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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 we 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 eight years than matched control firms, no differences exist between cash and stock targets, either in the timing or 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.

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 policy makers and academic researchers alike. One set of stylized facts that 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

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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 could 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 could 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 nonexclusive 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, independent 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 US 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] After an initial announcement effect (including the 25 trading-day run-up) of +25% for cash targets and +15% for stock targets, the value of the average cash target remains at +15% 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).[3] 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.

Because 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. 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. 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 with 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 with a sample of matched control firms. For example, after five years, 50% of the targets with failed bids have received a successful bid, compared with 20% in the control group. The abnormally high subsequent takeover activity persists until eight years 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.

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

[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 they find that unsuccessful stock acquirers perform worse.

[3] The announcement effect estimates are almost identical to those found by Huang and Walkling (1987) in their sample 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 similar ex ante.

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, finds 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 explained neither by future takeovers nor by common proxies of operational change. Our evidence is thus 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) find a large revaluation of targets after failed tender offers. Dodd (1980) compares revaluations of target firms that vetoed the bid with those that did not. He finds that only target firms with management that 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 with 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 one month prior to announcement of the original bid until five 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; 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 downward. The magnitude of this bias is economically significant. We show that matched control firms are taken over about 20% of the time in a span of five years. This amount of future takeover activity should also be priced in the stock market valuation of actual targets prior to the announcement of the (initial) takeover offer. Conditioning on the absence of takeover activity for five 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 $46.2\% \cdot 0.2 \approx 9\%$. Because 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 might 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, as 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. Because 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 five-year horizon. Concretely, in the sample of Bradley, Desai, and Kim (1983), the standard error of the CAR estimate for the window from one month before the bid until 60 months after the bid is economically large at 15.37%.[4] 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.[5]

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, because 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.

The results in our paper are consistent with earlier evidence by Sullivan, Jensen, and Hudson (1994).[6] 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

[4] 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 one month before the bid until 60 months after the bid is $\sqrt{62} \cdot 1.952\% = 15.37\%$.

[5] 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.

[6] 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.

Table 1
Summary statistics.

The table reports statistics for the main sample, as described in Section 2. *Time to completion/failure* is measured in trading days. *Transaction value* is converted to billions of 2010 dollars using Consumer Price Index (CPI) conversion factors. *Target size* is the target's market value of equity in billions of 2010 dollars. *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 zero and two. *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 percent	45.93	37.16	45.47	0	100	44.03	19.19	45.71	0	100	0.546
Stock in percent	46.48	43.06	45.39	0	100	55.49	75.18	45.79	0	100	0.004
Other payment in percent	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 percent	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
Percent 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 percent	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
Percent 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			

takeovers. They find that winners of (long-duration) bidding contests under-perform 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 US between 1980 and 2008 from the Securities Data Company (SDC) Mergers and Acquisitions database, and we merge the data with stock market and accounting data from the Center for Research in Security Prices (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 five to 250 trading days after the announcement.[7] Second, we exclude bids with

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

competing offers, i.e., offers that are announced before failure of the initial bid, to avoid capturing returns to the competing offer.[8] 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.[9] 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 we predict out-of-sample premia based on transaction value and market capitalization (when available). Following Officer (2003), we truncate deal premia below zero 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 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 809 deals.[10] 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, which 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 restriction to public-to-public transactions 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.[11]

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, a marginally significant difference exists 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 or 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.[12] 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.

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. “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.[13] “Target news (private)” refers to failed deals in which 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 US Department of Justice's view. “Management terms” refers to cases in which target management and acquirer management could not agree on organizational issues, such as the nomination of a Chief

[8] 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.

[9] 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 seventh digit (typically 1).

[10] We thank Harry DeAngelo, our referee, for pointing this out. All results are robust to including LBOs.

[11] See, in particular, Appendix Table B3.

[12] For completeness, we also show the characteristics of successful pure deals in Appendix Table B1.

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

Table 2
Failure categories.

The category "Price too low" denotes failed deals in which the parties could not agree on the transaction price. "Management rejection" refers to deals that failed because the management or the board, or both, refused the bid. "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 news (private)" to failed deals in which the acquirer discovered bad information in the due diligence process. "Market problems" denotes deal failure due to shifting market conditions (typically stock market plunges), and "Industry problems" are pertinent to the target's or the acquirer's industry, or both. "Regulator" refers to deal failure in which the news search revealed lack of regulatory approval. "Management terms" describes all failed deals in which acquirer and target were unable to agree on terms other than the price (e.g., the nomination of a Chief Executive Officer 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 or 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 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 cumulative abnormal return from 25 days before announcement to 25 days after deal failure on the fraction offered in cash and a constant. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Failure reason	Average % cash	Cash coefficient		*N*
		Target	Acquirer	
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 news (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
Bids with failure reason	42.4%	0.166*	0.231***	150
Bids in sample *C*	39.3%	0.192**	0.347***	81

Executive Officer (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 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 we had no information beyond which party canceled the takeover for another 51 deals.

