Does Takeover Increase Stockholder Value?

Kewei Hou[1], Per Olsson[2], David Robinson[3]

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[1]Doctoral Candidate in Finance, The University of Chicago Graduate School of Business. email: kewei.hou@gsb.uchicago.edu, phone: (847) 425-1945

[2]Assistant Professor of Accounting, University of Wisconsin at Madison. email: polsson@bus.wisc.edu.

[3](contact author) Doctoral Candidate in Finance, The University of Chicago Graduate School of Business. email: pdrobins@gsbphd.uchicago.edu, phone: (773) 702-7420. We would like to thank Gregor Andrade, Steve Kaplan, Mark Mitchell, Raghuram Rajan, Erik Stafford, Per Strömberg, Andy Wong, and Luigi Zingales for helpful comments. Any errors are our own.

Abstract

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Yes. We modify the calendar-time portfolio regressions (CTPRs) approach to measure the abnormal returns of a takeover portfolio composed exclusively of successful bidders and targets from 1963 to 1995. This technique balances the positive announcement-period stock price effects against the alleged post-announcement drift that is commonly thought to accompany takeovers. By regressing the takeover portfolio returns on asset-pricing factors with GARCH(1,1) error specification, our methodology overcomes a number of limitations that would otherwise confound this approach. Studying 3,467 successful takeover events, we find that value weighted portfolios earn a highly significant 72 basis points a month in abnormal returns. Equally weighted results are even more dramatic. We extend the analysis to study mergers within and across industry boundaries. Using method of payment as a proxy for pooling versus purchase accounting, we also examine the impact of accounting choice on performance.

JEL Classification Codes: *G00, G30, G34*

1 Introduction

This paper asks whether successful mergers and acquisitions increase the *overall* value of the publicly-traded sector of our economy. This question has long been a central focus of financial economics: Jensen [23], arguing the disciplining role of the takeover market, provides a forceful case in favor of mergers whereas others, perhaps most notably Roll [42], argue that empire-building and managerial hubris may drive many takeovers, and that the institution of takeover therefore may not be a net value creator to the economy.

A vast empirical literature has sought to address various aspects of the larger question we address here. This literature includes event studies of the stock price effects on bidders and targets in the period immediately surrounding the takeover, studies of the post-acquisition stock returns of the acquiror, studies of the returns to a target stockholder who decides to stick with the merged company, and studies of accounting performance measures in the merged company. No empirical study to date has, however, tried to answer the overall question about the institution of takeover: whether it increases or destroys value overall. Furthermore, while the existing literature agrees on the market effects immediately surrounding the takeover, the results regarding what happens in the post-merger period are markedly different. This makes it impossible to answer our overall question based on extant evidence, since the alleged post-merger drift not only speaks to value erosion, but also

indicts the efficient markets hypothesis on which all stock-priced value measures are based.

Using a measure of value creation based on stock market performance, we make several contributions to the literature. First, our sample is exhaustive. We investigate all successful mergers between listed US firms over a thirty-year period. One reason for the different post-merger performance results in prior studies may be that the samples are vastly different. Indeed, we show that results based on samples restricted to a particular decade may distort the overall picture.

Second, we measure the *total* value implications of each takeover. Specifically, we use calendar-time portfolio regressions (CTPRs) to measure the abnormal returns of a *takeover portfolio* containing acquirors, targets, and the firms emerging from the merger. But we augment the standard CTPR approach, allowing firms to enter *before* the event date. This allows us to include effects occurring both around the announcement and event dates as well as any potential long-term effects (we allow for portfolio horizons of up to three years following the takeover). By using a value-weighted portfolio of all takeovers spanning a thirty-year period we are able to capture the total wealth creation (or destruction) brought about by takeovers.

Third, we make methodological improvements to the CTPR approach. Since different numbers of firms enter and exit the portfolio over time, and since the characteristics of firms may change discretely around the event date, time-variation in the residual volatility of our portfolio is a central issue that

could, if incorrectly accounted for, lead to biased inferences. We employ a generalized, autoregressive, conditional heteroscedasticity (GARCH) estimation approach to allow for changes in the portfolio's composition to affect the conditional volatility of the portfolio returns. As a result, we are able to make unbiased inferences about the sum of short-term and long-term value implications of takeover. Finally, we have several sub-analyses that allows us to comment on, and in most cases reconcile conflicting results from prior literature.

Of course, we have to guard against the obvious criticism that markets are inefficient, and thus we draw false conclusions from stock price movement. Since we investigate increasing measurement horizons in our portfolio approach, we are able to investigate how long it takes the market to judge the success of takeovers, thus accounting for the fact that markets may react slowly. Our longest horizon is three years after the takeover event, so our design allows for a very long price adjustment period. Moreover, with our GARCH-based tests, we are able to replicate other findings in the literature (see, for instance, Mitchell and Stafford [37]) showing that post-merger drift may be an artifact of biased statistical inference. Hence, unless one believes in constant and continuous market inefficiency, our design should be robust to such concerns.

Our study is still limited in the sense that it only captures value creation (or destruction) that arises as a direct consequence of *actual* takeover. As argued by Jensen [23], the implicit threat of potential takeovers has a

disciplining role on managers, possibly forcing them to follow shareholder value-maximizing strategies. Since we make no effort to measure the stock price reactions to transient, unrealized takeover threats, our results may underestimate the value of takeovers to society.

Under this approach, which blends the pre-announcement drift with any long-horizon price reversion, we find that takeover increases shareholder value. Moreover, our results show that ignoring time varying volatility in our portfolio would bias our point estimates—likelihood ratio tests *strongly* reject the null that OLS is correctly specified. Extending this methodology, we show that there is, in fact, little difference between horizontal and vertical mergers, and that cash offers *dramatically* outperform stock offers. To summarize, our evidence favors Jensen [23] over Roll's Hubris Hypothesis [42], or Morck, Shleifer, and Vishny [38].

The remainder of the paper is organized as follows. First, we briefly review the existing empirical evidence and discuss our motivation. This is presented in section 2. Section 3 details the research design we employ and compares it to popular alternatives. Section 4 describes the sample. Our main results are presented in section 5. One of the advantages of our methodology is that it can be easily modified to study different merger characteristics; in section 6, we examine the value implications of within- and across-industry mergers, and how differences in the mode of payment (and the accounting rules that underlie this choice) affect stockholder value. Section 7 concludes.

2 Motivation

As mentioned above, one motivation for our analysis grows out of the conflicting value implications of short-horizon and long-horizon studies. Short-horizon studies indicate strong overall stock price gains to successful takeovers. On the other hand, some long-horizon studies demonstrate dramatic negative stock price drift after the completion of the merger. The net effect is unclear (and even more unclear when considering that some long-term studies do not find any negative drift). Moreover, since short-horizon studies and long-horizon studies almost always use different research designs, no clear conclusion can be reached by simply adding the positive effect of pre-event studies and the negative effect of (some) post-effect studies.

To put this in a better perspective, consider the life cycle of a typical successful takeover. Prior to the takeover, a typical target has experienced a long price decline that typically reverts about one month prior to the takeover announcement (Asquith [4]). The bidder, meanwhile, exhibits modest price increases. Asquith [4] documents a clear pattern of positive stock price performance for takeover targets around the announcement date; target firms earn an average of 6% return over the three days surrounding a takeover announcement. Acquirors involved in successful takeovers show little price reaction at this time. In the interim period between announcement and execution of the takeover, both the target and bidder of a successful takeover show relatively little price movement. Thus, if the story ended on the day of

the takeover event, we would already be sure that takeover increases overall stockholder value. Indeed, Jensen and Ruback [24] summarize results that show successful takeovers generate roughly 18% return in the period starting ten days prior to the announcement and ending ten days after the execution.

