid, the censoring date, which is determined by the minimum of the target's bankruptcy date and the last trading day in our data.[22] Future successful bids include returning bidders of an earlier failed deal, although the vast majority are made by other bidders.[23] For control firms, we measure the time to arrival of a successful takeover bid starting from deal failure of the matched sample firm.

Figure 2 plots the corresponding Kaplan-Meier graphs of future takeovers for sample firms and matched control firms. Since this step of the analysis does not rely on the detailed data requirements of our main sample, we make use of our large sample (675 observations), which includes non-public acquirers. We eliminate 8 observations for which

[22] We use the following CRSP delisting codes to identify bankruptcy: any type of liquidation (400-490); price fell below acceptable level; insufficient capital, surplus, and/or equity; insufficient (or non-compliance with rules of) float or assets; company request, liquidation; bankruptcy, declared insolvent; delinquent in filing; non-payment of fees; does not meet exchange's financial guidelines for continued listing; protection of investors and the public interest; corporate governance violation; and delist required by Securities Exchange Commission (SEC).

[23] In Appendix-Table B.4, we summarize information on follow-up bids by the same bidder. Successful takeovers by returning bidders occur on average after one and a half years after the original failed bid; future unsuccessful bids occur about one and two-third years after the original bid.

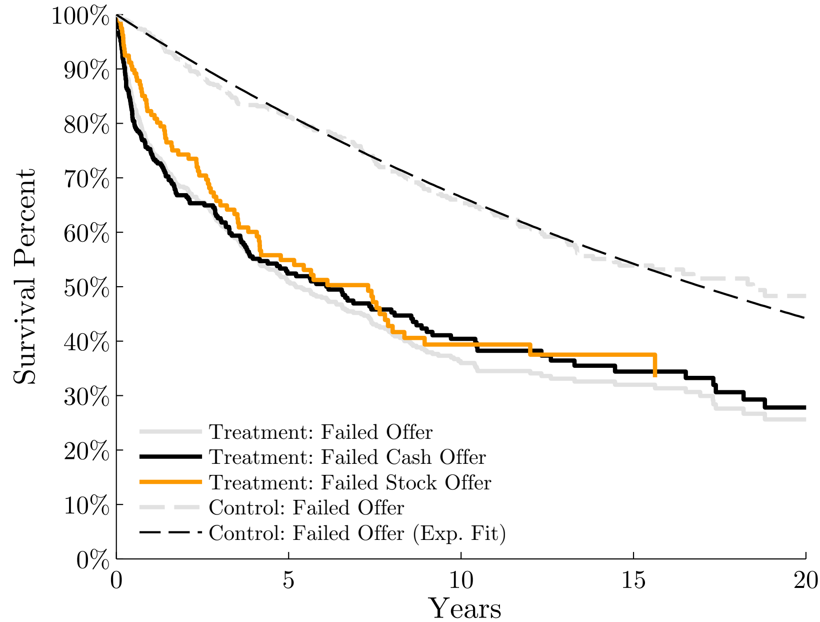


**Fig. 2. Kaplan-Meier Survival Estimator.** This graph plots the arrival rates of takeover bids for sample firms and matched control firms over 20 years after an unsuccessful takeover bid (adjusted for bankruptcy-induced censoring). For each year, the base is the set of surviving firms, and the event is the announcement day of an eventually successful takeover bid. The sample consists of all deals in the large sample (starting January 1985) for which we can identify prior takeover activity. Within this sample of 667 failed bids, there are 348 pure-cash and 164 pure-stock bids. The estimated exponential arrival rate for matched control firms is 4.1%.

we cannot verify whether the firm received an offer in the previous five years, resulting in a sample of 667 failed bids. (The graphs look essentially identical if we include these 8 deals. The graphs also look the same if we use our main, rather than the large, sample.)

Figure 2 illustrates several important facts. First, recipients of a previously failed offer ("Treatment: failed offer") are significantly more likely to be ultimately taken over than the control group of firms matched on observable characteristics ("Control: failed offer"). For example, after five years 50% of the firms with an initial failed bid have been taken over, relative to 20% in the control sample. Second, even control firms are taken over at a high rate, 4.1% per year as determined by exponential fit. Hence, the stock prices of control firms – and therefore also the stock prices of actual target firms just before the announcement of the (ultimately failed) takeover bid – should reflect substantial expected future takeover activity and corresponding takeover premia. Third, target firms with a failed offer exhibit higher takeover activity for a long period, namely over the subsequent 8 years post failure. After 8 years, the arrival rate of takeovers is (almost) perfectly described by an exponential arrival rate of 4.3%, which is very close to the arrival rate of the matched control firms.

Finally, and of greatest importance to our analysis, Figure 2 reveals that cash and stock targets exhibit no *differential* takeover activity over the next 20 years following

the failed takeover attempt. We verify the lack of statistically significant differences in Cox proportional-hazard regressions, both for the main sample and for the large sample. Table 5 reports hazard ratios for the event that the target of a failed bid eventually receives another, successful takeover offer. Hazard ratios for the cash coefficient in excess of 1 indicate by how much the rate of future takeover offers exceeds the rate for stock targets. As can be seen across all columns of Table 5, targets of failed cash bids are no more likely to receive future takeover offers than targets of failed stock bids, irrespective of whether we consider the main sample, the large sample, or the subset of pure deals. We conclude that cash and stock targets are not subject to differential future takeover activity in terms of their timing.

We now turn to the value of future offers. Even if the frequencies of future bids for cash and stock targets are not significantly different, the higher revaluation of cash targets might reflect higher future bids. In Table 6, we relate the dollar value of the next offer to the medium of exchange in the initial failed offer, controlling for the usual array of firm, deal, industry and time variables, and conditional on the existence of successful future takeover attempts.[24] We use two alternative measures of target size as control variables, which allows us to capture two different hypothetical counterfactuals. In columns 1 and 2, target size is measured as market capitalization one month prior to the failed bid and, thus, prior to any revaluation induced by the bid. Using this measure of target size, any difference in future bids, even if proportional to the differential revaluation of cash and stock targets post failure, is attributed to the original medium of exchange. A caveat of using the value of the target before the original bid to normalize takeover premia is that the resulting regression estimates are subject to the usual precision problems of long-run-returns studies (see our discussion of Bradley, Desai, and Kim, 1983 in the introduction). In columns 3 and 4, instead, we control for the value of the target one month prior to the subsequent bid, as motivated by the findings of Schwert (1996). This approach ensures that stock-market noise between announcements does not affect our estimates. In all specifications, we account for the timing of subsequent takeover bids by controlling for the time between announcements (in years).

In both sets of regressions, the coefficient estimate for cash is insignificant. That is, regardless of whether we test for differences in bid value relative to the original target value or relative to its value at the time of the next bid, we cannot reject the hypothesis that cash and stock targets receive equal dollar premia on subsequent bids. In the last two columns of Table 6, we control for both previous and contemporaneous target size. The cash coefficient is again insignificant.[25] Note that, in the specification controlling for previous target size (columns 1 and 2), the coefficient on the control for time between bids (10.1% and 5.8%) can be interpreted as the annualized real risk-adjusted discount rate.[26]

[24] See Appendix-Table B.5 for the same analysis in the subsample of pure deals.

[25] The much larger magnitude of the contemporaneous-size coefficient, compared to the previous-size coefficient (both of which add up to roughly one), and the comparison of the $R^2$ across specifications imply that contemporaneous target size is the relevant reference point, consistent with Schwert (1996).

[26] By estimating the discount rate, we do not have to impose an appropriate discount rate on our own.

In sum, we find that future takeovers of cash and stock targets are similar in their timing and value, suggesting that the revaluation difference pertains to the target's stand-alone value. We now analyze whether the changes in stand-alone value can be related to anticipated changes of operational policies, the catalyst channel.