We 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 news (private)," "Market problems," and "Industry problems." Second, we consider a more conservative sample *C*, which contains only those deals for which we identify 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 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

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

where r_{ij} and r_{mj} denote firm *i*'s equity return and the CRSP value-weighted market return at time *j*, respectively.[14] 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.[15]

Univariate results: Fig. 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

[14] 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 because debt is less sensitive to information.

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

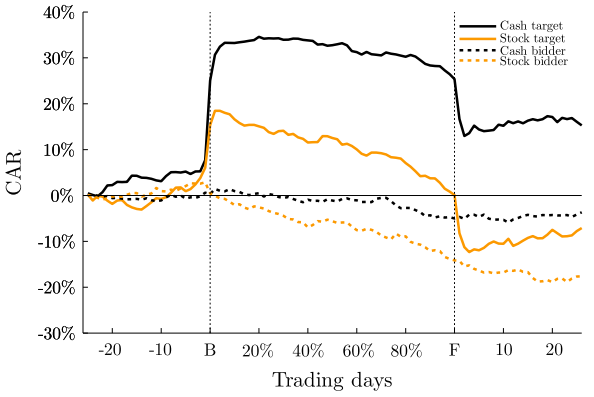

Fig. 1. Announcement effects at bid and at failure (± 25 days). The figure depicts cumulative abnormal returns (CARs) from 25 trading days before announcement of the initial bid (B) to 25 trading days after deal failure (F). The sample consists of 81 pure-cash and 102 pure-stock deals (see summary statistics in Panel B of Table 1).

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. For the illustration of the intermediate period, we normalize trading days (in percent) because the time from bid announcement to failure differs across deals. We linearly interpolate between trading days if needed [see Appendix Section A.1 for details]. For example, 50% refers to trading day 50 if a bid fails after one hundred trading days, but it refers to trading day 20 if a bid fails after 40 trading days. 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 include 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 Eq. (1). Instead, we adopt the calendar-time portfolio approach advocated by Fama (1998) (see also earlier work by Jaffe, 1974; 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 Capital Asset Pricing Model (CAPM) as the underlying asset pricing model.[16] Because the number of firms in the portfolios is time-varying, we efficiently estimate the coefficients by weighted least squares (see Appendix Section A.2 for

Table 3
Long-term persistence of cash versus stock revaluation differences.

Capital Asset Pricing Model calendar-time portfolio estimates of alpha (in percent per month) are based on weighted least squares regressions of the monthly portfolio excess return relative to the one-month Treasury rate (as the dependent variable) on the monthly market excess return. For the estimates of the long-short portfolio, the dependent variable is the excess return of the cash portfolio over the stock portfolio. 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 one to five (across rows). We use the main sample in the first three columns and the large sample with (also) targets of nonpublic acquirers in the last three columns. N is the number of months with nonempty portfolios. Observations are weighted as explained in Appendix Section A.2. Robust standard errors are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Main sample			Large sample		
Alpha	Cash	Stock	Long–short	Cash	Stock	Long–short
One 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
Two 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
Three 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
Four 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
Five 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

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

Table 4
Cash versus stock revaluation differences in a controlled regression framework.

The table reports ordinary least squares regressions with target cumulative abnormal return (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, and the sample "Main, Pure" consists 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 or 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 billions of 2010 dollars. *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 zero and two. 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 Standard Industrial Classification codes. A constant term is always included in the absence of fixed effects. Robust standard errors are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Variable	Target CAR ($B-25$, $F+25$)					
	(1)	(2)	(3)	(4)	(5)	(6)
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 and year fixed effects	No	Yes	Yes	No	Yes	Yes
Sample	Main	Main	Main, Pure	*N*	*N*	*N*, Pure
N	236	236	183	192	192	152

details). 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, as a monthly observation is included only if both the cash and the stock portfolio are nonempty. 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 could 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).[17] Including such controls is important because Jensen and Ruback (1983) find that only targets in unsuccessful tender offers are positively revalued. 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.[18]

[17] See Appendix Table B2 for an analysis of the correlates of the medium of exchange.

[18] 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

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 nonpublic acquirers (see Appendix Table B3). 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 could 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, revaluation differences between cash and stock targets still could be driven by differential sorting of cash and stock deals into failure. For example, good news about the target could 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 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.[19] However, even if this specific concern 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 are revalued by 30%, and failure reason *B* is associated with targets that are 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 one 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 could be driven by specific deal-failure categories, we make use of our hand-collected sample of failure reasons. We reestimate 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 reestimate 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. 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 reestimate 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," which reflects that revaluation estimates for individual companies during market crashes such as September 11 or October 1987 are extremely volatile, even after adjusting for market returns.