The problem is that the story does not end here. It begins here. Post-acquisition studies provide conflicting evidence for the long-run effects of takeover. The results of studies summarized in Jensen and Ruback [24] point towards a downward drift in the first year after takeover, although the negative abnormal return is statistically significant in only three of the seven cited investigations. Franks, Harris, and Titman [18] find no evidence of negative abnormal returns.[1] Agrawal, Jaffe, and Mandelker [1] report an abnormal return of -10% for the five-year period after the completion of the acquisition using a sample of 765 mergers between 1955 and 1987. Likewise, Rau and Vermaelen [40] document underperformance when investigating a sample of 3,169 mergers three years after the event. But for a sample of 348 tender offers they find the opposite: overperformance. The theory also provides different predictions. Manne [33] discusses the importance of the market for corporate control, and the question of whether inefficiently run companies will (or should) be taken over has been a point of debate ever since. Jensen [23] has argued that takeover is a critical corporate governance mechanism required to ensure that modern managers do not waste resources as they con-

[1] They use a three-year horizon after the acquisition for a sample of 399 takeovers between 1975 and 1984.

tinue to try to expand their sphere of control in ever-shrinking industries. In contrast, Roll [42] has argued that the efficiency gains to takeover are zero, since they are motivated by managerial hubris.[2]

One attempt to resolve this conflict can be found in the strand of literature that uses information revealed between the initial announcement and the ultimate takeover to draw inferences about would-be value creation or destruction. Baghat and Hirshleifer [6] deem this the *intervention* approach, since it is based on using the market's assessment of takeover success probabilities based on intervening information. Schurman [43] applies this approach to anti-trust intervention to determine whether synergies existed in deals forbidden by the SEC. However, Hietala, Kaplan, and Robinson [20] show that these inferences are only appropriate under special conditions relating the number of bidders and targets to the number of unknowns that must be extracted from stock prices.

A number of avenues of research point to long-term stock price movement as the most natural indicator of the overall efficiency of takeover. In part, this stems from the fact that post-acquisition studies using accounting-based performance measures provide inconclusive answers to the question of takeover's efficiency. Ravenscraft and Scherer [41] investigate a sample of 95 takeovers and find no indication of increased operating profitability in the segments of the merged firms they identify as stemming from the acquired

[2]This argument builds on Jensen [22]. Empirical evidence related to this can be found in Mitchell and Lehn [35].

firms. On the other hand, Healy, Palepu, and Ruback [19] report significant improvements in asset productivity (industry-adjusted) in the post-merger period for their sample of 50 large mergers.

But accounting-based performance measures may also indicate improved efficiency when no efficiency gains have been realized: Kaplan, Mitchell, and Wruck [27] use internally generated performance data in in-depth clinical studies of two acquisitions, neither of which led to improved efficiency. Their work highlights the problems with both event studies and traditional accounting-based efficiency measures. Both acquisitions in their study turned out to be value reducing. In spite of this, one of them initially was viewed favorably by the stock market, illustrating that it may be hazardous to interpret event study results as evidence of efficiency gains. The accounting-based performance measures fared no better. Operating income (EBITDA) to sales and operating income to assets could indicate improved efficiency, even though in reality the opposite was true.[3] Their results suggest that long-term stock price movement is the one measure that does go in the right direction. Not surprisingly, the market adjusts the stock price downward (upward) as more and more negative (positive) information comes out. Thus, the results in Kaplan, Mitchell and Wruck [27] indicate that longer-term stock performance is correlated with actual firm performance, whereas no

[3]To be exact, both acquiring companies had increases in performance measures after the acquisitions. When adjusting for industry, one of the acquisitions still shows up as successful. When using the performance measure developed in Healy, Palepu, and Ruback [19], the other acquiring firm shows up as successful.

other performance measure short of firm-internal data seem to provide reliable measurement.

3 Methodology

Given the tension between long-horizon and short-horizon studies discussed in the previous section, another way to phrase our research question is as follows: Are the short-term gains to takeover eroded by the potentially negative long-term post-event drift? In this section, we develop the methodology that allows us to answer this question.

Briefly, our approach works as follows. We introduce targets and acquirors into our portfolio one month prior to the announcement of a successful merger. Targets remain until they de-list; acquirors (i.e., the emerging firms) remain for up to 36 months. In this manner, our portfolio captures pre-event information leakage, positive event-date reaction, and potential negative long-term drift. Benchmarking this portfolio against an asset-pricing model gives us a way of measuring the abnormal returns to takeover. The test thus includes the price reaction at the announcement as well as the market reaction to any information the market receives subsequent to a takeover, be it from earnings announcements, from disclosures of operating performance or from the collective efforts of the analyst community. Our three-year window incorporates all likely updating that the market requires about the value of the takeover. In addition, we also report results from shorter time horizons

(four, seven, twelve, and 24 months) to both allow our tests to better capture the manner in which the market revises its expectations as well as to provide important robustness checks. To model time variation in the volatility of our merger portfolio, we estimate factor models under a GARCH(1,1) error structure.

In the remainder of this section, we discuss our approach to measuring stockholder returns, and compare it to other methodologies commonly used. Since a detailed discussion of the pros and cons of various alternative methodologies is beyond the scope of this paper, we refer the reader to the surveys of Mitchell and Stafford [37], Fama [16], or Kothari and Warner [28] for a thorough treatment of the issues. Instead, we focus on the main points of contrast in existing techniques, as well as the contributions that our methodology makes.

3.1 Alternatives Methods for Measuring Performance

Two standard techniques for studying long-horizon stock price movement involve calculating either buy and hold abnormal returns, or cumulative average abnormal returns. Mitchell and Stafford [37] and Fama [16] document a number of methodological problems with these approaches. In addition, Brav [12] and Barber, Lyon, and Tsai [31] provide statistical corrections for these approaches when special conditions apply. For instance, Brav [12] provides a methodology for correcting problems arising from industry clustering.

Cumulative average abnormal returns (CAARs) suffer from three main

problems. First, since they represent returns in event time, not in calendar time, it is not possible for an investor to actually earn them. Thus, the exact consequences of positive or negative CAARs are somewhat unclear. But more critically, important statistical issues arise with their use. Barber and Lyon [7] and Kothari and Warner [28] have shown that their means are systematically non-zero (on random samples) and that their standard errors are understated, leading to rejections of the null too frequently. Thirdly, CAARs suffer from the problem of contemporaneous correlation in the returns of firms that are clustered in calendar time.

Buy and hold abnormal returns (BHARs) suffer from many of the same problems, plus additional ones. Unlike CAARs, an investor can earn a buy and hold returns, but the problems of inappropriate statistical inference remain. In addition, compounding buy and hold returns over long horizons induces skewness which furthers complicates the use of the standard statistical distributions for making inferences.

The problems with inappropriate statistical inference arise mainly because of differences between the control portfolio (the benchmark) and the study portfolio. For instance, the new listing bias occurs when new listings enter the benchmark, but not the study group, thus biasing upward the calculated CAARs or BHARs in relation to them. But the bias is not limited to the new listing bias; any latent effect that imparts differences between the study group and the reference group will introduce bias into the results.

Mitchell and Stafford [37] systematically catalogue and discuss the results

of these alternative methodologies, along with methodologies similar to the ones we use in this paper. They focus specifically on post-event studies, including merger, but also including IPOs, and SEOs. The general conclusion that they draw is that many of the recently documented post-event abnormal returns phenomena are highly sensitive to methodological specification, and frequently disappear under more robust methodologies. Our approach is to use a method that delivers robust conclusions for post-event studies, and improve it to account for its known shortcomings.

3.2 Calendar-Time Portfolio Regressions

Calendar-Time Portfolio Regressions (CTPRs) are performed by first forming an event portfolio comprising all firms that have experienced a relevant event within a certain time horizon. This portfolio is then regressed against a set of explanatory variables, in similar fashion to how mutual fund performance studies (see Ippolito [21], Elton et al. [14], or Carhart [13] for examples) are conducted. As in these studies, the explanatory variables can constitute an asset-pricing model, like the CAPM or the Fama and French [17] 3-factor model, or the right-hand side variables can simply be thought of as performance benchmarks.[4]

Formally, let acquiring firms be subscripted by A, targets by B: then

[4]The results we present are all benchmarked against the Fama and French [17] 3-factor model. CAPM results, which yield very similar conclusions, are available from the authors, but are omitted for brevity. Note that in order for us to draw efficiency implications from our findings, we must interpret the 3-factor model as a multi-factor efficient, empirical implementation of an ICAPM (see Merton [34]).