### *3.2.2. Change in operational policies*

Failed takeover attempts can serve as a catalyst inducing target managers to improve their operational policies. For example, Safieddine and Titman (1999) report that targets of failed takeover attempts tend to increase their leverage, especially after hostile bids, and that such targets with increased leverage exhibit superior operating performance and are less likely to be taken over in the future.

The catalyst effect can explain our results if it is stronger for cash than for stock deals. In light of the analysis of Safieddine and Titman (1999), we first note that all of our multivariate regressions, in particular in Table 4, control for hostility. We then consider the following outcome variables, which have been used as proxies for operational change in prior literature: the sum of short-term and long-term debt, employment, capital expenditure, R&D expenses, and – as a proxy for asset sales – the book value of assets. For each of these outcome variables $y$, we consider raw changes ($\Delta \log y$), changes scaled by assets $A$ ($\Delta \log(y/A)$), and scaled changes relative to the respective matched control firm ($\Delta_{T,C} \Delta \log(y/A) = \Delta \log(y/A) - \Delta \log(y_C/A_C)$), from the calendar year-end before (the year of) deal announcement to the calendar year-end after (the year of) deal failure.[27] For example, for the variable "debt" this reflects total changes in debt, changes in book leverage, and changes in book leverage relative to the matched control firm.

For both the main and the large sample, we regress these outcome variables on cash and our usual set of control variables (offer premium, indicator variables for hostility and tender offer, and $q$ of target). We include industry and year fixed effects when we do not subtract the corresponding change in the matched control firm, as the control firms were matched in part based on those variables. For ease of exposition, Table 7 reports only the cash coefficients from 26 separate regressions. Note that, in the specifications with the book value of assets as the dependent variable (last column), scaling (by assets) is not feasible in a meaningful manner.

In 25 of our 26 regression specifications, the cash coefficient is insignificant, suggesting that there are no meaningful differences in the operational changes between cash and stock targets. We find only one instance in which the cash coefficient is significant: in the main sample, targets of cash offers feature a higher growth rate of employment if scaled by assets. However, this result for employment is neither existent in the large sample, nor is it robust to the other two definitions of the outcome variable.

In unreported regressions, we also analyze CEO turnover as a possible catalyst out-

The size of the estimated coefficients is economically sensible.

[27] This selection requires survival of the respective companies as a stand-alone entity.

come. For example, Mikkelson and Partch (1997) provide evidence of a positive relationship between takeover activity and top-management turnover during the hostile-takeover wave in the 1980s. Here, we investigate whether the use of cash versus stock in the initial takeover bid is related to CEO turnover after deal failure. We can analyze this relationship only within the (smaller) set of targets that is covered by the ExecuComp database, which starts only in 1992. We fail to find any indication that failed cash bids are more likely than failed stock bids to be followed by CEO turnover.

In sum, we cannot detect any post-failure operational differences between cash and stock targets. It is important to stress that the lack of operational differences does not imply that the catalyst channel is irrelevant altogether. The result merely states that the catalyst channel may not be *differentially* at work for cash and stock targets, which is the main concern in our analysis. Moreover, our analysis relies on the usual metrics of operational policies (see, for instance, Safieddine and Titman, 1999) based on public databases such as Compustat/ExecuComp. Hence, our approach potentially fails to detect differences in operational policies along dimensions that would require deeper drilling into managerial decision making at a granular level for a large number of firms.

## 4. Conclusion

Our paper documents a robust link between revaluation of targets in failed takeover bids and the medium of exchange: targets of cash offers typically trade 15% above their pre-announcement level, whereas targets of stock offers are not revalued on average. We relate our differential revaluation estimates for cash and stock targets to future takeover activity, a plausible channel for revaluation (Bradley, Desai, and Kim, 1983). While we find strong evidence that targets of failed bids are more likely to receive future takeover bids than matched control firms for up to 8 years post failure, we do not detect any differential effects for cash and stock targets. Our results imply that the differential revaluation of cash and stock targets is not a by-product of future takeover activities. We also cannot detect differential subsequent operational policies between cash and stock targets. Hence, our findings are most consistent with papers such as Rhodes-Kropf and Viswanathan (2004), which suggest that the choice of the medium of exchange reveals information to the market about the stand-alone value of the entities involved.

By "ruling in" the possibility of information effects of takeover bids, in contrast to earlier literature, we hope that our results will help to rekindle the classical debate about the relative importance of information revelation about the target vis-à-vis real effects induced by takeover bids. Our evidence suggests that future work in this area ought to account for the informational implications of the medium of exchange not just on the bidder side (see Bhagat, Dong, Hirshleifer, and Noah, 2005), but also on the side of the target.

## Appendix A. Methodology

### *A.1. Linear approximation*

The time interval between announcement of the initial bid and failure of the deal varies across the sample. For the graphical illustration, we normalize this window to relative time, i.e., between $t_R = 0$ and $t_R = 100\%$. Suppose a deal has 40 days between announcement and failure, i.e., $T_i = 40$. Then, the cumulative abnormal return after $t_R = 5\%$ relative time, $\widehat{CAR}_i(5\%)$, is equal to the cumulative abnormal return after $40 \cdot 5\% = 2$ actual trading days, i.e., $CAR_i(t_R T_i)$. If $t_R T_i$ is not an integer number, we use linear interpolation between the actual trading days, i.e.,

$$\widehat{CAR}_i(t_R) = \left(1 - w_{(i,t_r)}\right) CAR_i\left(\lfloor t_R T_i \rfloor\right) + w_{(i,t_r)} CAR_i\left(\lfloor t_R T_i \rfloor + 1\right), \tag{A.1}$$

where $\lfloor x \rfloor$ refers to the floor function and $w_{(i,t_r)} = t_R T_i - \lfloor t_R T_i \rfloor$. For example, if $T_i = 40$ days and $t_R = 8\%$, then $w_{(i,t_r)} = 40 \cdot 8\% - \lfloor 40 \cdot 8\% \rfloor = 0.2$, so that the cumulative abnormal return after 8% relative time has passed is given by $\widehat{CAR}_i(8\%) = 0.8CAR_i(3) + 0.2CAR_i(4)$.

### *A.2. Long-run abnormal returns*

Denote the calendar-month return on our post-failure target portfolio by $R_{p,t}$. To calculate the corresponding abnormal returns, we use the capital asset pricing model (CAPM), and run the following regression:

$$R_{p,t} - R_{f,t} = \alpha_p + \beta_p(R_{m,t} - R_{f,t}) + \varepsilon_{p,t}, \tag{A.2}$$

where $R_{f,t}$ is the one-month treasury bill rate, $R_{m,t}$ is the monthly return on the CRSP value-weighted NYSE/AMEX/NASDAQ broad market index, and $\alpha_p$ captures the monthly abnormal return on the post-failure target portfolio.

We account for the fact that monthly returns with more firms entering the respective portfolio are more precisely estimated than months with few firms. The residual variance of portfolio $p$ in month $t$ with $N_{p,t}$ equally-weighted firms is given by:

$$Var\left(\varepsilon_{p,t}\right) = Var\left(\sum \frac{1}{N_{p,t}} \varepsilon_{i,t}\right) = \frac{\overline{Var\left(\varepsilon_i\right)}}{N_{p,t}} + \frac{N_{p,t} - 1}{N_{p,t}} \bar{\rho} \overline{Var\left(\varepsilon_i\right)}, \tag{A.3}$$

where $\overline{Var\left(\varepsilon_i\right)}$ is the average residual variance of all stocks and $\bar{\rho}$ is defined such that $\bar{\rho}\overline{Var\left(\varepsilon_i\right)}$ represents the average covariance across all stocks. (Target firms in a given month are predominantly in similar industries, so that we expect residuals to be positively correlated.) Since the most important change from month to month concerns the number of firms, $N_{p,t}$, variations in $\overline{Var\left(\varepsilon_i\right)}$ and $\bar{\rho}$ are second order, so that the variance of an equal-weight portfolio scales with $\frac{1}{N_{p,t}} + \frac{N_{p,t}-1}{N_{p,t}}\bar{\rho}$. Based on the empirical results of Campbell, Lettau, Malkiel, and Xu (2001), we choose $\bar{\rho} = 0.1$. We then apply weighted least squares.

