MANAGERIAL DECISIONS AND LONG-TERM STOCK PRICE PERFORMANCE

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ABSTRACT:

A rapidly growing literature claims to reject the efficient market hypothesis by producing large estimates of long-term abnormal returns following major corporate events. The preferred methodology in this literature is to calculate average multi-year buy-and-hold abnormal returns and conduct inferences via a bootstrapping procedure. We show that this methodology is severely flawed because it assumes independence of multi-year abnormal returns for event firms, producing test statistics that are up to four times too large. After accounting for the positive cross-correlations of event firm abnormal returns we find virtually no evidence of reliable abnormal performance for our samples.

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How reliable are estimates of long-term abnormal returns subsequent to major corporate events? The view expressed in recent long-term event studies is that we can precisely identify systematic mis-pricings of large samples of equity securities for up to five years following major corporate decisions. Taken at face value, these findings strongly reject the notion of stock market efficiency. However, this is at odds with the conventional view that the stock market quickly and completely incorporates public information into the stock price. We try to reconcile these two views by reassessing the reliability of recent long-term abnormal return estimates and providing new estimates that are robust to common statistical problems. In particular, we investigate the impact on inferences of several potential, but often overlooked problems with common methodologies using three large well-studied samples of major managerial decisions, namely, mergers, seasoned equity offerings (SEOs), and share repurchases, all of which have been the focus of numerous long-term event studies, and each of which have the benefit of a long sample period, 1958-1993.

There are several important components to measuring long-term abnormal stock price performance, including an estimator of abnormal performance and a means for determining the distribution of the estimator. Beginning with Ritter (1991), the most popular estimator of long-term abnormal performance is the mean buy-and-hold abnormal return, $\overline{BHAR}$. Concerns arising from the skewness of individual firm long horizon abnormal returns hampered statistical inference in many initial studies, which either avoided formal statistical inference or relied on assumptions that were later questioned, such as normality of the estimator. To address the skewness problem, Ikenberry, Lakonishok, and Vermaelen (1995) introduce a bootstrapping procedure for statistical inference, which simulates an empirical null distribution of the estimator, relaxing the assumption of normality.

Several long-term event study methodology papers have questioned each aspect of measuring long-term abnormal performance. Barber and Lyon (1997) argue that the $\overline{BHAR}$ is the appropriate estimator because it "precisely measures investor experience." However, Barber and Lyon (1997) and Kothari and Warner (1997) provide simulation evidence showing that common estimation procedures can produce biased $\overline{BHAR}$ estimates. In particular, biases arise from new listings, rebalancing of benchmark portfolios, and skewness of multi-year abnormal returns. Proposed corrections include careful construction of benchmark portfolios to eliminate known biases and conducting inferences via a bootstrapping procedure as applied by Ikenberry, Lakonishok, and Vermaelen (1995). In addition, large sample sizes mitigate many of these biases. The common conclusion of these methodology papers is that "measuring long-term abnormal performance is treacherous."

Fama (1998) argues against the *BHAR* methodology because the systematic errors that arise with imperfect expected return proxies—the bad model problem—are compounded with long-horizon returns. In addition, any methodology that ignores cross-sectional dependence of event firm abnormal returns that are overlapping in calendar-time is likely to produce overstated test statistics. Therefore, Fama strongly advocates a monthly calendar-time portfolio approach for measuring long-term abnormal performance. First, monthly returns are less susceptible to the bad model problem. Second, by forming monthly calendar-time portfolios, all cross-correlations of event-firm abnormal returns are automatically accounted for in the portfolio variance. Finally, the distribution of this estimator is better approximated by the normal distribution, allowing for classical statistical inference.

Despite the apparent attractiveness of the calendar-time portfolio approach, Lyon, Barber, and Tsai (1999) and Loughran and Ritter (1999) prefer the *BHAR* methodology. Lyon, Barber, and Tsai again argue that the $\overline{BHAR}$ is the appropriate estimator because it "accurately represents investor experience," and that statistical inference should be performed via either a bootstrapped skewness-adjusted *t*-statistic or the bootstrapping procedure of Ikenberry, Lakonishok, and Vermaelen (1995). Loughran and Ritter primarily argue that the calendar-time portfolio approach has low power to detect abnormal performance because it averages over months of "hot" and "cold" event activity. For example, the calendar-time portfolio approach may fail to measure significant abnormal returns if abnormal performance primarily exists in months of heavy event activity.

Following the prescriptions of the methodology papers described above; there is a second wave of long-term event studies also finding large estimates of abnormal performance. The typical approach is to focus on *BHAR*s, using various benchmarks that are carefully constructed to avoid known biases, and assessing statistical significance of the $\overline{BHAR}$ via a bootstrapping procedure. The authors conclude that since they find evidence of long-term abnormal performance even after taking into account the potential problems highlighted by the methodology papers, their results are especially robust. There is some sense that the recent estimates of abnormal performance are perhaps conservative, but since they are still very statistically significant, market efficiency is strongly rejected.

The general findings from the long-term abnormal performance studies can be described by Figure 1. Typically, the estimated mean buy-and-hold abnormal return, $\overline{BHAR}$, falls far into the tail of the null distribution of the $\overline{BHAR}$, and often falls well beyond the maximum, or below the minimum, of the bootstrapped null distribution. The methodology papers emphasize that the benchmarks must be carefully constructed to avoid known biases, which can move the $\overline{BHAR}$ in either direction. However, this has little impact on inferences in practice for large samples. For our samples, different methods of constructing benchmark portfolios change estimated mean *BHAR*s roughly 20% in either direction. In other words, if the mean *BHAR* is -10%, modifying the benchmark construction produces estimates ranging from -8% to -12%. Although this may appear significant, this is not a meaningful difference since the minimum of the bootstrapped null distribution is only around -4%, producing a *p*-value of 0.000 regardless of which estimate is used. The common inference from results similar to these is that average *BHAR*s following major corporate events are very far from zero, and thus market efficiency is rejected.

We are suspicious of this interpretation. It is difficult to reconcile extremely precise measurement of abnormal performance when expected performance is difficult to determine, *a priori*. Our view of Figure 1 is that the popular bootstrapped distribution of three-year $\overline{BHAR}$s may be too tight given that we cannot price equity securities very precisely, especially at long horizons, and thus, it is not clear whether the $\overline{BHAR}$ is far from zero or not.

Our primary concern with the advocated bootstrapping procedure is that it assumes event firm abnormal returns are independent. Major corporate actions are not random events, and thus event samples are unlikely to consist of independent observations. In particular, major corporate events cluster through time by industry. This leads to positive cross-correlation of abnormal returns making test statistics that assume independence, severely overstated. We are more comfortable assuming that the mean *BHAR* is normally distributed with large sample sizes than we are assuming that all multi-year event firm abnormal returns are independent. The dependence problem increases with sample size, whereas the normality assumption becomes more plausible with large samples.

To gain perspective on the magnitude of the dependence problem, we assume normality of the mean *BHAR* and impose a simple structure on the covariance matrix to estimate average cross-correlations of three-year *BHAR*s. The estimated correlations are used to calculate correlation-adjusted standard errors of the $\overline{BHAR}$ for each of the three samples. We find that the normality assumption for the mean *BHAR* is reasonable for our three large event samples. However, accounting for dependence has a huge effect on inferences. It is common for *t*-statistics to fall from over 6.0 to less than 1.5 after accounting for cross-correlations. Equivalently, we find that a three-year $\overline{BHAR}$ of 15% is not statistically different from zero. In fact, after accounting for the positive cross-correlations of individual firm *BHAR*s, we find no reliable evidence of long-term abnormal performance for any of the three event samples using the *BHAR* approach. Importantly, this result is due to accounting for the dependence of individual event firm abnormal returns, not due to the construction of the benchmark portfolios. Although our estimated returns are similar in magnitude to those reported in previous studies, our test statistics that allow for dependence are dramatically smaller.

Our results directly contradict the prescriptions of most methodology papers that advocate the *BHAR* approach in conjunction with bootstrapping. On the other hand, the calendar-time portfolio approach advocated by Fama (1998) is robust to the most serious statistical problems. Interestingly, the inferences from the calendar-time portfolio approach are quite similar to those from our modified *BHAR* analysis after accounting for the positive cross-sectional dependence of event firm abnormal returns. Moreover, we directly address the concerns raised by Loughran and Ritter (1999), and find no evidence in their support. In fact, we find that the calendar-time portfolio procedure has *more* power to identify reliable evidence of abnormal performance in our samples than the *BHAR* approach after accounting for dependence. We, like Fama (1998), strongly advocate a calendar-time portfolio approach.

Contemporaneous research by Brav (1999) also emphasizes the problems of long-term abnormal performance methodologies that assume independence. In particular, Brav develops an elaborate Bayesian predictive methodology for measuring long-term abnormal returns, which relaxes the assumption of independence in certain circumstances. However, his approach does not provide a complete correction to the dependence problem.

Finally, we show that much of what is typically attributed to an "event," is merely a manifestation of known mispricings of the model of expected returns. Following Fama and French (1992, 1993), virtually all recent studies documenting long-term abnormal returns use some form of risk-adjustment that assumes the cross-section of expected returns can be completely described by size and book-to-market equity. However, this appears to be a mis-reading of the evidence presented in Fama and French (1993). Although expected returns are systematically related to size and book-to-market attributes, Fama and French point out that three of twenty-five portfolios formed based on these characteristics are associated with abnormal return estimates that are significantly different from zero. In other words, the model of market equilibrium used to identify mis-pricing cannot completely price the cross-section of expected returns on the dimensions that it is designed to explain. After controlling for the sample composition of our three samples, based on size and book-to-market attributes, reliable evidence of abnormal performance is substantially reduced, and is restricted to a few sub-samples of small stocks. This is consistent with the evidence provided by Brav and Gompers (1997) for IPOs and suggests that many of the "event anomalies" previously documented by other researchers are actually manifestations of known pricing deficiencies of the model of expected returns. This further highlights the inconsistency of being able to reliably identify mis-priced assets when asset pricing is imprecise.