R_{At} denotes the return to an acquiring firm at time t and R_{Bt} denotes the return to a target firm at time t. Finally, let X denote the new firm after the takeover. Then at each time t, the returns to a dollar invested in our portfolio are described by

$$R_t = \sum_{i=t}^{t+k} \sum_{A,B=1}^{N} (\omega_{Ai} R_{Ai} + \omega_{Bi} R_{Bi}) + \sum_{i=t-J}^{t-1} \sum_{X=1}^{N} \omega_{Xi} R_{Xi} \tag{1}$$

where ω_{Lt} is an weight for firm L at time t. In our value weighted portfolio ω_{Lt} is equal to the ratio of the market capitalization of firm L to the total market capitalization of the portfolio, both measured at time $t-1$. The first summand in 1 captures the acquirors and targets in takeovers that will take place within the next k months, where k is chosen to appropriately capture any news leakage that occurs before the actual announcement date. The second summand captures all firms that have emerged from takeovers during the last J months.

The two summation signs in each summand account for the fact that we aggregate firms contemporaneously as well as over time. For each term, the inner summation captures all targets and bidders involved in a takeover at each point in time. The outer sum aggregates over time. One problem we encounter with our portfolio formation technique arises from time-series clustering of takeovers within and across industries. We discuss the implications of this problem, and our solution to them, in detail below.

By varying the holding period we can study the manner in which in-

formation is processed in the market. We let J equal four, seven, twelve, 24, and 36-month time horizons. The short horizon portfolios (four and seven month duration) allow us to account for the market's revision of initial expectations as new information contained in, e.g., the first few quarterly earnings announcements is released. The inclusion criteria for our portfolio means that measurement of each takeover starts one month before the first announcement and ends up to three years after the completion of the acquisition. Hence, we include the market's initial assessment as well as all subsequent information releases that cause the market to re-assess its view of the takeover.

Equipped with portfolio returns that measure price reactions before, during, and after the takeover event, we then calculate abnormal returns from an N-factor asset pricing model as follows:

$$R_t^p - r_t^f = \alpha + \sum_{i=1}^{N} \beta_i (R_t^i - R_t^f) + \epsilon_t \tag{2}$$

Depending on the model of expected returns, the number of factors i will vary. But under the null hypothesis that the model given by 2 explains stock returns, abnormal returns are captured by the constant term α.[5] Thus, this becomes our test of the welfare implications of takeover. In addition to factors measuring common variation in stock returns, we also include clean-up variables on the right-hand side to remedy problems arising from time

[5]Throughout the text, we refer to this constant term from the multiple regression as *Jensen's* α, following the literature on performance benchmarking.

series clustering in the left-hand side variable.

This research design has a number of desirable features. By including both bidders and targets from before the announcement until three years after the takeover we capture the combined effect of takeovers, thus attacking our research question of overall value implications head-on. Furthermore, the approach allows us to study the long-term stock price effects of takeover without biasing t-tests, since we avoid the skewness that results from compounding returns.

3.3 Correcting the Shortcomings of CTPRs

By examining average abnormal returns, we have disposed with the problems endemic to BHARs and CAARs. Thus, we can draw inferences from standard *t*-distributions that are free of statistical bias. In addition, the time series behavior of our portfolio vis-à-vis an asset-pricing model naturally accounts for contemporaneous correlation across firms. Finally, our returns are calendar time abnormal returns, making their economic implications as relevant as their statistical ones.

At the same time, we have inherited a new set of problems. One question we face is how to weight appropriately each firm in our portfolio. The abnormal returns to an equally weighted portfolio indicate the returns to a dollar equally split amongst all mergers, regardless of the size of the firms in question. On the other hand, abnormal returns to a value-weighted portfolio may be a more accurate measure of the economic relevance of merger, since

these returns give more weight to larger mergers. We present results from both approaches to facilitate comparison with other findings in the literature, however value-weighting is the approach that most directly addresses our research question.

Mitchell and Mulherin [36] have shown that mergers cluster within industries over time. Andrade and Stafford [3] show that some of this clustering is due to industry expansions and contractions. In other words, a particular industry will experience a wave of takeovers at a point in time, with one takeover triggering a number of others in the same industry. This time series clustering is a serious problem for our methodology. Consider the effect of a single, favorably-received takeover that leads to a spate of other takeovers occurring in the industry. We will spuriously reject the null of zero abnormal returns if takeovers are clustered together in such a way that the positive returns from the ensuing takeovers are not independent of the initial successful takeover. To control for this effect, Mitchell and Stafford [37] and others simply include a dummy for months in which the number of takeovers is high. This approach allows a separate intercept to be estimated for months in which the number of takeovers is high, but assumes that changing the portfolio's size only affects the conditional mean of the portfolio.

Our approach is to introduce generalized, autoregressive, conditional heteroscedasticity (GARCH(1,1)) estimation to allow for changes in the portfolio's composition over time to affect the conditional volatility of the portfolio returns. We estimate a GARCH(1,1) model by maximizing the joint likeli-

hood of the following two-equation system:

$$R_t^p - r_t^f = \alpha_0 + \sum_{i=1}^{N} \beta_I (R_t^i - R_t^f) + \epsilon_t \quad (3)$$

$$\sigma_{t,\epsilon}^2 = \omega + \gamma_1 \sigma_{t-1}^2 + \gamma_2 \epsilon_{t-1}^2 + \gamma_3 N_t \quad (4)$$

Here N_t is the number of takeovers that have occurred at time t. The GARCH(1,1) model captures the tendency of volatility to cluster in time: A high value of ϵ_t^2 increases σ_{t+1}^2 , which in turn increases the expectation of ϵ_{t+1}^2, and so on. A large (small) value of ϵ_t^2 tends to be followed by large (small) value of ϵ_t^2. Including N_t as an explanatory variable in the variance equation has two effects: first, it allows us to model time-variation in volatility as a function of firms entering and leaving the portfolio. Second, by doing this, it allows the number of firms in the portfolio to (nonlinearly) affect the point estimates obtained in equation 3.

Accounting for time-varying volatility in our portfolio helps us to address other potential problems as well. The risk characteristics of takeover firms may change discontinuously on the event date. Since the firm emerging from takeover typically absorbs the value of the target on its books,[6] simply benchmarking a firm against other with similar book-to-market ratio may lead to misleading classifications. Also, months of intense recent takeover activity will be more heavily skewed towards pre-merger firms, while in months of less

[6]As we discuss in greater detail below, the manner in which this transaction is recorded on the acquiror's books is a function of the method of accounting used in the merger.

intense activity our takeover portfolio will look more like a portfolio of non-takeover firms. We carefully manage these potential problems by examining changes in the factor loadings as the portfolio time horizon is lengthened, by examining decade-specific results, and by using the GARCH(1,1) error specification described above.

4 Data

On an empirical level, one of this paper's contributions is the size of the sample under analysis. First, we identify 3,467 takeovers occurring between 1963 and 1995 from the CRSP merger database, a database of over 5,200 takeover 'events' that took place between 1958 and 1995. The reasons for the data shrinkage are as follows. A 'takeover event' is any publicly announced acquisition offer in which a potentially anonymous bidder announces the intention to acquire a particular firm. Since we are interested in only those transactions in which both firms are listed on NYSE, Amex, or NASDAQ, we exclude transactions that ultimately result in acquisition by a private concern. Since many potential bidders may jockey for the ultimate control of a target firm, many takeover events may be associated with a single acquisition. This data selection approach yields an exhaustive sample of takeovers in which both bidder and target are publicly traded US firms.

When determining a firm's entry into our portfolio, we resolve these potential difficulties in the following manner. Target firms enter our portfolio

one month prior to the date of the first takeover announcement, regardless of whether this particular firm was successful in the takeover. One aspect of the efficiency gains of takeover is its role in providing information to financial markets. This strategy enables us to capture the information effects accruing to the target firm in the period during which potential bidders scramble to make a successful offer. For bidder firms, we use one month prior to the date of the successful announcement. Thus, for a given bidder-target pair in our portfolio, the bidder may not be the same one that triggered the inclusion of the target.