For the long-short portfolio, the monthly variance is given by:

$$\begin{aligned} Var\left(\varepsilon_{C,t}-\varepsilon_{S,t}\right) &= Var\left(\varepsilon_{C,t}\right)+Var\left(\varepsilon_{S,t}\right)-2Cov\left(\varepsilon_{C,t},\varepsilon_{S,t}\right) \\ &= \overline{Var\left(\varepsilon_{i}\right)}\left(1-\bar{\rho}\right)\left(\frac{1}{N_{C,t}}+\frac{1}{N_{S,t}}\right). \end{aligned} \quad \text{(A.4)}$$

The second line follows from assuming that cash and stock firms share average residual variances and average residual covariances. For the long-short portfolio, we thus obtain that the variance scales with $\frac{1}{N_{C,t}}+\frac{1}{N_{S,t}}$.

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**Table 1. Summary Statistics**

Statistics for the *main sample*, as described in Section 2. Time to completion/failure is measured in trading days. Dollar values are based on historical transaction values, converted to 2010 $bn using Consumer Price Index (CPI) Conversion Factors. Target size is the target's market value of equity in 2010 $bn. Relative deal size is the transaction value over the acquirer's market value of equity. Offer premium is the payment to target shareholders normalized by the target's market capitalization at one month prior to the announcement of the bid, and truncated between 0 and 2. Hostile and Tender offer are dummy variables indicating hostile bids and tender offers, respectively. The $q$ ratio is the market value of equity plus assets minus the book value of equity, all over assets. Experienced acquirers (dummy variable) have attempted (successfully or not) at least ten acquisitions in the five years up to the year of the takeover bid in question. All non-deal-related variables are measured at the end of the year prior to the deal's announcement, and all $q$ variables are winsorized at the 1st and 99th percentiles. The p-values in the last column are for a two-sided difference-in-means test. Both panels show the main sample, i.e., bids for which all control variables are available. In Panel B, the sample is restricted to unsuccessful pure deals.

**Panel A**: Successful and Unsuccessful Bids

| | Successful Bids | | | | | Unsuccessful Bids | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Variable | Mean | Median | Std. dev. | Min | Max | Mean | Median | Std. dev. | Min | Max | p-value |
| Cash in % | 45.93 | 37.16 | 45.47 | 0 | 100 | 44.03 | 19.19 | 45.71 | 0 | 100 | 0.546 |
| Stock in % | 46.48 | 43.06 | 45.39 | 0 | 100 | 55.49 | 75.18 | 45.79 | 0 | 100 | 0.004 |
| Other payment in % | 7.59 | 0.00 | 18.20 | 0 | 100 | 0.48 | 0.00 | 5.94 | 0 | 100 | 0.000 |
| Time to completion/failure | 76.10 | 68.00 | 43.16 | 5 | 245 | 62.93 | 50.50 | 47.46 | 5 | 232 | 0.000 |
| Transaction value in 2010 $bn | 1.46 | 0.31 | 4.34 | 0.00 | 70.51 | 1.64 | 0.15 | 7.04 | 0.01 | 77.04 | 0.601 |
| Target size in 2010 $bn | 0.91 | 0.18 | 2.98 | 0.00 | 65.16 | 1.24 | 0.11 | 5.61 | 0.00 | 56.04 | 0.156 |
| Relative deal size | 1.54 | 0.21 | 25.34 | 0.00 | 830.22 | 1.16 | 0.52 | 2.24 | 0.00 | 17.57 | 0.816 |
| Offer premium in % | 46.24 | 37.72 | 38.66 | 0 | 200 | 46.59 | 38.56 | 42.77 | 0 | 200 | 0.897 |
| Hostile | 0.01 | 0.00 | 0.12 | 0 | 1 | 0.14 | 0.00 | 0.35 | 0 | 1 | 0.000 |
| Tender offer | 0.24 | 0.00 | 0.42 | 0 | 1 | 0.10 | 0.00 | 0.30 | 0 | 1 | 0.000 |
| $q$ of acquirer | 2.51 | 1.70 | 2.42 | 0.51 | 15.20 | 2.36 | 1.47 | 2.58 | 0.51 | 15.20 | 0.396 |
| $q$ of target | 2.06 | 1.45 | 1.73 | 0.50 | 9.91 | 1.85 | 1.23 | 1.61 | 0.50 | 9.91 | 0.080 |
| $q$ of acquirer > $q$ of target | 0.63 | 1.00 | 0.48 | 0 | 1 | 0.62 | 1.00 | 0.49 | 0 | 1 | 0.796 |
| Experienced acquirer | 0.22 | 0.00 | 0.41 | 0 | 1 | 0.14 | 0.00 | 0.35 | 0 | 1 | 0.010 |
| % of target sought | 98.76 | 100.00 | 6.09 | 50.2 | 100 | 96.86 | 100.00 | 9.04 | 50.80 | 100 | 0.000 |
| N | | | 1,846 | | | | | 236 | | | |

**Panel B:** Unsuccessful Pure Cash and Stock Bids

| | Cash Bids | | | | | Stock Bids | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Variable | Mean | Median | Std. dev. | Min | Max | Mean | Median | Std. dev. | Min | Max | p-value |
| Time to failure | 60.27 | 51.00 | 45.40 | 5 | 188 | 59.90 | 47.00 | 47.58 | 5 | 232 | 0.958 |
| Transaction value in 2010 $bn | 0.66 | 0.12 | 1.49 | 0.01 | 8.88 | 1.36 | 0.10 | 5.79 | 0.01 | 55.64 | 0.289 |
| Target size in 2010 $bn | 0.40 | 0.09 | 0.77 | 0.00 | 4.09 | 1.32 | 0.09 | 5.76 | 0.00 | 56.04 | 0.155 |
| Relative deal size | 1.22 | 0.41 | 2.32 | 0.00 | 11.42 | 0.84 | 0.50 | 1.18 | 0.01 | 8.49 | 0.157 |
| Offer premium in % | 53.13 | 45.80 | 38.55 | 0 | 200 | 45.02 | 37.98 | 43.76 | 0 | 200 | 0.192 |
| Hostile | 0.27 | 0.00 | 0.45 | 0 | 1 | 0.04 | 0.00 | 0.20 | 0 | 1 | 0.000 |
| Tender offer | 0.23 | 0.00 | 0.43 | 0 | 1 | 0.01 | 0.00 | 0.10 | 0 | 1 | 0.000 |
| $q$ of acquirer | 1.74 | 1.32 | 1.61 | 0.51 | 13.72 | 2.95 | 1.65 | 3.16 | 0.58 | 15.20 | 0.002 |
| $q$ of target | 1.47 | 1.12 | 1.26 | 0.50 | 9.91 | 2.27 | 1.52 | 1.85 | 0.55 | 9.91 | 0.001 |
| $q$ of acquirer $>$ $q$ of target | 0.64 | 1.00 | 0.48 | 0 | 1 | 0.60 | 1.00 | 0.49 | 0 | 1 | 0.546 |
| Experienced acquirer | 0.10 | 0.00 | 0.30 | 0 | 1 | 0.13 | 0.00 | 0.34 | 0 | 1 | 0.548 |
| % of target sought | 94.78 | 100.00 | 11.86 | 50.80 | 100 | 97.63 | 100.00 | 7.87 | 62.00 | 100 | 0.053 |
| N | | | 81 | | | | | 102 | | | |