I. DATA DESCRIPTION

A. Sample Selection

The datasets for this paper consist of three large samples of major managerial decisions, namely mergers, SEOs, and share repurchases, completed during 1958-1993.[1] Each of these event samples has been the focus of numerous recent studies of long-term stock price performance, although data from the 1960s is for the most part unexplored. The use of three large well-explored samples facilitates comparisons of abnormal performance estimates across both our samples and those of other studies, as well as across various methodologies. In addition, the use of data from the 1960s may be useful in determining how sensitive results are to short sample periods.

The sample consists of 4,911 underwritten primary and combination seasoned equity offerings (SEOs); 2,421 open market and tender-offer share repurchases (excludes odd-lot repurchases); and 2,193 acquisitions of CRSP firms. We exclude multiple events by the same firm within any three-year period. In other words, after the first event, we ignore additional events until after the three-year event window. Our objective is to focus on the long-run price performance of the broad samples rather than to delve into the cross-sectional particulars, so we limit the cross-sectional characteristics of individual transactions to those that previous research has shown to be important. For example, we classify merger transactions based on form of

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[1] The event samples are from the CRSP-EVENTS database currently under development at the University of Chicago's Center for Research in Security Prices. The CRSP-EVENTS database contains detailed information on mergers & tender offers, primary seasoned equity issues, and stock repurchases from 1958 to present, where CRSP price data is available at the time of the event announcement. Data sources include corporate annual reports, *Investment Dealer's Digest*, *Mergers & Acquisitions*, U.S. Securities & Exchange Commission filings, Standard & Poor's Compustat database, *The Wall Street Journal* (and Dow Jones News Retrieval Service), and various miscellaneous sources.

payment. For repurchase events we distinguish between open market repurchases and self-tenders, similar to prior research.

B. Announcement and Completion Dates

In the long-run abnormal performance tests, we begin the multi-year event window at the end of the completion month rather than the announcement date. For example, the announcement date for SEOs is the registration date, whereas the completion date is the offering date. The average time between the registration and completion date is one month. For mergers, the interval between announcement and completion is typically several months. Since stock repurchase programs rarely provide a definitive completion date and because these programs can take place over a period of a year or two, we treat the announcement date as the completion date. In the empirical tests examining the pre-event long-term abnormal performance, the event window ends in the month before the event announcement.

C. Returns, Size, and Book-to-Market Equity

The return data come from the CRSP monthly NYSE, AMEX, and Nasdaq stock files.[2] Firm size refers to the market value of common equity at the beginning of the month. In part of the empirical analysis that follows, we assign sample firms to portfolios based on size and book-to-market equity (BE/ME). We follow Fama and French (1997), and define book equity as total shareholder's equity, minus preferred stock, plus deferred taxes (when available), plus investment tax credit (when available), plus post-retirement benefit liabilities (when available). Preferred stock is defined as redemption, liquidation, or carrying value (in this order), depending on availability. If total shareholder's equity is missing, we substitute total assets minus total liabilities. Book-to-market equity is the ratio of fiscal year-end book equity divided by market capitalization of common stock at calendar year-end. We use the most recent fiscal year-end

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[2] Shumway (1997) reports that "performance" related delistings often have missing final returns, which he estimates to be -30%. We find that substituting missing performance related delisting returns with -30% does not alter our findings, and so we report results based on the unaltered data.

book equity, as long as it is no later than the calendar year-end market equity. Consequently, if the fiscal year-end occurs in January through May, we use book equity from the prior fiscal-year.

The event samples begin in 1958, and extend through 1993. Since the book-equity data is limited on Compustat during the early part of the sample period, we fill in much of the missing data by hand-collecting book-equity from Moody's Manuals (Chan, Jegadeesh, and Lakonishok (1995), Kothari, Shanken, and Sloan (1995)). In the fiscal year 1962, we supplement the 974 firms with Compustat book equity data with 769 firms from the Moody's Manuals. In all, we replace about 7,000 missing book-equity observations.

D. Distribution of Event Firms by Size and Book-to-Market

In some of the empirical tests to follow, we compare the stock price performance of the event firms to 25 portfolios formed on size and book-to-market quintiles using NYSE breakpoints (see Fama and French (1992, 1993)). Table 1 displays the distribution of the event firms according to the size and BE/ME classifications.

All three event samples have size distributions that are tilted towards large firms relative to the population of CRSP firms. Close to 60% of all CRSP firms fall into the bottom size quintile based on NYSE breakpoints. Acquirers are more likely to have relatively low BE/ME ratios and large equity values. SEO issuers are also more likely to have low BE/ME ratios, and tend to be small firms relative to the NYSE breakpoints. Share repurchasers are also relatively small firms, and interestingly, more likely to be low BE/ME firms, suggesting that these firms are not typically "value" stocks as some behavioral stories suggest.

The distribution of sample firms does not change very much between the pre- and post-event years. This suggests that there is little systematic change in the firm-level size and book-to-market characteristics following the event. However, this hides the fact that *most* firms change portfolio assignments following the event. In particular, only 25% of our sample firms are in their original size and book-to-market portfolio three years after the event.

II. BUY-AND-HOLD ABNORMAL RETURNS (*BHAR*S)

Buy-and-hold abnormal returns have become the standard method of measuring long-term abnormal returns (see Barber and Lyon (1997) and Lyon, Barber, and Tsai (1999)). Buy-and-hold abnormal returns measure the average multi-year return from a strategy of investing in all firms that complete an event and selling at the end of a pre-specified holding period, versus a comparable strategy using otherwise similar non-event firms.

Barber and Lyon (1997) and Lyon, Barber, Tsai (1999) argue that *BHAR*s are important because they "precisely measure investor experience." While it is true that *BHAR*s capture the investor's experience from buying and holding securities for three to five years, this is not a particularly compelling reason to restrict attention to this methodology if reliably assessing long-term stock price performance is the objective. First, this is only one type of investor experience—the buy-and-hold experience. There are other reasonable trading strategies that capture other investors' experiences, for example, periodic portfolio rebalancing. Second, because of compounding, the buy-and-hold abnormal performance measure is increasing in holding period, given abnormal performance during any portion of the return series. For instance, if abnormal performance exists for only the first six months following an event, and one calculates three- and five-year *BHAR*s, both can be significant, and the five-year *BHAR* will be larger in magnitude than the three-year *BHAR*. This is important to consider since the length of the holding period is arbitrary, and various holding period intervals are often analyzed to determine how long the abnormal performance continues after the event. Finally, and most important, we show in the next section that there are serious statistical problems with *BHAR*s that cannot be easily corrected. Since our objective is to reliably measure abnormal returns, it is imperative that the methodology allows for reliable statistical inferences.

A. *Calculating* BHAR*s*

We calculate three-year *BHAR*s for each firm in the three event samples using 25 value-weight, non-rebalanced, size-BE/ME portfolios as expected return benchmarks[3]

$$BHAR_i = \prod_{t=1}^{T}(1+R_{i,t}) - \prod_{t=1}^{T}(1+R_{benchmark,t}) \qquad (1)$$

where the mean buy-and-hold abnormal return is the weighted average of the individual *BHAR*s:

$$\overline{BHAR} = \sum_{i=1}^{N} w_i \cdot BHAR_i \qquad (2)$$

Both equal-weight (EW) and value-weight (VW) averages are computed, where the value-weights are based on market capitalizations at event completion, divided by the implicit value-weight stock market deflator. In other words, we standardize market values of the sample firms by the level of the CRSP VW market index at each point in time before determining the weights. This is to avoid the obvious problems with unstandardized value-weights, which would weight recent observations much more heavily than early observations. The size-BE/ME portfolio benchmarks are designed to control for the empirical relation between expected returns and these two firm characteristics (see Fama and French (1992, 1993) for discussion and evidence).

The benchmark portfolios exclude event firms, but otherwise include all CRSP firms that can be assigned to a size-BE/ME portfolio. At the end of June of each year *t*, all stocks are allocated to one of five size groups based on market capitalization rankings relative to NYSE quintiles. In an independent sort, all stocks are also allocated to one of five BE/ME groups based on where their BE/ME ranks relative to NYSE quintiles. The returns for the 25 portfolios are calculated for the year defined July of year *t* through June of year *t*+1, as the value-weight average of the individual firm monthly returns in each of the size-BE/ME quintile intersections. To allow for changing firm characteristics, the size-BE/ME benchmarks are allowed to change at the end of June of each year when new portfolio assignments are available. Moreover, the size-

[3] Employing long-term windows of different lengths does not alter the substantive results of this paper.

BE/ME portfolios are composed of firms that have reported prior BE data, which partially mitigate any bias caused by including recent IPOs in the benchmarks.

In calculating the *BHAR*s for the individual firms, we impose two conditions to ensure that all *BHAR*s represent true three-year buy-and-hold returns. First, because of delistings, not all of the sample firms have a full three years of valid return data following the completion of the event. Therefore, we fill-in missing sample firm returns with the benchmark portfolio return. Second, in forming the benchmark portfolios, we do not rebalance, so that each *BHAR* is a true buy-and-hold return. This means that we compute the three-year returns for each of the 25 size-BE/ME portfolios each calendar month.