Once these dates are determined for our bidder and target firms, we use CRSP data to obtain monthly returns and market capitalization (price times shares outstanding) for five portfolios of varying horizons. For target firms, this data is always obtained starting one month prior to the first announcement, and extending forward to the date at which CRSP records the firm's de-listing from the exchange. For bidder firms, we begin collecting this information one month prior to the successful bid, as described above, but extend this forward for five horizons: four months, seven months, one year, two years, and three years. Finally, we use factors calculated by Fama and French [17] to form benchmarks. Using their market factor along with their size and book-to-market factors, we calculate monthly, average, abnormal returns based on their three-factor model.[7]

[7]While we only present results from 3-factor regressions, the results from CAPM regressions are similar at certain portfolio horizons. In our discussion of the results, we are careful to note instances in which the two diverge.

Table 1 presents summary statistics for our portfolio for each decade in our sample. Not only do the 1980s account for over one-third of our sample observations, but this period also accounts for over one-half of the cash and mixed payment transactions in our data. Since these summary statistics suggest that the 1980's may potentially differ from the remaining sample in an important way, we later present decade-specific breakdowns of our main findings.[8]

Table 2 analyzes the characteristics of the merger portfolios that are the dependent variables in our regression analysis. For our merger portfolio, which includes both the pre-announcement and post-merger effects, both the average, raw return and the Sharpe ratio decline monotonically as the portfolio horizon increases. There are two possible explanations for this. The first results from the fact that the number of firms in the portfolio is increasing monotonically as the horizon lengthens. As we lengthen the portfolio horizon, a larger and larger fraction of each month's return is accounted for by firms that are farther and farther from the takeover event that caused them to be initially included. Thus, the 36-month portfolio is much closer to a portfolio of 'average', non-merger stocks than is the four month portfolio. Indeed, if market efficiency holds, we should expect the abnormal returns to this portfolio to be quite close to those of a non-merger portfolio. By contrast, the second possible reason for this decline is based on market inefficiency. The

[8]Indeed, the difference between GARCH and OLS estimates is most pronounced on decade-specific samples.

alleged post-merger drift phenomenon is a second reason why average returns may decline monotonically as the horizon lengthens. However, comparing the post-merger portfolio (which shows a decline in raw returns of only 20 basis points as the horizon lengthens) with the merger portfolio suggests that this phenomenon accounts for only a small portion of the overall decline.

Another important feature of our portfolios revealed in table 2 is the number of firms in the different portfolios. No matter whether we look at overall merger portfolios, or portfolios based on merger characteristics, we always have over thirty firms in each portfolio *on average*. But the minimum number of firms becomes quite small for some portfolios as the time horizons shrinks. At horizons below one year, we sometimes have minimum portfolio sizes of fewer than ten firms. Since an individual stock has a much larger impact on the overall portfolio variance in these situations, accounting for the number of firms in the GARCH variance specification is critical at short horizons.

5 Evidence from Takeover Portfolios

We present GARCH(1,1) estimates of monthly, average abnormal returns in excess of the 3-factor model in Table 3. Jensen's α for our four-month horizon value-weighted portfolio is 72 basis points per month, which is highly statistically significant. Even at the twelve-month horizon, the value weighted portfolio has a statistically significant 30 basis points average, abnormal re-

turn. At 24- and 36-month horizons, the portfolios' abnormal returns remain positive, although their statistical significance weakens dramatically.[9] For comparison purposes, Table 4 reports the same regressions run under OLS, without a correction for time-variation in the error variance. Comparing the αs from the two tables shows that OLS estimates are biased towards zero: ignoring the fact that the error variance is not constant over time results in underestimating the shareholder value from takeovers. Indeed, the ARCH(1) and GARCH(1) effects are highly statistically significant at all horizons, and the count of the portfolio size is significant at short horizons, where variation in the portfolio size is most pronounced.[10]

The pattern of factor loadings in the value-weighted results presented in Table 3 suggest that our results are not influenced by the choice of asset-pricing model. At horizons of one year or less, the loadings on SMB and HML are statistically insignificant. Thus, the CAPM would yield virtually identical predictions to the 3-factor regressions we report.[11] Only at longer horizons does the 3-factor model depart from the CAPM, and then only because the loading on SMB becomes statistically significant.

The second panel of Table 3 presents results based on equally weighted merger portfolios. While equally weighted results are not strictly appropriate

[9]Note that under market efficiency, the α on our portfolio should converge to zero as the time horizon lengthens, since the effect of pre-announcement returns have a vanishingly small effect on the overall portfolio as the horizon increases.

[10]Likelihood ratio tests (omitted for brevity) strongly reject the null hypothesis that the equation obtained by GARCH(1,1) estimation is identical to that obtained by OLS.

[11]In tables available from the author, we present GARCH(1,1) estimates of CAPM regressions that explicitly demonstrate this.

for addressing our question of the overall economic gains to takeover, they do serve as a basis of comparison with other findings in the literature. The abnormal returns to equally weighted portfolios are massive, both statistically and economically. For the four-month horizon, the portfolio returns almost 3% per month; this decays to a highly statistically significant 71 basis points as the time horizon is expanded to 36 months. Thus, in equally weighted portfolios, the pre-announcement run-up and the announcement-period returns more than outweigh any alleged post-merger drift.

Comparing the factor loadings between Tables 3 and 4 further demonstrates the importance of accounting for conditional volatility in the regression residual. The value weighted results from Table 3, for instance, demonstrate a significant negative loading on SMB at horizons greater than one year. OLS biases this loading towards zero; in the OLS regressions presented in Table 4, SMB loadings are neutral and insignificant. Although the loadings on HML are never statistically significant for either GARCH or OLS at any portfolio horizon, the same downward bias appears in these coefficients as well.

One concern with our approach arises from the fact that the risk characteristics of the firms in the portfolio may change discontinuously around the event date. Examining the factor loadings for different portfolio horizons allows us to conclude that this is not a source of mis-measurement in our results. Note that the loading on the SMB factor in Table 3 stays relatively constant around $-.05$ percent; it is the standard error that changes as the

portfolio horizon increases. This stability is not found in the OLS regressions; Table 3 shows that the SMB loading declines from positive .04 to $-.01$ as the portfolio horizon is expanded. This comparison suggests that modeling the time variation in the residual volatility is important for correcting biases in the CTPR approach that have not been recognized in the literature to date.

6 Extensions and Robustness Checks

This section extends the basic portfolio formation methodology to explore a number of inter-related questions. First, as an additional robustness check, we present results from portfolios formed exclusively from post-event data. This is a safeguard against the potential criticism that without efficient markets we are unfounded in drawing economic efficiency conclusions from the positive abnormal returns to our merger portfolios. Second, we form sub-portfolios (including pre-announcement *and* post-announcement data) conditional on the characteristics of the merger. In particular, we divide our portfolios into sub-portfolios based on whether the bidder and target were in the same industry. We also form portfolios based on whether the method of payment was cash or stock. This distinction turns out to be highly relevant for the current debate on the future of pooling-of-interests accounting. Finally, we present results on a decade-by-decade basis to explore whether different merger waves might influence our overall results.

6.1 The Performance of Post-Takeover Portfolios

In this section, we perform another important robustness check and examine the results of post-event portfolios. This allows us to account for two potential problems in our results. First, we ensure that problems with joint hypothesis of market efficiency and the correct asset-pricing model do not corrupt our welfare assessments. Second, by comparing the factor loadings with those obtained earlier, we safeguard against the fact that the risk characteristics of firms are changing discontinuously around the event date.

We proceed by forming portfolios exactly as described in earlier sections, except that we focus exclusively on bidders. They enter the portfolio one month after the de-listing date of the target. At horizons of greater than one year, the point estimates on our value weighted portfolios demonstrate the downward drift documented elsewhere in the literature. This drift, however, is not significant in any economic or statistical sense.