**Table 2. Failure Categories**

The category Price too low denotes failed deals where the parties could not agree on the transaction price. Management rejection refers to deals that failed because the shareholders and/or management or board refused the bid, whereas Shareholder rejection indicates rejection by shareholders, e.g., leading to an insufficient number of shares being tendered. Target news (public) refers to failed deals associated with (good or bad) public news about the target, and Target problems (private) to failed deals where the acquirer discovered bad information in the due-diligence process. Market problems denotes deal failure due to shifting market conditions (typically stock-market plunges), whereas Industry problems are pertinent to the target's and/or the acquirer's industry. Regulator refers to deal failure where the news search revealed lack of regulatory approval. Management terms describes all failed deals where acquirer and target were unable to agree on terms other than the price (e.g., the nomination of a CEO of the future company). Bidder problems summarizes deal cancelations due to financing problems or other bad news on the part of the bidder. Bidder acquired are sudden cancelations triggered by the acquisition of the bidder. Alliance denotes failed bids after which bidder and target entered into other cooperations. A deal could be assigned to multiple categories. We denote the sample of bids that were *not* withdrawn due to news regarding the target or market/industry problems as sample $N$; the results are in Table 4. We denote the sample containing only bids that were canceled due to regulatory issues, bidder news, or disagreement on management terms as sample $C$. The sample Main is the *main sample* of all unsuccessful bids as defined in Section 2. The column entitled Average % cash shows the average percentage of the transaction value offered in cash. The columns Cash coefficient target and Cash coefficient acquirer show the coefficient estimates from regressing, respectively, the target's and the acquirer's CAR from 25 days before announcement to 25 days after deal failure on the fraction offered in cash and a constant. */**/*** denote significance at the 10%/5%/1% level, respectively.

| Failure reason | Avg. % cash | Cash coeff. target | Cash coeff. acquirer | N |
|---|---|---|---|---|
| Price too low | 57.0% | 0.241** | 0.075 | 25 |
| Management rejection | 57.5% | 0.234 | 0.103 | 27 |
| Shareholder rejection | 66.7% | 0.833** | 0.241 | 12 |
| Target news (public) | 40.2% | 0.489 | -0.067 | 19 |
| Target problems (private) | 28.9% | 0.084 | 0.312 | 8 |
| Market problems | 43.3% | -0.335 | 0.616*** | 15 |
| Industry problems | 29.4% | 0.045 | 0.014 | 4 |
| Regulator | 48.1% | 0.321** | 0.251** | 49 |
| Management terms | 33.1% | 0.101 | 0.185 | 13 |
| Bidder problems | 20.6% | 0.090 | 0.787*** | 22 |
| Bidder acquired | 33.3% | 0.789 | 0.422 | 3 |
| Alliance | 35.9% | 0.146 | 0.247 | 11 |
| All bids with failure reason | 42.4% | 0.166* | 0.231*** | 150 |
| All bids in sample $C$ | 39.3% | 0.192** | 0.347*** | 81 |
| All bids (Main) | 44.0% | 0.224*** | 0.127** | 236 |

**Table 3. Long-term Persistence of Cash vs. Stock Revaluation Differences**

CAPM calendar-time-portfolio estimates of alpha (in percent per month) based on WLS regressions of monthly premium of portfolio relative to the one-month treasury rate (as the dependent variable) on monthly market return, as well as long/short difference between portfolio returns. We form equal-weight portfolios of targets that received an unsuccessful pure-cash or pure-stock bid in the previous $n$ years, where $n$ varies from 1 to 5 (across rows). We use the *main sample* in the first three columns, and the *large sample* with (also) targets of non-public acquirers in the last three columns. Long/short portfolios go long in cash targets, and short in stock targets. N is the number of months with non-empty portfolios. Observations are weighted as explained in Appendix A.2. Robust standard errors are in parentheses. */**/*** denote significance at the 10%/5%/1% level, respectively.

| | Main sample | | | Large sample | | |
|---|---|---|---|---|---|---|
| Alpha | Cash | Stock | Long/short | Cash | Stock | Long/short |
| 1 year | 0.65 | 0.44 | 0.21 | -0.29 | 0.61 | -1.08 |
| | (0.75) | (0.71) | (1.14) | (0.37) | (0.61) | (0.70) |
| $R^2$ | 0.13 | 0.13 | 0.00 | 0.28 | 0.19 | 0.02 |
| N | 267 | 275 | 249 | 309 | 290 | 290 |
| 2 years | 0.11 | 0.69 | -0.36 | 0.05 | 0.65 | -0.70 |
| | (0.57) | (0.55) | (0.81) | (0.33) | (0.46) | (0.52) |
| $R^2$ | 0.17 | 0.26 | 0.01 | 0.35 | 0.33 | 0.04 |
| N | 293 | 290 | 290 | 321 | 293 | 293 |
| 3 years | 0.41 | 0.40 | 0.27 | 0.29 | 0.52 | -0.20 |
| | (0.53) | (0.46) | (0.67) | (0.29) | (0.39) | (0.43) |
| $R^2$ | 0.19 | 0.34 | 0.02 | 0.42 | 0.42 | 0.08 |
| N | 293 | 293 | 293 | 330 | 293 | 293 |
| 4 years | 0.48 | 0.27 | 0.34 | 0.26 | 0.38 | -0.06 |
| | (0.48) | (0.42) | (0.60) | (0.28) | (0.36) | (0.39) |
| $R^2$ | 0.21 | 0.38 | 0.03 | 0.43 | 0.45 | 0.07 |
| N | 293 | 293 | 293 | 331 | 293 | 293 |
| 5 years | 0.55 | 0.42 | 0.20 | 0.26 | 0.51 | -0.16 |
| | (0.44) | (0.40) | (0.53) | (0.26) | (0.35) | (0.35) |
| $R^2$ | 0.25 | 0.40 | 0.03 | 0.47 | 0.47 | 0.08 |
| N | 293 | 293 | 293 | 331 | 293 | 293 |

**Table 4. Cash vs. Stock Revaluation Differences in a Controlled Regression Framework**

OLS regressions with target CAR from 25 days before announcement to 25 days after deal failure as the dependent variable. The sample Main consists of all unsuccessful bids in the *main sample* as defined in Section 2, the sample Main, Pure of all unsuccessful pure-cash and pure-stock bids from the *main sample*. In the last three columns, we limit the respective samples to bids that were not withdrawn due to any news regarding the target or market/industry problems ($N$ and $N$, Pure). Cash is expressed as a fraction of the total payment (and hence equal to a dummy for cash in the sample of pure deals in the third and sixth column). Target size is the target's market value of equity in 2010 $bn. Relative deal size is the transaction value over the acquirer's market value of equity. Offer premium is normalized by the target's market capitalization at one month prior to the announcement of the bid, and truncated between 0 and 2. We include indicator variables for whether the bid was hostile or a tender offer. All non-deal-related variables are measured at the end of the year prior to the unsuccessful deal's announcement, and all $q$ variables are winsorized at the 1st and 99th percentiles. Industry fixed effects are based on one-digit SIC codes. A constant term is always included in the absence of fixed effects. Robust standard errors are in parentheses. */**/*** denote significance at the 10%/5%/1% level, respectively.