B. Statistical Inference via Bootstrapping

Since the *BHAR* is the difference of a sample firm's three-year return and the three-year return on a benchmark portfolio, the distribution of individual firm *BHAR*s is strongly positively skewed (Barber and Lyon, 1997) and generally does not have a zero mean. Therefore, statistical inference for the mean *BHAR* is often based on an empirical distribution simulated under the null of the model as applied by Brock, Lakonishok, and LeBaron (1992) and Ikenberry, Lakonishok, and Vermaelen (1995). Within this framework, the implied model of expected three-year returns is simply the average three-year return of firms that have similar size and BE/ME.

Following their methodology, for each sample firm, we assign the completion date to a randomly selected firm with the same size-BE/ME portfolio assignment at the time of the event. This procedure yields a pseudo-sample that has the same size-BE/ME distribution, the same number of observations, and the same calendar time frequency as the original event sample. We then calculate the $\overline{BHAR}$ for this pseudo-sample in the same way as for the original sample. This results in one $\overline{BHAR}$ under the null of the model. We repeat these steps to generate 1,000 $\overline{BHAR}$s, and thus an empirical distribution of the $\overline{BHAR}$ under the null. A *p*-value is calculated as the fraction of the $\overline{BHAR}$s from the pseudo-samples that are larger in magnitude (but with the same sign) than the original $\overline{BHAR}$.

C. Results from the **BHAR** *Analysis Assuming Independence*

Table 2 displays the *BHAR* results for the three event samples over the period July 1961 through December 1993. We report both EW and VW results and compare to previous research when possible.

C.1 Mergers

As displayed in Table 2.a, the three-year EW $\overline{BHAR}$ for acquirers is -1%, and has a *p*-value of 0.164. The wealth relative, measured as the average gross return of the event firms divided by the average gross return of the benchmark firms, is 0.994, implying that investing in these acquirers generated total wealth 0.6% less than a strategy which invested in similar size-BE/ME firms after three years. The VW $\overline{BHAR}$ is -0.038 with a *p*-value of 0.027, and a wealth relative of 0.973.[4]

The closest comparison to our results is with Loughran and Vijh (1997) who study 947 acquisitions during 1970-1989. They report an EW five-year $\overline{BHAR}$ of -6.5%, with a *t*-statistic of -0.96. The most extreme abnormal returns in other papers are usually documented for special groupings of event firms, based on BE/ME rankings or form of payment. Popular groupings based on BE/ME are commonly denoted as "growth" (or glamour), which refers to firms with low BE/ME; and "value," which refers to firms with high BE/ME. The growth and value groupings are important for several recent behavioral theories of stock market over- and underreaction following major corporate decisions. These theories have been interpreted as predicting negative long-term abnormal returns for growth firms and positive abnormal returns for value firms completing major corporate actions. For example, Rau and Vermaelen (1998) document large differentials in performance between glamour and value acquirers. Specifically, they report bias adjusted three-year *CAR*s for glamour acquirers of -17.3% (*t*-statistic = -14.45)

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[4] The pre-event *BHAR*s document superb performance before the event for small acquirers relative to the benchmark portfolios, with an EW average three-year raw return of 88%, an abnormal return of 28.5%, and a wealth relative of 1.18. However, this abnormal performance appears to be stronger for small acquirers, as the VW average pre-event *BHAR* is insignificant.

and value acquirers of 7.6% (t-statistic = 14.23). In direct contrast, when we analyze the stock price performance of growth and value acquirers we find no evidence of reliable differential performance, 1% on an EW basis, representing differential performance several times smaller than the amount previously documented.

Similar to previous research, we find that acquirers that use stock to finance the merger perform worse than those that abstain from equity financing. The EW average unadjusted three-year return for acquirers that finance mergers with at least some stock is roughly two-thirds that of acquirers that abstain from stock financing, 39% vs. 63%. The EW $\overline{BHAR}$ for stock acquirers is -8.4% (p-value = 0.000), while the $\overline{BHAR}$ for non-stock acquirers is 6.4% (p-value = 0.047). The VW results are similar, although the differences are much smaller in magnitude. Loughran and Vijh (1997) document a similar pattern in abnormal returns related to financing, albeit their differences are larger by a magnitude of three times. They report a five-year EW $\overline{BHAR}$ of -24.2% (t-statistic = -2.92) for stock mergers and 18.5% (t-statistic = 1.27) for cash mergers.

C.2 Seasoned Equity Offerings (SEOs)

Table 2.b reports the *BHAR* results for SEOs. The EW average three-year return for the SEO sample is 35%, while the average size-BE/ME three-year return is 45%, producing a $\overline{BHAR}$ of -10% with a p-value of 0.000. The EW $\overline{BHAR}$ of -10% is 2.5 times smaller than the minimum of the empirical distribution which is only -4.1%. In other words, not a single one of the 1,000 pseudo-sample $\overline{BHAR}$s even comes close to the -10% EW $\overline{BHAR}$, which is typical of many of the $\overline{BHAR}$s reported. The VW $\overline{BHAR}$ is -4.2%, with a p-value of 0.165.[5] To facilitate comparison to other studies, we also report results after excluding utilities. These results are virtually identical to the full sample results.

[5] During the three-year pre-event period, small issuers experience enormous average returns. The EW average unadjusted pre-event buy-and-hold return is 144%, corresponding to an abnormal three-year return of 75%. These extremely large pre-event returns appear strongest for small stocks, as the VW *BHAR* is slightly negative, although insignificant.

Overall, our results are similar to other research. Spiess and Affleck-Graves (1995) and Brav, Geczy, and Gompers (1999) both report EW three-year raw returns of 34% and 32%, respectively, whereas the average benchmark returns are on the order of 57% and 44%, respectively. Brav, Geczy, and Gompers also document that the abnormal performance is largely confined to small low BE/ME firms, and is substantially reduced with value-weighting.

C.3 Share Repurchases

The EW $\overline{BHAR}$ for repurchasers is 14.5% with *p*-value = 0.000 (Table 2.c). Again, not a single one of the 1,000 pseudo-sample $\overline{BHAR}$ even comes close to the 14.5% EW $\overline{BHAR}$. These results are virtually identical to those reported by Ikenberry, Lakonishok, and Vermaelen (1995) for repurchase programs announced during 1980-1990. The abnormal performance is considerably reduced with value-weighting, VW $\overline{BHAR}$ equals 6.6% (*p*-value = 0.227).[6]

The EW $\overline{BHAR}$ is larger for firms repurchasing their shares on the open market (15.6%, *p*-value=0.000), rather than through tender offers (8.7%, *p*-value=0.228). This is interesting in light of a tender offer being a more dramatic event in the life of a firm. In addition, the abnormal returns are twice as large for value firms than for growth firms (24.4% versus 9.9%), similar to Ikenberry, Lakonishok, and Vermaelen (1995). But, the VW $\overline{BHAR}$s are not reliably different from zero for the sub-samples.

III. ASSESSING THE STATISTICAL RELIABILITY OF *BHAR*S

As described in Section II, it is common to simulate an empirical distribution in order to perform statistical inference of the $\overline{BHAR}$s since the individual *BHAR*s have poor statistical properties, producing biased test statistics in random samples [see Barber and Lyon (1997), Kothari and Warner (1997), and Lyon, Barber, and Tsai (1999)]. Most researchers view this bootstrapping as a robust solution to the known statistical problems associated with the *BHAR*

[6] One misconception about firms that repurchase their shares is that they perform poorly before the event. The pre-event abnormal performance is insignificant on both an EW and VW basis.

methodology. In this section, we question the robustness of the bootstrapping procedure with respect to statistical inference for event samples.

The empirical distribution is simulated under the null hypothesis assuming (1) the 25 size-BE/ME benchmark portfolios completely describe expected returns, and (2) the randomly selected firms used to construct the empirical distribution have the same covariance structure as the sample firms. Fama (1998) details the problems associated with the first assumption as the "bad model" problem, arguing that "all models for expected returns are incomplete descriptions of the systematic patterns in average returns during any sample period." In other words, if the model for expected returns does not fully explain stock returns, measured abnormal performance is likely to exist with respect to event samples exhibiting common characteristics. The best means of checking the robustness of our results with respect to this assumption is to repeat the analysis with a different model of expected returns and a different methodology.

We focus on the second assumption, which is crucial to the statistical reliability of *BHAR*s and is unique to the manner in which the empirical distribution is constructed. The bootstrapping procedure makes two implicit assumptions: (1) the residual variances of sample firms are no different from randomly selected firms and (2) the observations are independent. The first assumption may pose a problem if the sample firms' returns are more or less volatile than the firms that are used to create the pseudo-samples. Although on average, the $\overline{BHAR}$ will be correct, the empirical distribution may be too "tight," leading to an overstatement of significance (see Brav (1999)). The second assumption may be problematic if the events themselves are driven by some underlying factor not captured by size and BE/ME. Andrade and Stafford (1999) show that mergers (from the acquirer's perspective) tend to cluster in calendar-time by industry. Similarly, Mitchell and Mulherin (1996) identify fundamental industry shocks that lead to increased takeover activity at the industry level, and Comment and Schwert (1995) do the same at the aggregate level. Ritter (1991) states that IPOs cluster by industry at given points in time. It is also likely that SEOs and share repurchases cluster by industry. If the event clustering leads to positively correlated individual *BHAR*s, statistical significance will be overstated by any methodology that assumes independence.