Comparing the value weighted results between our merger and post-merger takeover portfolios, we see that the factor loadings on the market factor, SMB, and HML are very stable. Not only are the factor loadings stable as the portfolio horizon is expanded, but they are also robust to the exclusion of the pre-announcement and event period. This is an advantage to our risk-based approach over the standard characteristics-based approach used in Rau and Vermaelen [40] and elsewhere. The characteristics-based approach matches bidders to a matched sample on the basis of book-to-market, which almost certainly be confounded by the fact that the book equity of

the acquiror will change dramatically around the takeover event. If book-to-market is a proxy for risk characteristics, then this discrete change in book-to-market around the event date may cause firms to be classified incorrectly. Our risk-based approach avoids this problem, since any value vs. glamour judgment that one would draw from our findings would be purely based on a covariance with HML, a factor based on book-to-market risk in the universe of all publicly-traded firms (not just firms which have had recent, dramatic revaluations in their book-to-market ratios).

The equally weighted results do, however, exhibit some evidence of post-merger drift. The average abnormal returns for the four month horizon portfolio are positive (and insignificant) 7 basis points, while at the three-year horizon they are $-.15$ basis points, with a t-statistic of -2.55. As the portfolio horizon increases, the equally weighted portfolio loads more heavily on HML, indicating that the portfolio increasingly has risk characteristics like those of high book-to-market (value) stocks. Thus, this portfolio, which increasingly under-performs its benchmark, looks more and more like a portfolio of small, value stocks as the horizon increases. That the value weighted portfolio does not share these characteristics suggests that the bulk of this phenomenon is concentrated among small, economically unimportant mergers. Since our research question concerns the overall economic gains to takeover as experienced by *all* shareholders, the post-merger results that are most relevant to our findings are the value-weighted results. And they show that there is no evidence of post-merger drift that would encroach on our ability to draw

value-relevance conclusions from our results.

6.2 Are Mergers Across Industries Value Destroying?

Mitchell and Mulherin [36] document time-series clustering of takeovers on an industry by industry basis. This is evidence of industry-specific merger 'waves.' Andrade and Stafford [3] show that these waves comprise both an expansionary component, during which mergers accomplish industry growth, and a contractionary component, in which consolidation results in lower output and lower industry capacity. These findings suggest that whether the merger occurs between firms within the same industry or across different industries may have implications for the overall efficiency of the merger. Indeed, Morck, Shleifer, and Vishny [38] sample 326 mergers occurring before 1988 and show that bidders in diversifying mergers—mergers where the bidder acquires a firm outside its industry—earn negative abnormal returns around the announcement.

Our portfolio formation technique, coupled with our large sample, allows us to expand the Morck, Shleifer, and Vishny results to see if they have importance for the overall value implications of merger. Theoretical evidence in Jensen [22] and in Roll [42] suggest that if managers are motivated by hubris or empire-building, then merger may not be value-enhancing to the manager's shareholders. However, no evidence exists as to whether hubris-motivated managers ill serve the economy as a whole.

To do this, we form merger portfolios exactly as before, except that we

classify all mergers into two categories, within-industry and across-industry, based on the 2-digit SIC code membership of the bidder and target. These results are present in table 6.

Table 6 shows no evidence of the alleged negative performance of across-industry takeovers. Bidders may well over-pay, however this results in simply a transfer of wealth from one class of shareholders to another. It does not result in overall value destruction. This is an important finding, because it indicates that while managers' incentives may be at odds with those of their shareholders, they do not decrease the overall economic value of publicly-traded firms.

The results do indicate that mergers occurring between firms in the same industry are more value creating than diversifying mergers. The difference in the monthly returns at the four month portfolio horizon is roughly twenty basis points, which amounts to about 3% per year. This difference in value weighted abnormal returns remains at horizons of up to two years.

Interestingly, just the opposite pattern in the abnormal returns obtains for equally weighted portfolios. Across-industry mergers outperform within-industry mergers by fifteen to twenty basis points at all portfolio horizons. While the results from equally weighted portfolios do not directly address our research question, the contrast between them and the value weighted results are suggestive of the sources of value creation in within- and across-industry results, and of the likely scenarios that the Jensen [22] and Roll [42] best describe. They suggest that large companies expanding their borders across

industry boundaries are precisely those which do the most damage to overall shareholder value.

6.3 Abnormal Returns by Mode of Payment

In table 7, we provide abnormal returns according to the mode of payment used in the transaction. We ignore transactions involving mixed or unknown payment and focus exclusively on all-cash or all-stock transactions,[12] which allows us to accomplish two tasks. First, this facilitates comparison with the work of Loughran and Vijh [30], Rau and Vermaelen [40] and others who explicitly test hypotheses regarding mode of payment.

Secondly, analyzing abnormal returns by mode of payment allows us to bring our methodology to bear on the ongoing debate about the whether firms should be allowed to account for mergers by using the pooling-of-interests method. Loughran and Vijh [30] argue that undervalued firms will prefer to pay by cash, whereas overvalued firms prefer to pay stock. We put forward another important argument, namely that type of payment also proxies for accounting treatment, which in turn can serve as a powerful proxy for the actual economic merits of the takeover.

The choice of accounting method affects only *accounting* accruals, and not the underlying *economic* cash flows. Pooling involves joining together the two balance sheets of the bidder and target into one merged balance sheet,

[12]This eliminates 682 observations from our sample that are either classified as mixed payment or unknown payment type.

where the target's books are written onto the bidder's books at book value. On the other hand, purchase accounting requires the bidder to write the books of the target on at market value, recognizing any additional premium as a long-lived asset (goodwill), which is then written down against future years income statements. Thus, purchase accounting will lead to lower future accounting earnings than pooling-of-interests accounting, since the purchase method involves extra depreciation and amortization expense. But this only affects accounting earnings, and not cash flows.

Since the choice of accounting method only affects earnings per share, and has no economic impact on the fundamentals of the business, one would not expect the choice to matter. Firms that recognize the market's ability to 'see through' the transaction should simply pursue the method that involves the lowest transactions costs.

In spite of the fact that the choice of accounting method has no cash flow consequences, we see that firms clearly exhibit a preference for pooling over purchase. One example can be found in Lys and Vincent [32], who document the fact that AT&T paid as much as $500 million to satisfy the strictures of pooling accounting in its acquisition of NCR. Consequently, earnings per share were increased by 17% by what they would have been under purchase accounting, but this had no effect whatsoever on cash flows. Lys and Vincent [32] find that the merger with NCR ultimately wasted $3.0 billion of stockholder wealth.

There still appears to be considerable confusion about the would-be mer-

its of favorable accounting methods. Concerning the recent merger agreement between Pfizer and Warner-Lambert, which involved a $1.8 billion payment to American Home Products as part of a concession fee, the New York Times [39] had this to say about the two methods of accounting:

> American Home also held certain stock options that prevented Pfizer or any other suitor of Warner-Lambert from using a favorable accounting method known as pooling. This week all three companies have been grappling with how to pay American Home more than the $1.8 billion breakup fee, without jeopardizing Pfizer's ability to use the favorable accounting. ... The S.E.C. could then force Pfizer and Warner-Lambert to use another method of accounting *that would significantly reduce the combined company's profit for many years.* (italics added)

FASB regulations require firms to use purchase accounting for a merger if any one of 12 conditions is not met. The regulations alluded to in the New York Times article quoted above concern major payments to shareholders prior to the merger event, which are forbidden under pooling accounting. For our purposes, however, the salient criteria involve the method of payment: firms can only use pooling if the transaction is stock-for-stock. Any cash transactions must be accounted for using purchase accounting.

Thus, the method of payment provides a proxy for the type of accounting used in the transaction. We divide firms into three groups based on method of payment: cash, stock and mixed. Every cash transaction is purchase, and all pooling transactions are in the stock category. While the method of payment is not a perfect proxy, the evidence in Andrade [2] suggests that

the correlation between type of payment and type of accounting treatment is quite high. In his sample of 224 acquisitions in which the target was large relative to the acquiror, he finds that 100% of the all-cash transactions use purchase accounting, while only a handful of all-stock transactions are purchase. Since his data are a subsample of our data ranging from 1975 to 1996, we feel confident that the type of payment is highly correlated with the accounting treatment. To keep from smearing the distinction between pooling and purchase, we exclude mixed method of payment.