| | Target CAR (B-25, F+25) | | | | | |
|---|---|---|---|---|---|---|
| Cash | 0.224*** | 0.199** | 0.226** | 0.244*** | 0.195** | 0.224** |
| | (0.07) | (0.09) | (0.11) | (0.07) | (0.08) | (0.10) |
| Log(target size) | | -0.042* | -0.067** | | -0.031 | -0.053* |
| | | (0.02) | (0.03) | | (0.02) | (0.03) |
| Log(relative deal size) | | 0.016 | 0.023 | | 0.013 | 0.020 |
| | | (0.03) | (0.03) | | (0.03) | (0.03) |
| Offer premium | | 0.306*** | 0.297** | | 0.358*** | 0.350*** |
| | | (0.11) | (0.14) | | (0.08) | (0.09) |
| Hostile | | 0.178** | 0.075 | | 0.083 | -0.005 |
| | | (0.08) | (0.11) | | (0.07) | (0.10) |
| Tender offer | | 0.013 | 0.057 | | 0.015 | 0.035 |
| | | (0.10) | (0.13) | | (0.11) | (0.13) |
| $q$ of acquirer | | 0.033* | 0.039* | | 0.016 | 0.021 |
| | | (0.02) | (0.02) | | (0.02) | (0.03) |
| $q$ of target | | -0.011 | 0.013 | | -0.003 | 0.014 |
| | | (0.03) | (0.03) | | (0.04) | (0.04) |
| Industry & year FE | N | Y | Y | N | Y | Y |
| Sample | Main | Main | Main, Pure | $N$ | $N$ | $N$, Pure |
| N | 236 | 236 | 183 | 192 | 192 | 152 |

**Table 5. Frequency of Future Takeovers**

This table reports hazard ratios from Cox proportional-hazard regressions estimating the probability that the target of a failed takeover receives another, successful takeover bid after a failed bid. The sample Main consists of all unsuccessful bids in the *main sample* as defined in Section 2, the sample Main, Pure of all unsuccessful pure-cash and pure-stock bids from the *main sample*. In the third and fifth column, we extend the respective samples to unsuccessful bids by non-public acquirers (Large and Large, Pure). Cash is expressed as a fraction of the total payment (and hence equal to a dummy for cash in the sample of pure deals in the last two columns). Target size is the target's market value of equity in 2010 $bn. Offer premium is normalized by the target's market capitalization at one month prior to the announcement of the bid, and truncated between 0 and 2. We include an indicator variable for whether the bid was hostile or a tender offer. Target CAR (B-25, F+25) is the cumulative abnormal return from 25 days before announcement until 25 days after deal failure. All non-deal-related variables are measured at the end of the year prior to the unsuccessful deal's announcement, and $q$ of target is winsorized at the 1st and 99th percentiles. Industry fixed effects are based on one-digit SIC codes. A constant term is always included in the absence of fixed effects. Robust standard errors are in parentheses. */**/*** denote significance at the 10%/5%/1% level, respectively.

| | Successful takeover bid in future | | | | |
|---|---|---|---|---|---|
| Cash | 1.106 | 1.147 | 0.866 | 1.513 | 1.089 |
| | (0.22) | (0.36) | (0.12) | (0.63) | (0.18) |
| Log(target size) | | 1.058 | 0.977 | 1.194* | 1.009 |
| | | (0.08) | (0.03) | (0.12) | (0.04) |
| Offer premium | | 1.283 | 0.739* | 0.918 | 0.702* |
| | | (0.38) | (0.13) | (0.36) | (0.14) |
| Hostile | | 0.389* | 0.776 | 0.350* | 0.642** |
| | | (0.19) | (0.15) | (0.22) | (0.14) |
| Tender offer | | 1.456 | 1.091 | 1.343 | 1.110 |
| | | (0.60) | (0.23) | (0.69) | (0.25) |
| $q$ of target | | 0.819** | 0.847*** | 0.737*** | 0.857** |
| | | (0.08) | (0.05) | (0.09) | (0.06) |
| Target CAR (B-25, F+25) | | 0.732 | 1.088 | 1.094 | 1.278 |
| | | (0.19) | (0.17) | (0.40) | (0.24) |
| Industry & year FE | N | Y | Y | Y | Y |
| Sample | Main | Main | Large | Main, Pure | Large, Pure |
| N | 236 | 236 | 675 | 183 | 518 |

**Table 6. Value of Future Takeover Bids**

OLS regressions of the value of next takeover bid (log in 2010 $bn) following a failed takeover bid for the same target as the dependent variable. The sample Main* consists of all unsuccessful bids in the *main sample* as defined in Section 2 that were followed by a successful takeover bid for the same target, conditional on the availability of the dollar value of the next offer. The sample Large* also includes non-public acquirers. Cash is expressed as a fraction of the total payment. Previous (contemporaneous) target size is the target's market value of equity in 2010 $bn one month prior to the previous unsuccessful (next) bid's announcement. Offer premium is normalized by the target's market capitalization at one month prior to the announcement of the bid, and truncated between 0 and 2. We include an indicator variable for whether the bid was hostile and for tender offers, and control for the years passed between the two deal announcements under consideration (Years between). The target's $q$ ratio is measured at the end of the year prior to the unsuccessful deal's announcement, and is winsorized at the 1st and 99th percentiles. Industry fixed effects are based on one-digit SIC codes. Robust standard errors are in parentheses. */**/*** denote significance at the 10%/5%/1% level, respectively.

| | Log(next offer value) | | | | | |
|---|---|---|---|---|---|---|
| Cash $\in [0, 1]$ | 0.040 | 0.112 | -0.111 | -0.037 | -0.102 | -0.028 |
| | (0.24) | (0.13) | (0.09) | (0.04) | (0.08) | (0.04) |
| Log(prev. target size) | 0.937*** | 0.921*** | | | 0.072** | 0.062** |
| | (0.07) | (0.04) | | | (0.03) | (0.03) |
| Log(cont. target size) | | | 0.968*** | 0.972*** | 0.916*** | 0.924*** |
| | | | (0.02) | (0.01) | (0.03) | (0.03) |
| Offer premium | 0.675*** | 0.744*** | 0.233*** | 0.027 | 0.272*** | 0.077 |
| | (0.23) | (0.21) | (0.07) | (0.05) | (0.08) | (0.06) |
| Hostile | -0.185 | 0.216 | 0.180* | 0.061 | 0.140 | 0.054 |
| | (0.40) | (0.17) | (0.10) | (0.05) | (0.09) | (0.05) |
| Tender offer | 0.015 | 0.194 | -0.049 | 0.027 | -0.035 | 0.038 |
| | (0.27) | (0.18) | (0.10) | (0.06) | (0.09) | (0.05) |
| $q$ of target | -0.139 | -0.029 | -0.017 | -0.013 | -0.030 | -0.019 |
| | (0.08) | (0.06) | (0.02) | (0.01) | (0.02) | (0.01) |
| Years between | 0.101*** | 0.058*** | -0.008 | -0.000 | -0.004 | 0.001 |
| | (0.03) | (0.02) | (0.01) | (0.00) | (0.01) | (0.00) |
| Industry & year FE | Y | Y | Y | Y | Y | Y |
| Sample | Main* | Large* | Main* | Large* | Main* | Large* |
| N | 99 | 254 | 99 | 254 | 99 | 254 |
| $R^2$ | 0.850 | 0.818 | 0.988 | 0.984 | 0.988 | 0.984 |
| Adjusted $R^2$ | 0.766 | 0.785 | 0.981 | 0.981 | 0.981 | 0.982 |

**Table 7. Post-failure Operational Changes**

Post-failure changes in operational policies are measured along five dimensions: the target's sum of long-term and short-term debt ($D$), number of employees ($Emp$), capital expenditure ($CapEx$), R&D expenses ($R\&D$), and book value of assets ($A$). The respective changes are measured from the calendar year-end before deal announcement to the calendar year-end after deal failure. We regress each operational policy $y$ on cash and the following control variables: offer premium, normalized by the target's market capitalization at one month prior to the announcement of the bid, and truncated between 0 and 2; indicator variables for whether the bid was hostile or a tender offer; $q$ of target, measured at the end of the year prior to the unsuccessful deal's announcement, and winsorized at the 1st and 99th percentiles; and industry and year fixed effects based on one-digit SIC codes (in rows 1 and 2, subsumed by matching in row 3). We only display the coefficient on cash, which is expressed as a fraction of the total payment. In row 1, the dependent variable is the raw difference in log values, $\Delta\log(y)$. In row 2, the dependent variable is the difference in log values scaled by assets, $\Delta\log(y/A)$. In row 3, the dependent variable is the scaled difference in log values of the actual target minus the scaled difference in log values of its matched control firm, $\Delta_{T,C}\Delta\log(y/A) = \Delta\log(y/A) - \Delta\log(y_C/A_C)$. The sample Main* consists of all unsuccessful bids in the *main sample* as defined in Section 2, conditional on data availability, until the calendar year-end after deal failure. The sample Large* adds bids by non-public acquirers. Sample sizes vary depending on the availability of the dependent variable. They are, in order of the columns for the samples Main* and Large*, respectively: 159 and 441; 163 and 438; 177 and 479; 102 and 239; 186 and 497. Sample sizes in the last row further depend on the availability of the respective variables for the matched control firms. Robust standard errors are in parentheses. */**/*** denote significance at the 10%/5%/1% level, respectively.