We have some reason to be concerned that the *p*-values reported in Table 2 are overstated because we find strong statistical significance for economically small estimates. For example, the VW $\overline{BHAR}$ for acquirers is -3.8% with a *p*-value of 0.027, and a wealth relative of 0.973. In other words, the average three-year investment in acquiring firms generated 2.7% less wealth than an otherwise similar investment in non-acquirers, on a value-weight basis. In economic terms, this does not seem significant, but the test statistic suggests that this represents statistically significant long-term mispricing. In addition, we find $\overline{BHAR}$s 2.5 times larger (in magnitude) than the extreme of the empirical distribution. For example, the EW $\overline{BHAR}$ for SEOs is -10.2%, whereas the minimum of the empirical distribution is only -4.1%. Assuming normality of the empirical distribution of the mean *BHAR*, this corresponds to a *p*-value less than 0.000000001.

A. Properties of the Empirical Distribution of the Mean BHAR

Figure 1 plots the simulated empirical null distribution of the EW $\overline{BHAR}$ for the SEO sample, as described above. The plots reveal that the distributions are quite symmetric and reasonably well approximated by the normal density superimposed on the graphs. Because of the large sample size, the mean should be close to normal regardless of the underlying distribution of the individual firm *BHAR*s.[7] However, the Jarque-Bera test statistic rejects normality of both the EW and VW empirical distributions. Although the empirical distributions are not normal, it is interesting to see how poor an assumption normality is. Table 3 reports various critical values based on the empirical distributions for the three event samples, and compares them to the critical values assuming normality. The critical values assuming normality are calculated based on the mean and standard deviation of the empirical distribution. For the most part, assuming

[7] The Central Limit Theorem guarantees that a standardized sum of random variables will converge to a Normal(0,1) random variable, even if the individual random variables are correlated (see Chung (1974) for a discussion).

normality seems reasonable, and inferences would be unaffected at either the 1% or 5% levels for all three samples, both EW and VW.

We also create a bootstrapped distribution of the $\overline{BHAR}$ (not reported) for the three samples by re-sampling from the event-firm *BHAR*s themselves (see Efron and Tibshirani, 1993). This again assumes that the events are independent, but the bootstrapping makes no assumptions about the event-firm residual variances relative to randomly selected firms, as re-sampling is done using the original *BHAR* data. This allows us to isolate the effect of differential residual variance on inferences via the empirical distribution. Comparison of the empirical and the bootstrapped distributions reveals no noticeable difference in dispersion, suggesting that increased residual variance of event firms is not a serious problem with these three samples.[8]

B. Cross-Sectional Dependence of **BHAR***s*

Our primary statistical concern is that major corporate actions are not random events, and thus may not represent independent observations. The very nature of an event sample is that all of these firms have chosen to participate in an event, while other firms have chosen not to. As indicated above, major corporate events cluster through time by industry. This may lead to cross-correlation of abnormal returns, which could flaw inferences from methodologies that assume independence. There is an extensive accounting literature documenting cross-sectional dependence of individual firm residuals (see Bernard, 1987; Collins and Dent, 1984; and Sefcik and Thompson, 1986). These studies find that contemporaneous market model residuals for individual firms are significantly correlated, on the order of 18% within individual industries. Since major corporate events cluster in certain industries at any given point in time; correlated residuals will pose a significant problem for the *BHAR* methodology, which assumes independence of all observations, including those that are overlapping in calendar time.[9]

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[8] In general, this may be a major problem. For example, Brav (1999) documents that IPO firms have significantly larger residual variances than otherwise similar non-IPO firms.

[9] Note that overlapping observations on the same firm are excluded from the samples. The overlapping observations that are important for this analysis are those of similar firms, such as those in the same industry.

To gain perspective on the magnitude of this problem, we calculate average pairwise correlations of monthly and annual *BHAR*s for each of our three event samples where there is perfect calendar-time overlap. In other words, all possible unique correlations are calculated using five years of either monthly or annual abnormal return data for firms that complete events in the same month. The grand average is displayed below.

Average Pairwise Correlations of *BHAR*s with Complete Calendar-Time Overlap

Frequency	Mergers	SEOs	Repurchases
Monthly	0.0020	0.0177	0.0085
Annual	0.0175	0.0258	0.0175

The average correlations increase substantially with the interval for all three event samples, which is consistent with previous research. For example, Bernard (1987) finds that average intra-industry correlations in individual firm market model residuals increase with holding period, nearly doubling from 0.18 to 0.30 when the interval is increased from monthly to annual.

Although the average correlations appear small, they can have a significant impact on inferences with large samples. This can be seen by inspecting the formulas for the sample standard deviation, equation (3), and the ratio of the standard deviation assuming independence to the standard deviation accounting for dependence, equation (4).[10]

$$\sigma_{BHAR} = \sqrt{\frac{1}{N} \cdot \overline{\sigma_i^2} + \frac{(N-1)}{N} \cdot \overline{\rho_{i,j}\sigma_i\sigma_j}} \tag{3}$$

$$\frac{\sigma_{BHAR}(Independence)}{\sigma_{BHAR}(Dependence)} \approx \frac{1}{\sqrt{1+(N-1)\overline{\rho_{i,j}}}} \tag{4}$$

where: $N =$ number of sample events, $\overline{\sigma_i^2} =$ average variance of individual *BHAR*s, $\overline{\rho_{i,j}\sigma_i\sigma_j} =$ average covariance of individual *BHAR*s, and $\overline{\rho_{i,j}} =$ average correlation of individual *BHAR*s. In large samples with positive cross-correlations, the covariance term comes to dominate the individual variances. As such, ignoring cross-correlations will lead to overstated test-statistics.

[10] The approximation of the ratio of σ*(Independence)* to σ*(Dependence)* assumes equal variances of the individual *BHAR*s.

To determine the severity of overstated test-statistics for our event samples, we calculate "corrected" *t*-statistics that account for dependence in *BHAR*s under various assumptions about the average correlation of three-year *BHAR*s and the covariance structure. We report results only for the EW test-statistics because EW results are the largest and tend to be the focus of most previous research. We assume that the average correlation for overlapping observations is linear in the number of months of calendar-time overlap, ranging from 0.0 for non-overlapping observations to the estimated average correlation of three-year *BHAR*s of firms with complete overlap. Table A.1 in the appendix displays the assumed covariance structure for the SEO sample. It is difficult to directly estimate average correlations of three-year *BHAR*s because of limited data. Therefore, we report a range of estimates and show the impact on *t*-statistics over this range. Table 4 displays the results.

First, we should point out that we are assuming that the empirical distribution is normal, which although not technically true, appears to be a reasonable approximation. This assumption allows us to calculate *t*-statistics for the $\overline{BHAR}$s using the mean and standard deviation from the empirical distribution. We are also able to calculate *t*-statistics using standard deviations that account for cross-correlations. Second, because the average correlation appears to be increasing in holding period, it is unlikely that the average correlation of three-year *BHAR*s is less than the average correlation from the annual *BHAR*s. Therefore, the *t*-statistics that assume that the average correlation of three-year *BHAR*s is equal to our estimate of average correlations from annual *BHAR*s are still likely to be overstated.

The corrected *t*-statistics reveal that there is no statistical evidence of abnormal returns for any of the three event samples. The massive *t*-statistics of -6.05 (SEOs) and 4.86 (repurchases) that assume independence fall to -1.49 and 1.91, respectively, after accounting for the positive cross-correlations of individual *BHAR*s. Since the average correlation of three-year *BHAR*s is almost surely larger than that of annual *BHAR*s, the *t*-statistics that assume average correlations of 0.02 for mergers and repurchases and 0.03 for SEOs are probably more reflective of the true level of significance.

C. The Bottom Line on **BHAR***s*

The literature on long-term stock price performance heavily emphasizes results from the *BHAR* methodology despite well-known potential problems. Barber and Lyon (1997), Kothari and Warner (1997), and Lyon, Barber and Tsai (1999) provide simulation evidence showing that $\overline{BHAR}$ estimates can be biased because of poor statistical properties of individual firm *BHAR*s. Many of these biases are mitigated with large sample sizes and careful construction of benchmark portfolios. However, the problems associated with standard error estimates for *BHAR*s on non-random samples cannot easily be corrected, and are generally increasing in sample size. This point is often missed in methodology papers and dismissed in long-term event studies, which frequently claim that bootstrapping solves all dependence problems. However, this is not true. Event samples are clearly different from random samples. Event firms have chosen to participate in a major corporate action, while non-event firms have chosen to abstain from the action. An empirical distribution created by randomly selecting firms with similar size-BE/ME characteristics does not replicate the covariance structure underlying the original event sample. In fact, the typical bootstrapping approach does not even capture the cross-sectional correlation structure related to industry effects documented by Bernard (1987), Brav (1999) and others. Moreover, Bernard shows that the average *inter*-industry cross-sectional correlation of individual abnormal returns is also positive, suggesting that dependence corrections concentrating only on industry effects will not account for all cross-correlations.

Our results suggest that the popular *BHAR* methodology, in its traditional form, should not be used for statistical inference. Some type of correction for positive cross-correlations of individual event firm *BHAR*s should be made. Bootstrapping may be useful for determining the mean of the null distribution. Finally, it is worth noting that for our three major events there is no statistical evidence of long-term abnormal returns after accounting for positive cross-correlations of individual event firm *BHAR*s.