These results are presented in Panel B of table 7. Across the board, cash mergers generate higher returns to shareholders than stock mergers. The 4 month value weighted portfolio of cash payment mergers earns a statistically significant 92 basis points per month, while its counterpart stock payment portfolio earns only 29 basis points (still statistically different than zero). While the abnormal returns diminish for both portfolios as the portfolio horizon increases from seven to 36 months, this performance gap between cash and stock payment never vanishes. At the 36-month horizon, the cash portfolio earns 33 basis points, with a t-statistic of over 4, while the stock portfolio earns an insignificant -12 basis points.

The same pattern in abnormal returns can be found in the equally weighted portfolio at various horizons. The equally weighted cash portfolio earns 4.28% per month, which, not surprisingly, is highly statistically significant. By comparison, the stock payment portfolio earns 2.27% per month.

These results are consistent with the fact that cash deals generate higher

returns than stock deals. While this is consistent with the idea that firms pay for acquisitions with the cheapest currency available to them, whether that be cash or there own company's stock, the results also support our hypothesis that cash (purchase accounting) takeovers are substantially more value generating than stock (pooling) mergers. Indeed, the two hypotheses may be intrinsically linked: firms with over-valued stock may be exactly the ones which hope to 'fool the market' into imputing real economic value to accounting accruals.

Andrade [2] finds that EPS dilutive acquirors show only mild negative returns, an order of magnitude less than what is predicted by a 'fool the market' theory like the one we sketch above. Our findings show that while accounting for the welfare that is transferred from acquirors to targets make the overall results positive to both types of shareholders, significantly more wealth is generated in cash transactions. These findings lends support to the current efforts of the FASB to abolish pooling accounting.

6.4 Decade-by-Decade Results

To take a closer at the time series behavior of the abnormal returns of the portfolios we study, we break our results down by decade. Earlier, we noted that Table 1 alerted us to the potential for the 1980s to be influential in our results. This suspicion is partially borne out in Table 8, which presents these decade-by-decade results for value weighted portfolios.

Judging from value weighted results, the 1980s not only add more than

any other decade to the positive returns on our four-month horizon merger portfolio, but they also detract the most from the 36-month post-merger portfolio. While all the other decades reported show highly positive and significant abnormal returns on the value weighted four month portfolio, the abnormal returns during the 1980s were about twice as high as either the 1970s or the 1990s. On the other hand, the 1980s are the only decade to show statistically negative returns to the value weighted post-merger portfolio at the 36-month portfolio horizon. These findings suggest that the vast number of studies that rest heavily on observations drawn from this time-frame are likely overstating any post-merger drift that is present in the takeover market as a whole.

This same pattern in the abnormal returns can be found in the equally weighted portfolios summarized in Table 9. By far the most negative and statistically significant equally weighted post-merger abnormal return occurs in the 1980s sub-sample. Comparing the equally weighted 36-month portfolio results for the cash-only portfolio for the 1980s sub-sample with that of the surrounding sub-samples furthers this point: in the 1980s, returns from cash-only deals were a good deal less positively valued than cash-deals in surrounding time periods. The stock-only results for the equally weighted portfolio are even more dramatic. In the 1980s, the abnormal return was only 9 basis points, whereas the 1970s and 1990s were 41 and 106 basis points, respectively.

While it is not generally possible to average the (non-linear) GARCH

parameter estimates across portfolios, the results suggest that our understanding of post-merger drift would be quite different were it not for a spike in the 1980s. And while this spike is certainly an important phenomenon to understand in its own right, when placed in the larger context of overall economic welfare it may be of limited importance here. In other words, that which is unique to the 1980s may be informative for questions of corporate governance, but it seems less important for the more general question of economic efficiency.

7 Conclusion

Does the institution of takeover increase the overall economic welfare of the owners of capital? This seemingly straightforward question has not been fully addressed in the vast literature that has studied takeover. Instead, various strands of the literature have focused on smaller pieces of this question, and these individual strands have frequently arrived at answers that confound the overall picture of the economic welfare effects of takeover.

This paper develops and implements a comprehensive methodology for answering the broader question concerning the economic efficiency of takeovers. By accounting for the effects stockholders experience before, during, and after the event, we speak to the overall efficiency gains of takeover as a corporate governance institution. Our results provide strong evidence that takeover does, indeed, increase the welfare of shareholders in the long run. A battery

of robustness checks indicates that our tests are not confounded with problems of market inefficiency, inappropriate risk adjustments, or changes in the time-series of return volatility.

In addition to providing a robust answer to the question of stockholder welfare, we also cast light on why earlier findings in the literature differ so dramatically. The fact that the existing literature draws different conclusions about the efficiency effects of takeover appears to stem from three main sources: equally weighting takeovers, varying samples, and varying evaluation techniques. We address each of these. Our sample is larger than in most earlier studies: in fact, we have all takeovers involving NYSE, Amex and NASDAQ firms between 1963 and 1995. Finally, we evaluate the performance of our portfolio in a way that does not introduce bias into our statistical tests. The decade-by-decade breakdowns of our main findings suggest that many results are sensitive to the time period in which the sample is drawn. This effect is especially true among equally-weighted results, which most closely parallel the BHARs and CAARs that are common in the evaluation of post-merger takeover performance.

The flexibility and robustness of our approach allows us to explore a number of questions inter-related questions. First, we do find evidence that is not inconsistent with managerial hubris and empire-building as motives for merger. But putting these motives in a larger context, we conclude that while ill-motivated mergers may not necessarily be good for a certain class of shareholders, they are definitely not (on average) harmful to society as

a whole. Second, our findings buttress other recent results showing that, contrary to popular opinion, pooling-of-interests accounting does not fool a market that is too naive to see the difference between accounting conventions and economic reality. Far from it: the market on average rewards firms (in a relative sense) who adopt purchase accounting, a fact which may speak to the underlying economic fundamentals of the business deals that are undertaken with pooling or purchase accounting in the first place. These findings suggest that the answers to broad questions concerning economic welfare can inform narrower questions of economic policy and corporate governance.

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Table 1: Summary Statistics

This table catalogs the number of takeover events in our sample by date and by type of takeover. Grand total lists the total number occurring in our sample in the date range indicated on the left. Industry status is 'within' if the two firms belong to the same 2-digit SIC code, otherwise the merger is an across-industry merger.

Year Range	Grand Total	Industry Status		Type of Payment:				Median Size Differential
		Within	Across	Cash	Stock	Mixed	Unknown	
1963-1970	532	164	368	63	373	57	39	5.0
1971-1980	899	289	610	315	385	139	60	5.5
1981-1990	1212	482	730	548	375	208	81	4.0
post-1990	606	320	286	166	342	72	26	5.4
Overall	3249	1255	1994	1092	1475	476	206	4.9

Table 2: Portfolio Characteristics

This table presents the summary statistics for the portfolios that we subsequently use in asset pricing tests. Merger portfolios are formed by introducing targets one month prior to the first announcement associated with a successful takeover. Successful bidders are introduced one month prior to their first bid (thus the target may enter prior to the successful bidder) and remain past the target's de-listing date for the duration indicated. Post-merger portfolios are formed by including bidders one month after the target's de-listing, and holding them for the indicated duration. Within industry mergers are ones occurring within the same 2-digit SIC Code, while in Across industry mergers the bidder and target are in different 2-digit SIC Codes. Cash and Stock refer to the exclusive mode of payment. Returns are measured monthly, in percent. Sharpe Ratios are based on monthly returns as well.