| | Debt | | Employment | | Capital expenditure | | R&D expenses | | Assets | |
|---|---|---|---|---|---|---|---|---|---|---|
| | $\Delta\log(D)$ | | $\Delta\log(Emp)$ | | $\Delta\log(CapEx)$ | | $\Delta\log(R\&D)$ | | $\Delta\log(A)$ | |
| Cash | -0.295 | -0.113 | 0.058 | -0.062 | -0.219 | -0.081 | -0.036 | -0.024 | -0.094 | -0.075 |
| | (0.28) | (0.15) | (0.09) | (0.07) | (0.21) | (0.14) | (0.16) | (0.12) | (0.12) | (0.07) |
| | $\Delta\log(D/A)$ | | $\Delta\log(Emp/A)$ | | $\Delta\log(CapEx/A)$ | | $\Delta\log(R\&D/A)$ | | | |
| Cash | -0.133 | -0.009 | 0.245*** | 0.040 | -0.091 | 0.009 | 0.140 | -0.021 | | |
| | (0.29) | (0.14) | (0.09) | (0.06) | (0.22) | (0.12) | (0.16) | (0.11) | | |
| | $\Delta_{T,C}\Delta\log(D/A)$ | | $\Delta_{T,C}\Delta\log(Emp/A)$ | | $\Delta_{T,C}\Delta\log(CapEx/A)$ | | $\Delta_{T,C}\Delta\log(R\&D/A)$ | | | |
| Cash | -0.181 | -0.012 | 0.184 | 0.034 | 0.033 | -0.152 | -0.411 | -0.158 | | |
| | (0.31) | (0.17) | (0.13) | (0.09) | (0.28) | (0.15) | (0.29) | (0.17) | | |
| Sample | Main* | Large* | Main* | Large* | Main* | Large* | Main* | Large* | Main* | Large* |

# Online Appendix

to

# Target Revaluation after Failed Takeover Attempts – Cash versus Stock

by Ulrike Malmendier, Marcus M. Opp, and Farzad Saidi

## Appendix B. Long-run returns with forward-looking sample selection

In this section, we explore what one can learn from the analysis of long-run abnormal returns *conditional* on future takeover activities, i.e., when firms are sorted into groups based on takeover bids they receive in the future. In particular, we ask whether conditioning on not receiving any future takeover bid helps to better identify the effect of the (initial) failed bid on the target's stand-alone value. Such conditional long-run analysis mirrors the approach in the seminal paper by Bradley, Desai, and Kim (1983), who analyze targets of failed bids that do not receive a takeover offer in the five years after deal failure.[28]

First, we consider the biases involved in conditioning on the absence of future bids. Long-run returns in such a sample capture the joint effect of the failed takeover attempt and of the absence of future bids. As a result, the long-run returns (starting from before the announcement of the failed bid) are downward-biased estimates of the effect of the failed bid on the target's stand-alone value. This is because the ex-ante market price incorporates that the firm will be taken over with positive probability at some point over the next years, but the sample construction implies zero – i.e., abnormally low – successful takeover activity. This is the relevant bias affecting the analysis of Bradley, Desai, and Kim (1983).

Note that the nature of the bias depends on the goal of the conditional analysis. For example, if the objective was to estimate the effect of the permanent disappearance of a takeover bid – i.e., to estimate the loss of target value as the takeover probability goes from almost 100% to 0% – then identifying a sample in which future takeover activity is eliminated (to 0%) as in Bradley, Desai, and Kim (1983) is warranted.[29] However, such an analysis of long-run returns starting from the post-announcement target value would be affected by a different bias, with the opposite sign, because the post-announcement target value reflects that the probability of success is strictly less than 100%. This is because a rational market will anticipate the possibility of deal failure (over 11% of all deals fail, cf. Panel A of Table 1). As a result, long-run returns conditional on the absence of a future takeover reflect a decrease in takeover probability of less than 100%, implying that these long-run returns are an upward-biased measure of the "permanent" disappearance of the bid.

Returning to the objective of our analysis, i.e., to measure the effect of a failed bid on target valuation, the approximate magnitude of the bias is easy to gauge. As we show in the empirical analysis in the main paper, 20% of matched control firms are taken over within 5 years. Assuming an average takeover premium of 46.2% (cf. Panel A of Table 1), conditioning on the absence of any future bid over the next five years, produces a bias of roughly $46.2\% \cdot 0.2 \approx 9\%$. This ballpark estimate overstates the bias somewhat since it ignores discounting. In what follows, we propose two approaches to assess the

[28] The content of this section heavily benefited from comments by an anonymous referee.

[29] Here, absence of a takeover attempt over a five-year horizon proxies for the permanent absence of future bids.

magnitude of the bias more precisely, including the effect of discounting.

*Theoretical calculation of bias in constant-growth-model.* First, we employ a constant-growth model of stand-alone cash flows with a growth-adjusted discount rate of $\bar{r}$. The (admittedly strong) assumptions of constant growth and an all-equity firm allow us to obtain intuitive expressions. Let $\lambda$ denote the (exponential) arrival rate of takeover attempts and $\pi$ the takeover premium paid over the prevailing market price. Then, the unconditional stock-market value of a firm, $V_U$, is:

$$V_U = \frac{1}{\bar{r} - \lambda\pi}. \tag{B.1}$$

Here, the expected yearly premium $\lambda\pi$ accounts for growth due to future takeover activity. The value of a firm conditional on not being taken over for $T$ years is:

$$V(T) = \left(1 - e^{-\bar{r}T}\right)\frac{1}{\bar{r}} + e^{-\bar{r}T}\frac{1}{\bar{r} - \lambda\pi}. \tag{B.2}$$

Intuitively, this conditional value is a weighted average of the stand-alone value, $\frac{1}{\bar{r}}$, and the unconditional value, $V_U$. The larger $T$ the greater the weight on the stand-alone value. Combining the two equations, the look-ahead bias of conditioning on the absence of takeover activity for $T$ years, $\Delta = \frac{V(T) - V_U}{V_U}$, is given by:

$$\Delta = -\frac{\lambda\pi}{\bar{r}}\left(1 - e^{-\bar{r}T}\right) < 0. \tag{B.3}$$

To obtain an estimate for the magnitude of the bias if one conditions on the absence of takeover activity for $T = 5$ years, we choose $\lambda = 4.1\%$ per year. This is the empirical estimate for matched control firms, i.e., firms that look like actual targets just before the latter receive a (failed) takeover attempt. We choose $\pi = 46.2\%$ and set $\bar{r} = 6\%$. The resulting bias is $-8.2\%$ of firm value, an economically significant look-ahead bias, and similar in magnitude to the ballpark estimate above. We note that the bias is fairly insensitive to the choice of the growth-adjusted discount rate. For $\bar{r} = 4\%$, we obtain an bias of $-8.6\%$, whereas $\bar{r} = 8\%$ produces a bias of $-7.8\%$. Subtracting the (negative) bias allows us, under the modeling assumptions, to retrieve the unbiased estimate.