V. CALENDAR-TIME PORTFOLIO APPROACH

An alternative approach to measuring long-term stock price performance is to track the performance of an event portfolio in calendar-time relative to either an explicit asset pricing model or some other benchmark. The calendar-time portfolio approach was first used by Jaffe (1974) and Mandelker (1974), and is strongly advocated by Fama (1998). The event portfolio is formed each period to include all companies that have completed the event within the prior *n* periods. By forming event portfolios, the cross-sectional correlations of the individual event firm returns are automatically accounted for in the portfolio variance, at each point in calendar-time. In light of our strong evidence that the individual event firm abnormal returns are cross-sectionally correlated, calendar-time portfolios represent an important improvement over the traditional *BHAR* methodology, which assumes independence of individual firm abnormal returns.

A. Calculating Calendar-Time Abnormal Returns

Each month from July 1961 to December 1993, we form EW and VW portfolios of all sample firms that participated in the event within the previous three-years.[11] Portfolios are rebalanced monthly to drop all companies that reach the end of their three-year period and add all companies that have just executed a transaction. The portfolio excess returns are regressed on the three Fama and French (FF) (1993) factors as in equation (5):

$$R_{p,t} - R_{f,t} = a_p + b_p (R_{m,t} - R_{f,t}) + s_p SMB_t + h_p HML_t + e_{p,t} \tag{5}$$

The three factors are zero-investment portfolios representing the excess return of the market; the difference between a portfolio of "small" stocks and "big" stocks, SMB; and the difference between a portfolio of "high" BE/ME stocks and "low" BE/ME stocks, HML. Within this framework, the intercept, a_p, measures the average monthly abnormal return on the portfolio of event firms, which is zero under the null of no abnormal performance, *given* the model. If the

[11] We exclude multiple observations on the same firm that occur within three-years of the initial observation.

FF model provides a complete description of expected returns, then the intercept measures mispricing. However, if the model provides only an imperfect description of expected returns, then the intercept represents the combined effects of mispricing and model misspecification. This is what Fama (1970) refers to as the joint test problem—tests of market efficiency are necessarily joint tests of market efficiency and the assumed model of expected returns.

Table 5 reports the intercepts from regressions of the 25 EW and VW size-BE/ME portfolios on the FF three-factor model. These are the original assets used in Fama and French (1993) to test the model. As pointed out by Fama and French, the FF three-factor model is unable to completely describe the cross-section of expected returns, even on the dimensions on which it is based, which is illustrated by the several significant intercepts in Table 5. This suggests that the null hypothesis—intercept equals zero—may be problematic for samples tilted towards characteristics that the model cannot price in the first place. This can be seen most easily with IPOs. IPO firms are overwhelmingly small low BE/ME firms. When the abnormal returns of IPO firms are estimated with the FF three-factor model, the estimates are on the order of -12% to -15% for an EW portfolio over a three year period, or about -0.35% to -0.42% per month. However, Brav and Gompers (1997) argue that the underperformance of IPOs is not an IPO effect, *per se*. They find that similar size and book-to-market firms that have not issued equity perform as poorly as IPOs. Note that the intercept for all small, low BE/ME firms reported in Table 5 is -0.37, which is essentially identical to that found for IPOs.

In order to gain perspective on whether the known pricing deficiencies of the FF three-factor model affect the three samples studied in this paper, we decompose the intercepts into two components: (1) the *expected* abnormal performance, given the sample composition (based on size-BE/ME portfolio assignment and calendar-time frequency); and (2) the amount of abnormal performance attributable to other sources, including the event.

In particular, we estimate the expected intercept, conditional on the sample composition, as the mean intercept from 1,000 calendar-time portfolio regressions of random samples of otherwise similar non-event firms. This is directly comparable to the empirical distribution used in the *BHAR* analysis. However, we are using this methodology to determine the mean of the null distribution, not to measure the dispersion. Each of the 1,000 random samples has the same calendar-time frequency, and at each point in time, the portfolio of randomly selected firms has the same size-BE/ME composition as the corresponding event-portfolio. A new *t*-statistic is calculated using the expected intercept, $\hat{a}_0$, as the null, and the original intercept and standard error estimates. We refer to the difference between the estimated intercept and the expected intercept as the "adjusted intercept."

$$t = \frac{\hat{a} - \hat{a}_0}{\hat{s}} \tag{6}$$

B. Calendar-Time Portfolio Regression Results

Table 6 displays the EW and VW calendar-time portfolio regression results for the three samples over the period July 1961 through December 1993. The number of monthly observations varies slightly for different samples and sub-samples, as we require a minimum of 10 firms in each monthly event portfolio.

The EW three-year acquirer portfolio exhibits statistically significant average abnormal returns: -0.20% per month, or -7.2% after three years (-0.20% x 36 months), with a *t*-statistic of -3.70 (Table 6.a). When the intercept is adjusted to control for the size and BE/ME characteristics of the sample, the abnormal performance is lower. The adjusted intercept is -0.14%, which corresponds to a three-year abnormal return of -5% (*t*-statistic = -2.61). The intercept from the VW regression is not significant at -0.03 (*t*-statistic = -0.48), translating into a three year average abnormal return of only -1.1%. The adjusted intercept for the VW acquirer portfolio is virtually identical. Since the abnormal returns are only significant when the event firms receive equal weight in the portfolio, it appears that small acquirers are more prone to underperformance in the post-event period. This finding is similar to that previously documented by Brav and Gompers (1997) and Brav, Geczy, and Gompers (1999) with equity issuers.

With respect to portfolios composed of firms in the lowest BE/ME quintile (growth or glamour firms), EW acquirer portfolios have significantly negative intercepts of -0.37 (*t*-statistic = -3.64), corresponding to three-year average abnormal returns of -13.3%. However, this does not appear to be entirely a "merger effect," as the adjusted intercept is only -0.18% (-6.5% over three years), with a *t*-statistic of -1.76. Acquirers with high BE/ME (value firms) have an EW portfolio intercept of 0.00 and an adjusted intercept of -0.08, suggesting that these firms are fairly priced during the post-event period. Again, when the firms are value-weighted, the intercepts are statistically insignificant for both the growth and value portfolios.[12]

Like other researchers, we find that acquirer underperformance is limited to those firms that use at least some stock to finance the acquisition. The EW stock financed acquirer portfolio intercept is -0.33 (*t*-statistic = -4.64), versus -0.09 (*t*-statistic = -1.14) for the EW no stock portfolio. The differential performance based on the type of financing survives value-weighting. The *t*-statistic for the difference between the stock financed and the non-stock financed adjusted intercepts is 2.06, although neither adjusted intercept is significant on its own.

The conclusion from the acquirer regressions is that, on an EW basis, acquirers tend to significantly underperform in the three years following the acquisition, but that this appears to be limited to those acquirers using stock financing. On a VW basis, there is virtually no evidence of acquirer stock price underperformance. This can be due to either the VW regressions having low power to detect abnormal performance, or because the larger firms actually do not underperform. We try to distinguish between these two scenarios in the subsequent section, but the high R-squares (generally over 0.90) hint that these regressions indeed have considerable power. Moreover, the VW adjusted intercept estimates are small in economic terms, ranging from -0.20 to 0.15, which is consistent with these firms being fairly priced on average.

[12] Acquiring firms have significantly positive average monthly abnormal returns in the three-year period prior to the merger, on both an equal- and value-weight basis (17.6% and 6.8% over the three-year period, respectively).

Table 6.b displays the calendar-time SEO portfolio regression results. The EW issuer portfolio has significantly negative abnormal returns in the three years following the equity issue, averaging -0.33% per month or -12% over three years (*t*-statistic = -5.19). Again, not all of the measured abnormal performance is attributable to the equity issue event, as the adjusted intercept is -0.22, or -7.9% over three years (*t*-statistic = -3.51).[13] Although utilities account for a sizeable fraction of the equity issue sample, the measured underperformance is essentially unchanged when we exclude utilities. It appears that the underperformance of the seasoned equity issuers is confined to the EW value portfolio. When the equity issuer event portfolio is value-weighted, there is virtually no evidence of underperformance, for the full sample or for the various sub-samples.

The full sample repurchase portfolios show no signs of abnormal performance on either an EW or VW basis.[14] The EW post-repurchase average abnormal return is 0.08% per month—less than 3% after three years—and less than half this large for the VW portfolio. On an EW basis, there is strong support for the notion that value repurchasers outperform their expected return benchmark. The EW portfolio adjusted intercept is 0.48% per month, or 17.3% after three years, largely consistent with Ikenberry, Lakonishok, and Vermaelen (1995). However, this relation disappears when the repurchase firms are value-weighted.

C. Robustness of the Calendar-Time Portfolio Regressions

While the calendar-time portfolio approach solves the dependence problem associated with event-time abnormal performance measures, it has several potential problems that should be addressed. First, the regressions assume that the factor loadings are constant through time, up to 390 months, which is unlikely since the composition of the event portfolio changes each month. These events tend to cluster through time by industry, and different industries have different

[13] On an EW basis, issuing firms have significantly positive abnormal returns in the three years prior to issuing equity, averaging 1.19% per month or 42.8% over three years (*t*-statistic = 14.05), while the VW issue portfolios show no evidence of significant abnormal returns in the pre-issue period.