Merger Portfolios

Summary Statistic:	4 Month	7 Month	1 Year	2 Year	3 Year
Mean Return	3.278	2.756	2.270	1.644	1.363
Sharpe Ratio	0.61	0.50	0.41	0.30	0.25
Min # Firms	19	26	28	46	53
Max # Firms	181	220	267	378	503
Avg # Firms	86	113	150	231	305

Post-Merger

Summary Statistic:	4 Month	7 Month	1 Year	2 Year	3 Year
Mean Return	0.772	0.754	0.731	0.605	0.560
Sharpe Ratio	0.13	0.13	0.13	0.11	0.10
Min # Firms	6	10	16	33	40
Max # Firms	85	118	170	296	412
Avg # Firms	37	59	94	175	250

Table 2: Portfolio Characteristics, continued

Across-Industry Mergers					
Summary Statistic:	4 Month	7 Month	1 Year	2 Year	3 Year
Mean Return	1.08	0.86	0.72	0.55	0.52
Sharpe Ratio	0.21	0.17	0.14	0.11	0.11
Min # Firms	12	15	21	31	33
Max # Firms	116	148	187	269	344
Avg # Firms	52	68	91	142	189
Within-Industry Mergers					
Summary Statistic:	4 Month	7 Month	1 Year	2 Year	3 Year
Mean Return	1.42	1.04	0.86	0.62	0.54
Sharpe Ratio	0.26	0.19	0.17	0.13	0.11
Min # Firms	3	6	7	14	20
Max # Firms	85	111	140	205	255
Avg # Firms	34	44	58	89	116
Mode of Payment: Cash					
Summary Statistic:	4 Month	7 Month	1 Year	2 Year	3 Year
Mean Return	1.30	1.11	0.91	0.94	0.87
Sharpe Ratio	0.24	0.21	0.18	0.20	0.18
Min # Firms	6	9	12	18	21
Max # Firms	90	110	140	210	270
Avg # Firms	37	49	66	103	137

Table 2: Portfolio Characteristics, continued

Mode of Payment: Stock					
Summary Statistic:	4 Month	7 Month	1 Year	2 Year	3 Year
Mean Return	0.67	0.52	0.43	0.32	0.29
Sharpe Ratio	0.12	0.10	0.08	0.06	0.06
Min # Firms	17	27	38	67	101
Max # Firms	112	128	150	204	256
Avg # Firms	42	55	73	110	146

Table 3: GARCH(1,1) Estimates of Abnormal Returns to Merger Portfolios

This table presents GARCH(1,1) estimates of abnormal returns to merger portfolios. The three factors are the excess return on the market portfolio, SMB (a zero-cost portfolio capturing the difference between small and large stocks), and HML (a zero-cost portfolio capturing the difference between high and low book-to-market stocks). Acquirors enter the portfolio one month prior to the announcement of the successful merger and remain for the number of months indicated by Horizon. Targets enter the portfolio one month prior to the earliest announcement leading to the successful takeover, and remain until they de-list. Point estimates are presented in bold; t-statistics using Bollerslev-Wooldridge [10] standard errors appear in italics beneath the point estimates. Maximum likelihood is used to estimate the regression system:

$$\begin{aligned} R_t^p - r^f &= a + \beta(RMRF) + s(SMB) + h(HML) + \epsilon_t \\ \epsilon_t^2 &= \omega + \gamma_1 \sigma_{t-1}^2 + \gamma_2 \epsilon_{t-1}^2 + \gamma_3 N_t \end{aligned}$$

where N_t is a count of the number of mergers that have occurred between $t-1$ and t.

Value Weighted Portfolios					
Portfolio Horizon:	4 Month	7 Month	12 Month	2 Year	3 Year
a	0.72	0.46	0.30	0.09	0.05
	7.92	5.52	4.19	1.58	0.96
β	1.02	1.04	1.05	1.05	1.05
	49.31	51.15	60.11	70.96	85.83
s	-0.04	-0.03	-0.05	-0.07	-0.06
	-1.02	-0.77	-1.62	-2.97	-2.80
h	-0.01	0.01	0.00	0.03	0.04
	-0.14	0.25	-0.09	1.08	1.54
ARCH(1)	0.10	0.09	0.10	0.08	0.13
	1.98	2.31	2.34	2.54	3.53
GARCH(1)	0.81	0.81	0.76	0.86	0.83
	11.09	10.54	7.85	13.03	16.26
Count	-0.01	-0.01	-0.01	-0.01	-0.01
	-2.01	-1.62	-1.48	-1.05	-0.79
R^2	82%	86%	89%	92%	93%

Table 3: GARCH Estimates, continued

Equally Weighted Portfolios					
Portfolio Horizon:	4 Month	7 Month	12 Month	2 Year	3 Year
a	2.73	2.16	1.67	0.98	0.71
	28.75	26.67	24.53	16.18	12.90
β	0.94	0.98	1.00	1.04	1.06
	34.77	43.74	55.48	62.01	67.38
s	0.63	0.61	0.59	0.59	0.58
	15.94	18.44	19.91	21.16	23.22
h	0.15	0.15	0.15	0.19	0.21
	3.25	4.04	4.67	6.33	8.30
ARCH(1)	0.07	0.08	0.11	0.08	0.18
	1.57	1.92	2.21	1.94	2.71
GARCH(1)	0.58	0.60	0.58	0.48	0.69
	4.00	3.88	3.68	1.64	6.39
Count	-0.02	-0.01	-0.01	-0.01	-0.01
	-2.47	-2.18	-2.20	-1.44	-1.17
R^2	85%	89%	92%	95%	95%

Table 4: OLS Estimates of Abnormal Returns to Merger Portfolios

This table presents OLS estimates of abnormal returns according to the Fama and French [17] 3-factor model. The three factors are described in Table 3. The construction of the Merger Portfolio is described in Table 2. OLS point estimates are presented in bold; t-statistics appear in italics beneath the point estimates.

Value Weighted Portfolios					
Portfolio Horizon:	4 Month	7 Month	12 Month	2 Year	3 Year
a	0.65	0.40	0.28	0.08	0.02
	6.15	4.21	3.34	1.22	0.38
β	1.01	1.04	1.05	1.06	1.06
	38.04	43.41	49.97	60.30	65.59
s	0.03	0.04	0.01	-0.01	-0.01
	0.72	1.00	0.43	-0.47	-0.56
h	0.00	0.02	0.01	0.01	0.02
	0.05	0.42	0.22	0.44	0.88
R^2	83%	86%	89%	92%	93%
Equally Weighted Portfolios					
Portfolio Horizon:	4 Month	7 Month	12 Month	2 Year	3 Year
a	2.63	2.08	1.58	0.92	0.62
	24.51	22.43	19.86	14.15	10.43
β	0.96	1.01	1.04	1.06	1.08
	35.59	43.09	51.63	64.65	71.51
s	0.64	0.63	0.63	0.63	0.63
	16.03	18.47	21.34	26.07	28.51
h	0.13	0.14	0.15	0.20	0.21
	3.03	3.65	4.71	7.28	8.73
R^2	85%	89%	92%	95%	96%

Table 5: Results from Post-Merger Portfolios

This tables reports GARCH regressions performed on post-merger portfolios, which are formed by including successful bidders beginning the month after the target de-lists. Otherwise, the procedure is identical to that reported in table 3.

Value Weighted Portfolio					
Portfolio Horizon:	4 Month	7 Month	12 Month	2 Year	3 Year
a	0.03	0.04	0.06	-0.08	-0.06
	0.25	0.42	0.82	-1.20	-1.08
β	1.11	1.08	1.08	1.07	1.06
	35.88	45.15	48.11	57.35	79.49
s	-0.01	-0.06	-0.09	-0.06	-0.07
	-0.16	-1.30	-2.20	-2.14	-2.84
h	0.01	-0.01	-0.05	0.01	0.01
	0.18	-0.27	-1.16	0.30	0.37
ARCH(1)	0.36	0.13	0.13	0.09	0.13
	1.96	2.56	2.52	2.47	3.51
GARCH(1)	0.24	0.58	0.71	0.85	0.81
	1.25	4.19	5.86	12.86	13.04
Count	-0.08	-0.02	0.00	0.00	0.00
	-1.81	-2.35	-1.38	-1.01	-0.98
R^2	0.77	0.84	0.87	0.91	0.93
Equally Weighted Portfolio					
Portfolio Horizon:	4 Month	7 Month	12 Month	2 Year	3 Year
a	0.07	-0.06	-0.07	-0.16	-0.15
	0.67	-0.69	-0.84	-2.46	-2.55
β	1.17	1.15	1.11	1.11	1.12
	36.34	47.25	51.47	56.41	55.20

Table 5: Results from Post-Merger Portfolios, continued

s	0.58	0.56	0.58	0.57	0.57
	12.00	16.17	15.81	20.01	20.71
h	0.16	0.13	0.10	0.18	0.21
	2.74	3.97	2.61	6.85	8.55
ARCH(1)	0.14	0.25	0.02	0.12	0.17
	1.43	2.52	0.64	3.44	2.81
GARCH(1)	-0.08	0.44	0.55	0.83	0.75
	-0.61	2.85	1.22	16.64	9.52
Count	-0.10	-0.02	-0.01	0.00	0.00
	-3.16	-1.96	-0.87	-0.30	-0.26
R^2	0.86	0.90	0.93	0.95	0.95

Table 6: Mergers Within and Across Industries

This table presents GARCH(1,1) estimates of monthly, average, 3-factor abnormal returns to a merger sub-portfolio formed by restricting the merger portfolio to industries that occur either within or across industries. For definitions, see tables 1 and 2. The t-statistics are based on Bollerslev-Wooldridge [10] standard errors.