We note that our actual sample construction in the main paper also implies a small conditioning bias. We require that there be no announcement of another bid by the time of failure. Hence, our sample is restricted to firms that are not taken over for, on average, 60 trading days (the average time from announcement to failure, see Panel B of Table 1). For the conditioning horizon of $T = 60/250$ years, the estimated bias is $-0.45\%$. Hence, this bias is orders of magnitude smaller. Most importantly, this bias should apply equally to cash and stock targets and should, thus, not affect our differential results. We note that restricting ourselves to deals failing in a short amount of time (less than 250 trading days) limits the magnitude of the bias.

*Empirical estimation.* In order to estimate the bias empirically, without relying on functional-form assumptions, we consider two subsamples of firms. Subsample 1 consists of firms receiving a takeover bid that ultimately fails, and that do not receive a bid in the subsequent five years. Subsample 2 consists of matched control firms that do not receive a bid over the same period. The estimates for the second subsample capture the empirical bias inherent in estimating long-run returns for the first subsample. Subtracting the bias from the estimated returns in Subsample 1, we would obtain the unbiased estimate of the (stand-alone) value implications of a takeover bid.

While this approach is conceptually superior to employing the constant-growth model in that it does not rely on functional form assumptions, it is hard to implement in practice. The "ballpark" estimate of an 8% bias over five years suggests that we would need to statistically detect abnormal returns of $-1.6\%$ p.a., which is implausible given stock-market noise. For completeness we implement the approach empirically nevertheless.

For each failed bid in the large sample between January 1980 and December 2003, we determine whether the respective target and its control firm are not taken over within five years. (The sample of failed bids ends in December 2003 due to the forward-looking selection criterion.) For example, Oneida Limited received a takeover bid from Libbey, which failed in July 1999. Since Oneida did not receive a subsequent successful takeover bid until June 2004, it would be considered "not taken over." To calculate the performance of targets that have not been taken over, we rely on the calendar-time-portfolio methods (see Appendix A.2), now applied to this subset of targets. Thus, continuing with our example from above, Oneida is included in the 60-month portfolio between August 1999 and December 2003, its last month as a listed company.

We obtain the following results for abnormal returns:

1. Long-run returns for **actual** targets post failure conditional on not being taken over for 5 years. All alpha estimates are insignificant.
   (a) Cash targets: 0.14% p.a.
   (b) Stock targets: 0.69% p.a.
2. Long-run returns for **matched control** firms conditional on not being taken over for 5 years. All alpha estimates insignificant.
   (a) Matched firms of cash targets: -1.87% p.a.
   (b) Matched firms of stock targets: 2.8% p.a.
   (c) All matched firms (cash + stock + hybrid): -0.57% p.a.

Hence, the analysis of the post-failure returns does not reject the hypothesis that these portfolios have normal returns. We also see that the return estimates are rather noisy. For example, the 95% confidence interval for the yearly alpha of the matched control firms ranges from $-6.67\%$ to 5.54%. We can neither reject the hypothesis that the bias estimate of -0.57% p.a. is equal to the conceptual bias of $-1.6\%$ per year, nor that it is zero. In short, while the sample-selection bias (over 5 years) can be economically meaningful, i.e., around 8% of the equity value, the noise in long-run-returns analyses is orders of magnitude larger.

**Table B.1. Summary Statistics – Successful Bids**

The sample consists of all unsuccessful pure-cash and pure-stock bids from the *main sample*, as described in Section 2. Time to completion is in trading days. Dollar values are based on historical transaction values, converted to 2010 $bn using Consumer Price Index (CPI) Conversion Factors. Target size is the target's market value of equity in 2010 $bn. Relative deal size is the transaction value over the acquirer's market value of equity. Offer premium is normalized by the target's market capitalization at one month prior to the announcement of the bid, and truncated between 0 and 2. Hostile and Tender offer are dummy variables indicating hostile bids and tender offers, respectively. The $q$ ratio is the market value of equity plus assets minus the book value of equity all over assets. Experienced acquirers (dummy variable) have attempted (successfully or not) at least ten acquisitions in the five years up to the year of the takeover bid in question. All non-deal-related variables are measured at the end of the year prior to the deal's announcement, and all $q$ variables are winsorized at the 1st and 99th percentiles. The p-values in the last column are for a two-sided difference-in-means test.

| | Cash Bids | | | | | Stock Bids | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Variable | Mean | Median | Std. dev. | Min | Max | Mean | Median | Std. dev. | Min | Max | p-value |
| Time to completion | 55.72 | 46.00 | 34.62 | 7 | 230 | 84.29 | 74.00 | 38.67 | 25 | 239 | 0.000 |
| Transaction value in 2010 $bn | 0.72 | 0.22 | 1.52 | 0.00 | 17.40 | 1.65 | 0.25 | 5.80 | 0.00 | 70.51 | 0.000 |
| Target size in 2010 $bn | 0.48 | 0.14 | 1.04 | 0.00 | 11.40 | 1.10 | 0.17 | 4.20 | 0.00 | 65.16 | 0.000 |
| Relative deal size | 0.70 | 0.08 | 6.03 | 0.00 | 137.03 | 0.60 | 0.24 | 3.25 | 0.00 | 79.38 | 0.709 |
| Offer premium in % | 47.41 | 40.50 | 35.71 | 0 | 200 | 48.46 | 38.75 | 42.88 | 0 | 200 | 0.634 |
| Hostile | 0.02 | 0.00 | 0.15 | 0 | 1 | 0.01 | 0.00 | 0.08 | 0 | 1 | 0.019 |
| Tender offer | 0.49 | 0.00 | 0.50 | 0 | 1 | 0.03 | 0.00 | 0.17 | 0 | 1 | 0.000 |
| $q$ of acquirer | 2.15 | 1.63 | 1.76 | 0.51 | 15.20 | 3.38 | 2.14 | 3.27 | 0.51 | 15.20 | 0.000 |
| $q$ of target | 1.88 | 1.44 | 1.42 | 0.50 | 9.91 | 2.63 | 1.73 | 2.29 | 0.50 | 9.91 | 0.000 |
| $q$ of acquirer > $q$ of target | 0.60 | 1.00 | 0.49 | 0 | 1 | 0.65 | 1.00 | 0.48 | 0 | 1 | 0.128 |
| Experienced acquirer | 0.25 | 0.00 | 0.43 | 0 | 1 | 0.20 | 0.00 | 0.40 | 0 | 1 | 0.045 |
| % of target sought | 98.39 | 100.00 | 6.87 | 50.20 | 100 | 99.05 | 100.00 | 5.65 | 52.00 | 100 | 0.064 |
| N | | | 639 | | | | | 629 | | | |

**Table B.2. Determinants of Cash Offers**

OLS regressions with the medium of exchange as the dependent variable. The sample Main consists of all unsuccessful bids in the *main sample* as defined in Section 2, the sample Main, Pure of all unsuccessful pure-cash and pure-stock bids from the *main sample*. Cash is expressed as a fraction of the total payment (and hence equal to a dummy for cash in the sample of pure deals in the last three columns). Target size is the target's market value of equity in 2010 $bn. Relative deal size is the transaction value over the acquirer's market value of equity. Offer premium is normalized by the target's market capitalization at one month prior to the announcement of the bid, and truncated between 0 and 2. We include indicator variables for whether the bid was hostile or a tender offer. Experienced acquirers (dummy variable) have attempted (successfully or not) at least ten acquisitions in the five years up to the year of the takeover bid in question. All non-deal-related variables are measured at the end of the year prior to the unsuccessful deal's announcement, and all $q$ variables are winsorized at the 1st and 99th percentiles. Industry fixed effects are based on one-digit SIC codes. Robust standard errors are in parentheses. */**/*** denote significance at the 10%/5%/1% level, respectively.