[14] There is no evidence of abnormal performance prior to the stock repurchase.

factor loadings (Fama and French, 1997). The portfolio composition is likely heavily weighted in a few industries at each point in time, but different industries at longer intervals. This may lead to biased estimates. Second, the changing portfolio composition may introduce heteroskedasticity, as the variance is related to the number of firms in the portfolio. This may cause the OLS estimator to be inefficient, but will not lead to biased estimates. A third concern of this procedure is that the calendar-time portfolio approach weights each month equally, so that months that reflect heavy event activity are treated the same as months with low activity (Loughran and Ritter, 1999). If there is differential abnormal performance in periods of high activity versus periods of low activity, the regression approach will average over these, and may be less likely to uncover abnormal performance. In other words, the full sample period regression, which tests for average monthly abnormal returns (given the model), will have low power against the alternative of abnormal performance in "hot" markets and no abnormal performance otherwise. A final concern is that the calendar-time portfolio regressions have low power to detect abnormal performance, as argued by Loughran and Ritter (1999).

Our first approach is to directly address the specific concerns of Loughran and Ritter (1999). In addition, we address these concerns simultaneously, by repeating the calendar-time portfolio analysis using the calendar-time abnormal return (*CTAR*) methodology, first used by Jaffe (1974) and Mandelker (1974), and strongly advocated by Fama (1998).

C.1. Heteroskedasticity

One important statistical issue is whether and how to control for heteroskedasticity. Since the number of firms in the event portfolio changes through time, the portfolio residual variance may also be changing through time. We mitigate the heteroskedasticity problem substantially by requiring at least 10 firms in the event portfolio at each point in time, which accounts for the majority of the diversification effect of the portfolio residual variance. The question is whether more should be done. One common "correction" for EW portfolios is weighted least squares, where the weights are proportional to $\sqrt{n}$ (for example, see Franks, Harris, and Titman (1991). Here, the effects of the number of firms in the event portfolio changing through time on the residual variance can be neutralized by transforming the regression as in equation (7).

$$
\begin{aligned}
\sqrt{n} \cdot R_{p,t} &= \sqrt{n} \cdot (X_t \cdot \beta_p + \varepsilon_{p,t}) \\
n \cdot Var(R_{p,t}) &= n \cdot Var(X_t \cdot \beta_p) + n \cdot Var(\varepsilon_{p,t}) \\
n \cdot Var(\varepsilon_{p,t}) &= n \cdot Var\left(\frac{1}{n} \sum_{i=1}^{n} e_{i,t} \right) = n \cdot \frac{n \cdot \sigma_t^2}{n^2} = \sigma_t^2
\end{aligned}
\tag{7}
$$

This transformation assumes that the individual firm residuals are independent, and effectively gives equal weight to all observations. However, this completely defeats the purpose of forming calendar-time portfolios, which is to account for the fact that individual firm residuals are cross-sectionally correlated.

Our approach to deal with the potential problems of heteroskedasticity is to calculate the finite-sample critical values using a general non-parametric bootstrap procedure detailed in Horowitz (1996). This procedure is quite general and can be used for unknown forms of heteroskedasticity. Specifically, the bootstrapping procedure amounts to sampling 1,000 (y,X) pairs from the original data with replacement. We estimate b^*, s^*, and t^* for each bootstrap sample, where b^* and s^* are the OLS coefficient and standard error estimates, and $t^* = (b^* - \beta)/s^*$, where β is the original OLS estimate. The empirical distribution of the *t*-statistics is used to determine the finite-sample critical values. We reject the null if $|t| > z^*$, where t is the original *t*-statistic and z^* is the critical value from the empirical distribution of *t*-statistics.

We find that inferences are unaffected using the bootstrapped critical values rather than the traditional 5% level critical value of 1.96 for all of the intercepts. As can be seen below, the full sample bootstrapped critical values are tightly scattered around the theoretical 5% critical value of 1.96.

Bootstrapped Critical Values for Full Sample Calendar-Time Portfolio Regressions

Equal-Weight			Value-Weight		
Mergers	SEOs	Repurchases	Mergers	SEOs	Repurchases
2.09	2.13	1.92	2.08	1.91	1.92

C.2. Hot versus Cold Markets

We test for differential performance in heavy and low event activity periods. We rerun the full sample portfolio regressions after including two dummy variables, formed on whether there are an unusually large or small number of firms in the event portfolio during that calendar month (see Table 6). We define monthly activity as the number of firms in the event portfolio divided by the number of firms listed in the CRSP monthly return files that month, accounting for the changing composition of the CRSP population through time. Because Nasdaq firms are added to CRSP in the middle of our sample period, we first define event activity for each exchange, and then create a CRSP-level activity index as the weighted average of the exchange-level activity indices. The "HOT" variable is equal to one if event activity lies above the 70th percentile of all monthly activities, and zero otherwise; while the "COLD" variable is equal to one if event activity lies below the 30th percentile of all monthly activities, and zero otherwise. The coefficient on the HOT dummy variable is insignificant for all of the samples, regardless of whether EW or VW portfolios are analyzed, and regardless of whether the coefficients are adjusted or not. The coefficient on the COLD dummy variable is only significant for the VW SEO portfolio. Specifically, the average monthly abnormal returns (adjusted) for the VW SEO portfolio are -0.15% (*t*-statistic = -1.26) in normal activity periods, 0.07% (*t*-statistic = 0.41) in heavy activity periods, and 0.42% (*t*-statistic = 2.34) in low activity periods. These results suggest that the measured abnormal performance for our event samples is not systematically related to the intensity of the event activity. Our evidence is inconsistent with the hypothesis advanced by Loughran and Ritter (1999) that abnormal performance is concentrated in periods when there are a relatively large number of events.

*C.3. Calendar-Time Abnormal Returns (*CTAR*s)*

The *CTAR* is the average abnormal return calculated each calendar month for all sample firms that have completed the event within the prior three years:

$$CTAR_t = R_{p,t} - E(R_{p,t}) \tag{8}$$

where $R_{p,t}$ is the monthly return on the portfolio of event firms, and $E(R_{p,t})$ is the expected return on the event portfolio. The expected return on the event portfolio is proxied by both the Fama and French (FF) three-factor model and with the 25 size-BE/ME portfolios. The *CTAR* approach is similar in spirit to the portfolio regression method, in that event portfolio abnormal returns are calculated in calendar-time, such that the portfolio variance accounts for the cross-sectional correlation in the individual event firm abnormal returns.

When the 25 size-BE/ME portfolios serve as the expected return proxy, the benchmark can change through time to reflect changes in the firm's characteristics. Moreover, even when expected returns are proxied by the FF three-factor model, the changing parameter problem is mitigated. Proxying expected returns with the FF three-factor model amounts to estimating individual firm factor loadings over a five-year post-event estimation period (requiring at least 36 months of valid returns), and then averaging these to form the monthly portfolio factor loadings. Although parameter estimates are assumed constant for each sample firm over the five-year estimation period, the event portfolio parameters are allowed to change each month as new firms are added, and seasoned firms are dropped.

The monthly *CTAR*s are standardized by estimates of the portfolio standard deviation, which serves two purposes (see Table 7 for details). First, by standardizing the monthly *CTAR*s, we control for heteroskedasticity. Second, standardizing effectively gives more weight to periods of heavy event activity than periods of low event activity because the portfolio residual variance is decreasing in portfolio size, all else equal. Each standardized monthly *CTAR* should have mean zero, and should be independent. Therefore, statistical inference is based on the time-series mean of the monthly standardized *CTAR*s, and standard error of the mean.[15]

The results from the *CTAR* analysis are presented in Table 7. For the most part, the *CTAR* results are similar to the portfolio regression results for all three of the event samples,

[15] The time-series mean monthly *CTAR*s is the measure of abnormal performance, while *t*-statistics are calculated from the time-series of monthly standardized *CTAR*s. In some cases, it is possible for the mean *CTAR* to be associated with a *t*-statistic of opposite sign, depending on the estimated variances.

indicating that the regression results are quite robust. The primary difference is that virtually all of the *CTAR*s are smaller in magnitude than the corresponding regression estimate, suggesting that the regression intercepts are not biased towards zero as some of the potential concerns predict.

The main result from the acquirer sample—significantly negative abnormal returns are limited to stock mergers—is robust to the *CTAR* methodology, with the exception of the EW portfolio relative to the 25 size-BE/ME portfolios. In fact, when the 25 size-BE/ME portfolios are used as the expected return benchmark, none of the EW post-event acquirer portfolios have significant abnormal returns—the three year abnormal return for the full sample is only -1.44% with a *t*-statistic of 0.78. Moreover, there is no significant difference between the value and growth acquirer portfolios for either of the benchmarks, regardless of whether the portfolios are EW or VW.

Again, the *CTAR* results for issuers largely confirm those from the calendar-time portfolio regressions. The EW portfolio experiences abnormally low returns following the equity issue, on the order of -9% over three years, with a *t*-statistic around -4.6 or -3.4 depending on which benchmark is used. There is no hint of differential abnormal performance between the growth and value portfolios, as the *t*-statistics for the difference range from -0.07 to 0.57. Value-weighting eliminates reliable underperformance for the full sample.

The EW *CTAR* results for repurchasers have a greater tendency to be statistically significant than the calendar-time portfolio regressions, but are of similar magnitude. The most notable difference is that the EW repurchase portfolio average abnormal returns are now significant. In particular, the three-year EW abnormal returns are 3.2% (*t*-statistic = 2.06) and 5.8% (*t*-statistic = 3.65) for the FF 3-factor and 25 size-BE/ME adjusted *CTAR*s, respectively. Again, there is no difference between the abnormal returns of the value and growth portfolios, and no evidence of abnormal performance when the repurchaser portfolios are value-weighted.

Overall, the *CTAR* results tend to confirm our inferences from the calendar-time portfolio regressions, but indicate that the point estimates are slightly smaller. To the extent that the *CTAR* methodology is plagued by fewer statistical flaws, more faith should be placed in these results.