Subject to the caveat that we can address selection based only 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 could induce the target management to make operational improvements (see, e.g., Safieddine and Titman, 1999). For our purposes, the relevant issue 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

(*footnote continued*)
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; Jennings, LeClere, and Thompson, 2001), possibly tilting the medium of exchange toward 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.

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

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:

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

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 dollars), book value of total assets (in 2010 dollars), 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 Standard Industrial Classification (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.[20] Future successful bids include returning bidders of an earlier failed deal, although the vast majority are made by other bidders.[21] For control firms, we measure the time to arrival of a successful takeover bid starting from deal failure of the matched sample firm.

Fig. 2 plots the corresponding Kaplan-Meier graphs of future takeovers for sample firms and matched control firms. Because 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 nonpublic acquirers. We eliminate eight observations for which 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 eight deals. The graphs also look the same if we use our main, instead of the large, sample.)

Fig. 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 eight years post-failure. After eight 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.

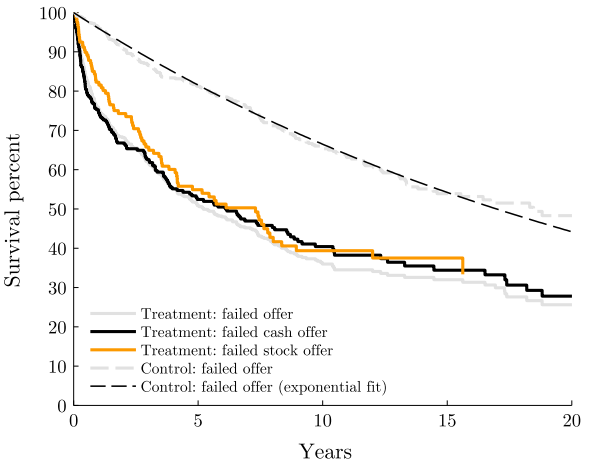

Fig. 2. Kaplan-Meier survival estimates. This graph plots the arrival rates of takeover bids for sample firms and matched control firms over 20 years after an unsuccessful takeover bid (adjusted for bankruptcy-induced censoring). For each year, the base is the set of surviving firms, and the event is 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%.

Finally, and of greatest importance to our analysis, Fig. 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 one 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.

[20] 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 noncompliance with rules of) float or assets; company request, liquidation; bankruptcy, declared insolvent; delinquent in filing; nonpayment 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.

[21] In Appendix Table B4, we summarize information on follow-up bids by the same bidder. Successful takeovers by returning bidders occur on average one and a half years after the original failed bid. Future unsuccessful bids occur about one and two-thirds years after the original bid.

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, and the sample "Main, Pure" consists 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 nonpublic 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 billions of 2010 dollars. *Offer premium* is normalized by the target's market capitalization at one month prior to the announcement of the bid and truncated between zero and two. We include an indicator variable for whether the bid was *hostile* or a *tender offer*. *Target CAR* 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 Standard Industrial Classification codes. A constant term is always included in the absence of fixed effects. Robust standard errors are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Variable	Successful takeover bid in future				
	(1)	(2)	(3)	(4)	(5)
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		0.732	1.088	1.094	1.278
		(0.19)	(0.17)	(0.40)	(0.24)
Industry and year fixed effects	No	Yes	Yes	Yes	Yes
Sample	Main	Main	Large	Main, Pure	Large, Pure
N	236	236	675	183	518

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 could 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.[22] 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.[23] 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. By estimating the discount rate, we do not have to impose an appropriate discount rate on our own. The size of the estimated coefficients is economically sensible.

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), 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, research and development (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

[22] See Appendix Table B5 for the same analysis in the subsample of pure deals.

[23] The much larger magnitude of the contemporaneous-size coefficient, compared with 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).

Table 6
Value of future takeover bids.

The table reports ordinary least squares regressions of the value of next takeover bid (log in billions of 2010 dollars) 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 nonpublic acquirers. *Cash* is expressed as a fraction of the total payment. *Previous* (*contemporaneous*) *target size* is the target's market value of equity in billions of 2010 dollars 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 zero and two. We include an indicator variable for whether the bid was *hostile* or a *tender offer*, and we 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 Standard Industrial Classification codes. Robust standard errors are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Log(next offer value)					
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Cash ∈ [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 and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
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

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.[24] 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.