| | Cash $\in [0,1]$ | | | Cash $\in \{0,1\}$ | | |
|---|---|---|---|---|---|---|
| Log(target size) | | 0.005 | -0.006 | | 0.009 | -0.009 |
| | | (0.01) | (0.01) | | (0.01) | (0.01) |
| Log(rel. deal size) | -0.056*** | -0.065*** | -0.051*** | -0.070*** | -0.079*** | -0.061*** |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| Offer premium | | | 0.009 | | | -0.025 |
| | | | (0.02) | | | (0.03) |
| Hostile | | | 0.141** | | | 0.183** |
| | | | (0.06) | | | (0.07) |
| Tender offer | | | 0.474*** | | | 0.529*** |
| | | | (0.02) | | | (0.02) |
| $q$ of acquirer | | -0.030*** | -0.022*** | | -0.030*** | -0.022*** |
| | | (0.00) | (0.00) | | (0.00) | (0.00) |
| $q$ of target | | -0.033*** | -0.022*** | | -0.039*** | -0.024*** |
| | | (0.01) | (0.00) | | (0.01) | (0.01) |
| Exp. acquirer | 0.000 | -0.015 | -0.006 | -0.018 | -0.036 | -0.019 |
| | (0.02) | (0.02) | (0.02) | (0.03) | (0.03) | (0.03) |
| % of target sought | -0.005*** | -0.004*** | -0.004** | -0.005*** | -0.005** | -0.005** |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| Industry & year FE | Y | Y | Y | Y | Y | Y |
| Sample | Main | Main | Main | Main, Pure | Main, Pure | Main, Pure |
| | 2,082 | 2,082 | 2,082 | 1,451 | 1,451 | 1,451 |

**Table B.3. Cash vs. Stock Revaluation Differences in a Controlled Regression Framework – Large Sample**

OLS regressions with target CAR from 25 days before announcement to 25 days after deal failure as the dependent variable. The sample Large consists of all unsuccessful bids, including those by non-public acquirers, in the *large sample* as defined in Section 2; the sample Large, Pure of all unsuccessful pure-cash and pure-stock bids, including those by non-public acquirers, from the *large sample*. Cash is expressed as a fraction of the total payment (and hence equal to a dummy for cash in the sample of pure deals in the last two columns). Target size is the target's market value of equity in 2010 $bn. Offer premium is normalized by the target's market capitalization at one month prior to the announcement of the bid, and truncated between 0 and 2. We include indicator variables for whether the bid was hostile or a tender offer. All non-deal-related variables are measured at the end of the year prior to the unsuccessful deal's announcement, and all $q$ variables are winsorized at the 1st and 99th percentiles. Industry fixed effects are based on one-digit SIC codes. A constant term is always included in the absence of fixed effects. Robust standard errors are in parentheses. */**/*** denote significance at the 10%/5%/1% level, respectively.

| | Target CAR (B-25, F+25) | | | |
|---|---|---|---|---|
| Cash | 0.108** | 0.077* | 0.144*** | 0.114** |
| | (0.04) | (0.04) | (0.05) | (0.05) |
| Log(target size) | | -0.030** | | -0.037** |
| | | (0.01) | | (0.01) |
| Offer premium | | 0.299*** | | 0.306*** |
| | | (0.06) | | (0.07) |
| Hostile | | 0.123*** | | 0.121*** |
| | | (0.04) | | (0.04) |
| Tender offer | | 0.146*** | | 0.188*** |
| | | (0.05) | | (0.06) |
| $q$ of target | | -0.010 | | 0.013 |
| | | (0.02) | | (0.02) |
| Industry & year FE | N | Y | N | Y |
| Sample | Large | Large | Large, Pure | Large, Pure |
| N | 675 | 675 | 518 | 518 |

**Table B.4. Returning Bidders – Summary**

This table summarizes facts about returning bidders after initial failed bids for the same target (but before the end of the data set in 2008). We consider the *main sample* in the first two columns, and extend to the *large sample*, including bids by non-public acquirers, in the last two columns. Time to next bid is in calendar days.

| | Main sample | | Large sample | |
|---|---|---|---|---|
| Number of observations | 236 | | 675 | |
| Received subsequent bid | 134 | | 409 | |
| | Successful | Failed | Successful | Failed |
| | 101 | 33 | 262 | 147 |
| Subsequent bids by returning bidders | 15 | 4 | 40 | 28 |
| Average time to next bid by returning bidder | 539.7 | 604.5 | 581.7 | 688.1 |

**Table B.5. Value of Future Takeover Bid – Pure Deals**

OLS regressions of the value of next takeover bid (log in 2010 $bn) following a failed takeover bid for the same target as the dependent variable. The sample Main*, Pure consists of all unsuccessful pure-cash and pure-stock bids from the *main sample* (as defined in Section 2) that were followed by a successful takeover bid for the same target, conditional on the availability of the dollar value of the next offer. The sample Large*, Pure also includes non-public acquirers. Cash is equal to a dummy indicating 100% cash, rather than 100% stock, as the method of payment. Previous (contemporaneous) target size is the target's market value of equity in 2010 $bn one month prior to the previous unsuccessful (next) bid's announcement. Offer premium is normalized by the target's market capitalization at one month prior to the announcement of the bid, and truncated between 0 and 2. We include an indicator variable for whether the bid was hostile and for tender offers, and control for the years passed between the two deal announcements under consideration (Years between). The target's $q$ ratio is measured at the end of the year prior to the unsuccessful deal's announcement, and is winsorized at the 1st and 99th percentiles. Industry fixed effects are based on one-digit SIC codes. Robust standard errors are in parentheses. */**/*** denote significance at the 10%/5%/1% level, respectively.

| | Log(next offer value) | | | | | |
|---|---|---|---|---|---|---|
| Cash $\in [0, 1]$ | 0.077 | 0.134 | -0.039 | -0.041 | -0.019 | -0.028 |
| | (0.27) | (0.15) | (0.10) | (0.06) | (0.08) | (0.05) |
| Log(prev. target size) | 1.059*** | 0.929*** | | | 0.197*** | 0.111*** |
| | (0.10) | (0.05) | | | (0.04) | (0.03) |
| Log(cont. target size) | | | 0.968*** | 0.977*** | 0.842*** | 0.890*** |
| | | | (0.03) | (0.02) | (0.04) | (0.03) |
| Offer premium | 0.869** | 0.734*** | 0.271** | -0.000 | 0.388*** | 0.094 |
| | (0.35) | (0.19) | (0.12) | (0.07) | (0.10) | (0.08) |
| Hostile | -0.874 | 0.378** | 0.109 | 0.071 | -0.154 | 0.076 |
| | (0.57) | (0.18) | (0.15) | (0.06) | (0.12) | (0.05) |
| Tender offer | 0.018 | 0.073 | -0.096 | 0.036 | -0.058 | 0.039 |
| | (0.32) | (0.19) | (0.12) | (0.07) | (0.08) | (0.06) |
| $q$ of target | -0.240* | -0.041 | -0.009 | -0.021 | -0.069 | -0.035* |
| | (0.12) | (0.07) | (0.04) | (0.02) | (0.04) | (0.02) |
| Years between | 0.060* | 0.033* | -0.017 | -0.000 | -0.007 | 0.000 |
| | (0.03) | (0.02) | (0.01) | (0.01) | (0.01) | (0.01) |
| Industry & year FE | Y | Y | Y | Y | Y | Y |
| Sample | Main*, Pure | Large*, Pure | Main*, Pure | Large*, Pure | Main*, Pure | Large*, Pure |
| N | 73 | 185 | 73 | 185 | 73 | 185 |
| $R^2$ | 0.867 | 0.852 | 0.984 | 0.981 | 0.988 | 0.983 |
| Adjusted $R^2$ | 0.749 | 0.816 | 0.969 | 0.977 | 0.976 | 0.979 |