C.4. Power of the Calendar-Time Portfolio Approach

In order to directly address the concerns raised in Loughran and Ritter (1999), we assess the specification and power of the calendar-time portfolio regressions and the FF *CTAR*s. Specifically, we calculate abnormal performance for random samples of similar size and time period to those analyzed in this study. We draw 1,000 random samples of 2,000 firms over the period July 1963 to December 1993 from the population of CRSP firms with at least one valid return. For each of the 1,000 random samples, we induce three-year abnormal returns ranging from -20% to 20%. Tables 8.a and 8.b report the results, and plot the power functions. The calendar-time portfolio regressions and *CTAR*s are better specified when the portfolios are value-weighted rather than equally-weighted. Moreover, both of the calendar-time portfolio methodologies reject over 99% of the time with induced abnormal performance of ±15% over three years, regardless of weighting scheme.

We conclude that the calendar-time portfolio approach has sufficient power to detect abnormal performance over economically important ranges. Moreover, in direct contrast to the claims of Loughran and Ritter (1999), we find that the calendar-time portfolio approach has more power to detect abnormal performance than the *BHAR* approach after accounting for cross-sectional dependence of individual firm abnormal returns.

VI. INTERPRETING THE RESULTS

A. Violating the Assumption of Independence

Because of positive cross-sectional correlation of individual firm abnormal returns, which is increasing in holding period, the *p*-values associated with *BHAR*s are surely overstated. For our three samples, standard errors that account for cross-sectional correlations of *annual* abnormal returns are over 4 times as large for the SEO sample and roughly 2.5 times those that assume independence for the merger and share repurchase samples. Consistent with earlier research by Bernard (1987), we find that the average correlation of individual firm abnormal returns increases dramatically with the holding period, suggesting that the standard errors assuming independence for three-year *BHAR*s are even more severely overstated.

This point is often dismissed in long-term event studies, which frequently claim that bootstrapping solves all dependence problems. However, this is not true. An empirical distribution created by randomly selecting firms with similar size-BE/ME characteristics does not replicate the covariance structure underlying the original event sample. In fact, the typical bootstrapping approach does not even capture the cross-sectional correlation structure related to industry effects documented by Bernard (1987), Brav (1999) and others. Moreover, Bernard shows that the average *inter*-industry cross-sectional correlation of individual abnormal returns is also positive. Our estimate of the average correlation of three-year *BHAR*s for SEOs is only 0.0035, but this is important when there are over 9.8 million unique correlations.

There are essentially three approaches to dealing with cross-sectional correlation of abnormal returns. The first approach is to ignore the problem by assuming all event announcements are independent, and that event firms are directly comparable to randomly selected non-event firms. The second approach is to recognize that cross-sectional dependence may be a serious problem, and estimate the covariance structure. The final approach is to form calendar-time portfolios, which completely avoids the problems associated with cross-sectional dependence. In light of the evidence that the event firm abnormal returns for our three samples show considerable cross-sectional dependence, ignoring the problem is clearly not appropriate. Moreover, the approaches prescribed by Brav (1999) and Lyon, Barber, and Tsai (1999) do not provide a complete correction to the dependence problem. Like Fama (1998), we strongly advocate the use of a calendar-time portfolio approach.

B. On the Joint Test Problem and Attributing Mis-Pricing to "Event" Samples

All tests of long-term stock price performance are necessarily joint tests of market efficiency and the assumed model of expected returns, whether an asset pricing model, such as the FF three-factor model, or some other expected return benchmark, such as the 25 size-BE/ME portfolios. With this in mind, it is interesting to examine whether the measured abnormal performance is merely a manifestation of known mis-pricings of the FF three-factor model. In other words, it is clear that there is some reliable mis-pricing of securities, given the model. However, it is not clear that all of the mis-pricing is unique to the event firms.

Fama and French (1993, 1996), Fama (1998), and Davis, Fama, and French (1999) emphasize that the FF three-factor model is unable to completely describe the cross-section of expected returns, even on the dimensions on which it is based. This is illustrated by the several statistically significant intercepts from the generic size-BE/ME portfolios reported in Table 5. Interestingly, across all three event samples, both EW and VW, none of the full sample post-event average abnormal returns fall outside of the range of the generic size-BE/ME portfolio intercepts. Although the EW acquirer and issuer portfolios have significantly negative intercepts, the estimates are smaller in magnitude than some of the intercepts that are deemed sufficiently small to justify use of the model in the first place.

This suggests that the null hypothesis—intercept equals zero—may be problematic for samples tilted towards characteristics that the model cannot price in the first place. We find that much of what is typically attributed to an "event" is really a more common phenomenon that the event firms happen to be correlated with. For example, comparison of the estimated intercepts and the adjusted intercepts for the EW portfolios suggests that one-third of the estimated abnormal performance for mergers and SEOs is due to model mis-specification, rather than the "event." The model misspecification is especially severe for the EW event portfolios composed of "growth" firms. In particular, one half of the estimated abnormal return for growth acquirers, and virtually none of the estimated abnormal returns for growth equity issuers and share repurchasers is unique to the event firms, *per se*. Instead, the FF three-factor model over estimates the expected returns of otherwise similar non-event growth firms just as poorly. This is especially severe for *small* growth firms.

C. Economic Significance

Interpreting the economic significance of the estimates is nearly as tricky as assessing the statistical reliability. Consider the SEO sample, which is associated with the most reliable abnormal return estimate. The EW and VW calendar-time portfolio regressions imply three-year abnormal returns of -7.9% and 0.0%, respectively, after adjusting for the expected abnormal performance, given the sample composition. On an equal-weight basis, where all firms are treated as equally important, this is suggestive of serious mis-pricing. However, the reliable evidence of abnormal performance is restricted to the smallest stocks. After controlling for the known pricing deficiencies of the FF three-factor model, only the smallest quintile of SEO firms have reliably significant abnormal returns, and this only on an EW basis (results not reported). Fama and French (1993) report that the firms in the smallest quintile (based on NYSE breakpoints) account for only 2.8% of the value of the CRSP VW stock market, on average. Although this represents a large number of firms, it is not clear how economically important this portion of the market is for assessing overall stock market efficiency.

In general, the VW average abnormal returns are not very large in economic terms. For the full samples, the VW calendar-time portfolio adjusted intercepts correspond to a range of -1.4% to 0.0% over three-years. Finally, it is interesting to note that for the firms in the largest size quintile—on average representing 73.9% of the value of the CRSP VW stock market—none of the adjusted intercepts from the calendar-time portfolio regressions are reliably different from the expected null for any of the three-samples, regardless of whether EWs or VWs are used (results not reported).

VII. CONCLUSION

This paper re-examines the reliability of recent long-term stock price performance estimates using three large well-explored samples of major corporate events. We find that the popular approach of measuring long-term abnormal performance with mean *BHAR*s in conjunction with bootstrapping is not an adequate methodology because it assumes independence of multi-year event firm abnormal returns. We show that event firm abnormal returns are positively cross-correlated when overlapping in calendar-time. As such, assuming independence is problematic for any long-term abnormal performance methodology. Moreover, this is likely to be a problem for most event samples, not just the mergers, SEOs, and share repurchases examined in this paper, since major corporate actions are not random. As a result, we strongly advocate a methodology that accounts for the dependence of event firm abnormal returns, such as the calendar-time portfolio approach.

The primary implication of our results is that most of the evidence against market efficiency consisting of recent studies measuring significant long-term abnormal returns following major corporate events is largely irrelevant because these studies assume independence. Our estimates of long-term abnormal performance that account for the positive cross-correlations of event firm abnormal returns produce very little evidence of long-term abnormal performance.

APPENDIX A --- CALCULATING "CORRECTED" *t*-STATISTICS

To calculate "corrected" *t*-statistics that account for cross-correlation of three-year *BHAR*s, we assume a very simple correlation structure to calculate the average correlation of three-year *BHAR*s across all of our 4,439 observations. The correlation matrix requires only an estimate of the average correlation of three-year *BHAR*s for sample firms with complete (36 months) calendar-time overlap. This is because we assume that the correlation is decreasing linearly as the amount of overlap falls from complete calendar-time overlap of 36 months to no overlap between observations (see Table A.1). We assume that the average correlation of *BHAR*s with 36 months of calendar-time overlap is $\rho = 0.02576$. Recall that this is our estimate using annual *BHAR*s, which almost surely understates the true correlation of three-year *BHAR*s with 36 months of overlap. We then estimate the correlation of three-year *BHAR*s with 35 months of overlap as $35/36 \cdot \rho = 0.02504$, and so on. The estimate for non-overlapping observations is zero. This procedure produces an average correlation for the *BHAR*s of 0.00351.

We then use the average correlation of the three-year *BHAR*s to adjust the *t*-statistic that assumes independence. Using equation (4), we are able to determine by how much the *t*-statistic that assumes independence is understated.[16]

$$\frac{\sigma_{BHAR}(Independence)}{\sigma_{BHAR}(Dependence)} \approx \frac{1}{\sqrt{1+(N-1)\overline{\overline{\rho_{i,j}}}}} = \frac{1}{\sqrt{1+(4{,}439-1)\cdot 0.00351}} = 0.246 \tag{4}$$

Although the average correlation is small, the standard deviation that assumes independence is less than one-fourth the magnitude of the standard deviation that accounts for cross-correlation. This translates directly into *t*-statistics that are four times too large if observations are assumed to be independent (-6.05 versus -1.49).

[16] The approximation of the ratio of σ*(Independence)* to σ*(Dependence)* assumes equal variances of the individual *BHAR*s.