Table 7 presents our analysis of operational changes along these dimensions for raw changes (Panel A), changes scaled by assets (Panel B), and scaled changes relative to the respective matched control firm (Panel C). For both the main and the large sample, the regressions control for our usual set of firm and deal variables (offer premium, indicator variables for hostility and tender offer, and q of target). We do not include industry and year fixed effects when we 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 relevant 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 sensible.

In 25 of our 26 regression specifications, the cash coefficient is insignificant, suggesting that no meaningful differences exist 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 robust to the other two definitions of the outcome variable.

In unreported regressions, we also analyze CEO turnover as a possible catalyst outcome. For example, Mikkelson and Partch (1997) provide evidence of a positive relation 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 relation only within the (smaller) set of targets that is covered by the ExecuComp database, which starts 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 might 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

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

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 ($CapEx$), research and development 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 firm and deal control variables: offer premium, normalized by the target's market capitalization at one month prior to the announcement of the bid, and truncated between zero and two; 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. In addition, we control for industry and year fixed effects based on one-digit Standard Industrial Classification codes (in Panel A and Panel B). We only display the coefficient on *Cash*, which is expressed as a fraction of the total payment. In Panel A, the dependent variable is the raw difference in log values, $\Delta \log(y)$. In Panel B, the dependent variable is the difference in log values scaled by assets, $\Delta \log(y/A)$. In Panel C, 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 nonpublic 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, and 186 and 497. Sample sizes in Panel C further depend on the availability of the respective variables for the matched control firms. Robust standard errors are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Raw changes

Variable	Debt $\Delta \log(D)$		Employment $\Delta \log(Emp)$		Capital expenditure $\Delta \log(CapEx)$		R&D expenses $\Delta \log(R\&D)$		Assets $\Delta \log(A)$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
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)
Firm and deal controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Main*	Large*	Main*	Large*	Main*	Large*	Main*	Large*	Main*	Large*

Panel B: Changes scaled by assets (A)

Variable	Debt $\Delta \log(D/A)$		Employment $\Delta \log(Emp/A)$		Capital expenditure $\Delta \log(CapEx/A)$		R&D expenses $\Delta \log(R\&D/A)$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
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)
Firm and deal controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Main*	Large*	Main*	Large*	Main*	Large*	Main*	Large*

Panel C: Changes scaled by assets (A) relative to matched control firm

Variable	Debt $\Delta_{T,C}\Delta \log(D/A)$		Employment $\Delta_{T,C}\Delta \log(Emp/A)$		Capital expenditure $\Delta_{T,C}\Delta \log(CapEx/A)$		R&D expenses $\Delta_{T,C}\Delta \log(R\&D/A)$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
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)
Firm and deal controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry and year fixed effects	No	No	No	No	No	No	No	No
Sample	Main*	Large*	Main*	Large*	Main*	Large*	Main*	Large*

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 eight 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. 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.,

$$\widehat{CAR}_i(t_R) = (1 - w_{(i,t_r)})CAR_i(\lfloor t_R T_i \rfloor) + w_{(i,t_r)}CAR_i(\lfloor t_R T_i \rfloor + 1), \quad (3)$$

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:

$$R_{p,t} - R_{f,t} = \alpha_p + \beta_p(R_{m,t} - R_{f,t}) + \varepsilon_{p,t}, \quad (4)$$

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 α_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

$$Var(\varepsilon_{p,t}) = Var\left(\sum \frac{1}{N_{p,t}}\varepsilon_{i,t}\right) = \frac{\overline{Var(\varepsilon_i)}}{N_{p,t}} + \frac{N_{p,t}-1}{N_{p,t}}\bar{\rho}\,\overline{Var(\varepsilon_i)}, \quad (5)$$

where $\overline{Var(\varepsilon_i)}$ is the average residual variance of all stocks and $\bar{\rho}$ is defined such that $\bar{\rho}\overline{Var(\varepsilon_i)}$ 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.) Because the most important change from month to month concerns the number of firms, $N_{p,t}$, variations in $\overline{Var(\varepsilon_i)}$ 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

$$Var(\varepsilon_{C,t} - \varepsilon_{S,t}) = Var(\varepsilon_{C,t}) + Var(\varepsilon_{S,t}) - 2Cov(\varepsilon_{C,t}, \varepsilon_{S,t}) = \overline{Var(\varepsilon_i)}(1-\bar{\rho})\left(\frac{1}{N_{C,t}} + \frac{1}{N_{S,t}}\right). \quad (6)$$

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 $\frac{1}{N_{C,t}} + \frac{1}{N_{S,t}}$.

Appendix B. Supplementary data

Supplementary data associated with this article can be found in the online version at http://dx.doi.org/10.1016/j.jfineco.2015.08.013.

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