Across-Industry Mergers: Value Weighted Portfolio					
Horizon:	4 Month	7 Month	12 Month	2 Year	3 Year
a	0.70	0.47	0.28	0.07	0.08
t(a)	6.89	4.99	3.65	1.18	1.31
Within Mergers: Value Weighted Portfolio					
Horizon:	4 Month	7 Month	12 Month	2 Year	3 Year
a	0.99	0.60	0.44	0.21	0.09
t(a)	6.78	4.64	3.94	2.32	1.11
Across-Industry Mergers: Equally Weighted Portfolio					
Horizon:	4 Month	7 Month	12 Month	2 Year	3 Year
a	2.80	2.22	1.65	1.05	0.76
t(a)	21.83	20.99	18.19	14.40	12.29
Within-Industry Mergers: Equally Weighted Portfolio					
Horizon:	4 Month	7 Month	12 Month	2 Year	3 Year
a	2.61	2.04	1.57	0.89	0.56
t(a)	19.26	16.95	15.43	11.07	7.68

Table 7: Abnormal Returns and Type of Payment

This table presents GARCH(1,1) estimates of monthly, average, 3-factor abnormal returns to a merger sub-portfolio formed by restricting the merger portfolio according to whether the merger was paid in cash or with the bidder's stock. For definitions, see tables 1 and 2. The t-statistics are based on Bollerslev-Wooldridge [10] standard errors.

Cash Payment: Value Weighted Portfolio					
Horizon:	4 Month	7 Month	12 Month	2 Year	3 Year
a	0.92	0.61	0.49	0.35	0.33
t(a)	6.39	5.42	4.60	4.13	4.06
Stock Payment: Value Weighted Portfolio					
Horizon:	4 Month	7 Month	12 Month	2 Year	3 Year
a	0.29	0.15	-0.02	-0.15	-0.12
t(a)	2.11	1.29	-0.24	-1.57	-1.31
Cash Payment: Equally Weighted Portfolio					
Horizon:	4 Month	7 Month	12 Month	2 Year	3 Year
a	4.28	3.41	2.60	1.65	1.26
t(a)	22.52	21.48	18.94	17.26	15.43
Stock Payment: Equally Weighted Portfolio					
Horizon:	4 Month	7 Month	12 Month	2 Year	3 Year
a	2.27	1.70	1.25	0.73	0.47
t(a)	20.09	14.01	11.43	9.07	5.30

Table 8: Summary of Value Weighted Results, by Decade

This table summarizes the value weighted average, abnormal returns to different portfolio classifications and presents results decade-by-decade. Horizon denotes whether bidders remain in the portfolio for four months or 36 months (interim results are omitted for brevity). Numbers appearing below point estimates are t-statistics based on Bollerslev-Wooldridge [10] standard errors.

Overall

Horizon	1963-1970	1971-1980	1981-1990	post-1990	Overall
4	0.84	0.41	0.91	0.51	0.72
	5.21	2.66	7.03	2.53	7.92
36	0.28	-0.03	-0.04	0.04	0.05
	3.30	-0.32	-0.49	0.24	0.96

Post-Merger

Horizon	1963-1970	1971-1980	1981-1990	post-1990	Overall
4	-0.34	0.08	-0.05	0.15	0.03
	-1.77	0.37	-0.56	0.77	0.25
36	-0.17	-0.07	-0.19	0.17	-0.06
	-1.92	-0.67	-2.39	1.40	-1.08

Mode of Payment: Cash

Horizon	1963-1970	1971-1980	1981-1990	post-1990	Overall
4	(from 7301)	1.42	0.86	0.37	0.92
		6.01	4.63	2.84	6.39
36	(from 7301)	0.18	0.18	0.28	0.33
		0.97	2.20	1.68	4.06

Mode of Payment: Stock

Horizon	1963-1970	1971-1980	1981-1990	post-1990	Overall
4	(from 7301)	-0.04	0.34	0.54	0.29
		-0.29	1.66	4.03	2.11
36	(from 7301)	-0.24	-0.19	-0.13	-0.12
		-1.92	-1.29	-0.57	-1.31

Within Industry					
Horizon	1963-1970	1971-1980	1981-1990	post-1990	Overall
4	1.20	0.59	1.08	0.69	0.99
	4.13	1.92	5.33	2.63	6.78
36	0.26	0.00	-0.16	0.26	0.09
	2.85	-0.02	-1.40	1.35	1.11
Across Industry					
Horizon	1963-1970	1971-1980	1981-1990	post-1990	Overall
4	0.87	0.54	0.82	0.42	0.70
	4.91	3.27	5.35	2.17	6.89
36	0.18	0.07	-0.02	0.19	0.08
	1.28	0.59	-0.30	1.16	1.31

Table 9: Summary of Equally Weighted Results, by Decade

This table summarizes the value weighted average, abnormal returns to different portfolio classifications and presents results decade-by-decade. Horizon denotes whether bidders remain in the portfolio for four months or 36 months (interim results are omitted for brevity). Numbers appearing below point estimates are t-statistics based on Bollerslev-Wooldridge [10] standard errors.

Overall

Horizon	1963-1970	1971-1980	1981-1990	post-1990	Overall
4	1.50	3.15	2.87	2.84	2.73
	9.80	20.99	23.39	11.60	28.75
36	0.41	0.87	0.52	1.01	0.70
	4.76	8.53	7.51	7.73	12.90

Post-Merger

Horizon	1963-1970	1971-1980	1981-1990	post-1990	Overall
4	-0.22	0.04	0.10	0.35	0.07
	-1.06	0.22	0.72	1.43	0.67
36	-0.22	-0.20	-0.28	0.07	-0.15
	-2.72	-1.43	-3.88	1.08	-2.55

Mode of Payment: Cash

Horizon	1963-1970	1971-1980	1981-1990	post-1990	Overall
4	(from 7301)	4.72	4.08	3.53	4.28
		15.70	17.02	18.46	22.52
36	(from 7301)	1.59	0.86	1.08	1.36
		16.66	8.41	9.65	13.63

Mode of Payment: Stock

Horizon	1963-1970	1971-1980	1981-1990	post-1990	Overall
4	(from 7301)	2.55	1.94	2.60	2.27
		12.44	8.74	11.72	20.09
36	(from 7301)	0.41	0.09	1.06	0.47
		3.16	0.86	7.40	5.30

Within Industry					
Horizon	1963-1970	1971-1980	1981-1990	post-1990	Overall
4	1.63	3.10	2.70	2.74	2.61
	5.01	12.25	30.37	20.48	19.26
36	0.19	0.78	0.33	1.00	0.56
	1.27	8.89	3.17	11.21	7.68
Across Industry					
Horizon	1963-1970	1971-1980	1981-1990	post-1990	Overall
4	1.74	3.22	2.86	2.72	2.80
	16.16	15.95	16.11	12.64	21.83
36	0.63	0.98	0.52	1.01	0.76
	9.32	8.23	9.97	11.43	12.29