Table A.1
Correlation Structure for the Seasoned Equity Issuer Sample

Number of Months of Overlap	Number of Unique Correlations $n(n-1)/2$	Assumed Correlation Structure	Estimated Correlation
36	50,241	ρ	0.02576
35	96,914	35/36*ρ	0.02504
34	92,094	34/36*ρ	0.02433
33	88,297	33/36*ρ	0.02361
32	84,531		0.02290
31	80,937		0.02218
30	79,158		0.02147
29	75,881		0.02075
28	73,894		0.02004
27	72,001		0.01932
26	71,554		0.01860
25	70,956		0.01789
24	71,347		0.01717
23	69,456		0.01646
22	68,503		0.01574
21	66,132		0.01503
20	66,093		0.01431
19	65,627		0.01360
18	65,674		0.01288
17	63,926		0.01216
16	63,106		0.01145
15	62,344		0.01073
14	62,531		0.01002
13	63,139		0.00930
12	64,265		0.00859
11	62,811		0.00787
10	63,017		0.00716
9	61,357		0.00644
8	62,370		0.00572
7	61,621		0.00501
6	61,945		0.00429
5	61,800		0.00358
4	62,704		0.00286
3	63,196		0.00215
2	63,684	2/36*ρ	0.00143
1	63,357	1/36*ρ	0.00072
0	7,373,678	0	0.00000
			Average = 0.00351

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Table I
Distribution of Event Sample Firms by Size and Book-to-Market Equity (7/61 – 12/93)

Size and book-to-market equity groupings are based on independent rankings of sample firms relative to NYSE quintiles in both the year prior (*t*-1) and subsequent (*t*+1) to the completion of the event.

Panel A: Mergers

	Book-to-Market (*t*-1)						Book-to-Market (*t*+1)					
	Low	2	3	4	High		Low	2	3	4	High	
Small	3.3	2.1	2.4	2.5	4.0	14.3	3.0	2.1	2.5	2.0	4.0	13.6
2	3.0	2.8	2.9	2.7	2.8	14.2	2.5	3.2	2.1	2.3	2.4	12.5
3	4.2	2.6	3.3	3.5	1.9	15.5	3.5	3.5	2.9	3.4	1.9	15.2
4	5.1	5.3	5.3	4.2	2.6	22.7	5.3	5.9	6.1	4.4	2.2	23.9
Large	11.4	8.5	6.6	5.0	1.8	33.3	12.0	8.2	7.0	5.1	2.6	34.8
	27.1	21.3	20.6	17.9	13.1	100.0	26.3	22.9	20.7	17.1	13.0	100.0

Panel B: Seasoned Equity Offerings

	Low	2	3	4	High		Low	2	3	4	High	
Small	16.1	6.0	4.4	4.6	5.5	36.6	16.5	5.8	3.8	2.8	2.7	31.6
2	8.2	3.0	2.9	2.6	2.5	19.3	10.9	3.9	2.6	2.2	1.6	21.1
3	4.3	2.9	2.9	3.2	2.0	15.3	7.1	2.7	2.7	2.8	1.4	16.6
4	3.0	2.3	3.9	3.3	1.7	14.2	4.3	2.7	3.7	3.2	1.5	15.4
Large	2.4	2.6	3.3	4.2	2.1	14.6	3.0	2.7	3.6	3.7	2.2	15.2
	34.1	16.8	17.5	17.9	13.7	100.0	41.7	17.9	16.3	14.7	9.4	100.0

Panel C: Share Repurchases

	Low	2	3	4	High		Low	2	3	4	High	
Small	4.9	6.6	5.4	5.6	10.0	32.6	3.7	5.5	7.3	7.8	9.7	34.0
2	3.7	4.8	3.8	3.6	2.5	18.3	2.9	4.1	4.1	3.7	3.1	18.0
3	4.2	3.1	3.1	2.5	1.9	14.8	3.7	3.7	3.0	2.6	1.6	14.6
4	4.0	3.7	4.1	2.2	1.8	15.8	3.5	3.9	3.8	2.9	2.0	16.2
Large	6.5	4.2	3.7	3.0	1.0	18.5	5.8	4.2	3.2	2.7	1.3	17.2
	23.4	22.4	20.1	16.9	17.2	100.0	19.6	21.3	21.5	19.8	17.8	100.0

Panel D: CRSP Universe

	Low	2	3	4	High	
Small	14.5	8.8	8.2	9.7	16.8	57.9
2	4.0	2.9	2.9	2.6	2.4	14.8
3	3.0	2.3	2.2	1.9	1.4	10.7
4	2.6	2.0	1.8	1.5	0.9	8.7
Large	2.7	1.8	1.5	1.2	0.6	7.9
	26.9	17.7	16.5	16.8	22.1	100.0

Table II.a
Three-Year Mean Buy-and-Hold Abnormal Returns (*BHAR*s) for Acquirers (7/61 - 12/93)

*BHAR*s are calculated as the difference between the equal- and value-weight average three-year return for the event firms and the benchmark portfolios. Three-year returns begin the month following completion of the event. The benchmark portfolios are 25 value-weight non-rebalanced portfolios formed on size and book-to-market equity (BE/ME) based on NYSE breakpoints. Statistical inference is based on an empirical distribution created by simulating 1,000 pseudo-samples with similar characteristics to those of the event sample firms and then calculating the mean *BHAR* for each pseudo-sample. The *p*-value is the fraction of the mean *BHAR*s from the pseudo-samples larger in magnitude (but with the same sign) than the original mean *BHAR*. The wealth relative (W.R.) is the average three-year gross return of the sample firms divided by the average three-year gross return of the benchmark firms. Growth firms are identified as firms with BE/ME ratios in the lowest quintile of all NYSE firms. Value firms are identified as firms with BE/ME ratios in the highest quintile of all NYSE firms.

	Equal-Weight						Value-Weight					
	Sample	Bench	W.R.	*BHAR*	*p*-value	n	Sample	Bench	W.R.	*BHAR*	*p*-value	n
Post-Event	0.514	0.524	0.994	-0.010	0.164	2,068	0.381	0.419	0.973	-0.038	0.027	2,068
Pre-Event	0.881	0.597	1.178	0.285	0.000	1,796	0.468	0.420	1.034	0.048	0.320	1,796
Financed with Stock	0.393	0.477	0.943	-0.084	0.000	1,029	0.297	0.350	0.961	-0.053	0.009	1,029
Financed without Stock	0.634	0.570	1.041	0.064	0.047	1,039	0.483	0.503	0.986	-0.021	0.291	1,039
Growth Firms	0.390	0.372	1.013	0.018	0.481	526	0.302	0.333	0.977	-0.031	0.186	526
Value Firms	0.718	0.691	1.016	0.027	0.471	257	0.556	0.697	0.917	-0.142	0.149	257

Table II.b
Three-Year Mean Buy-and-Hold Abnormal Returns (*BHAR*s) for Seasoned Equity Issuers (7/61 - 12/93)

	Equal-Weight						Value-Weight					
	Sample	Bench	W.R.	*BHAR*	*p*-value	n	Sample	Bench	W.R.	*BHAR*	*p*-value	n
Post-Event	0.348	0.450	0.930	-0.102	0.000	4,439	0.411	0.453	0.971	-0.042	0.165	4,439
Pre-Event	1.519	0.673	1.505	0.845	0.000	2,982	0.424	0.445	0.985	-0.022	0.516	2,982
Excluding Utilities	0.343	0.432	0.938	-0.089	0.000	3,842	0.435	0.441	0.996	-0.006	0.450	3,842
Growth Firms	0.259	0.237	1.017	0.022	0.714	1,410	0.451	0.324	1.096	0.127	0.052	1,410
Value Firms	0.427	0.668	0.856	-0.240	0.000	538	0.448	0.694	0.855	-0.246	0.000	538

Table II.c
Three-Year Mean Buy-and-Hold Abnormal Returns (*BHAR*s) for Equity Repurchasers (7/61 - 12/93)

	Equal-Weight						Value-Weight					
	Sample	Bench	W.R.	*BHAR*	*p*-value	n	Sample	Bench	W.R.	*BHAR*	*p*-value	n
Post-Event	0.787	0.642	1.088	0.145	0.000	2,292	0.541	0.475	1.044	0.066	0.227	2,292
Pre-Event	0.558	0.529	1.019	0.029	0.346	1,919	0.458	0.428	1.021	0.030	0.459	1,919
Open Market	0.776	0.620	1.096	0.156	0.000	1,942	0.559	0.480	1.053	0.079	0.112	1,942
Tender Offer	0.854	0.767	1.049	0.087	0.228	350	0.442	0.449	0.995	-0.007	0.482	350
Growth Firms	0.590	0.491	1.067	0.099	0.021	503	0.382	0.353	1.021	0.028	0.460	503
Value Firms	1.096	0.852	1.132	0.244	0.030	369	0.824	0.641	1.111	0.183	0.134	369

Table III
Critical Values for Mean Buy-and-Hold Abnormal Returns Assuming Independence

Critical values from the empirical distributions of the equal- and value-weight mean buy-and-hold abnormal returns (*BHAR*s) from the merger, seasoned equity offering, and share repurchase event samples. The empirical distribution is created by simulating 1,000 pseudo-samples with similar characteristics to those of the event sample firms and then calculating the mean *BHAR* for each pseudo-sample. The critical values assuming normality of the empirical distribution are calculated by adding (subtracting) two standard deviations to the mean for the 2.5th (97.5th) percentile and by adding (subtracting) three standard deviations to the mean for the 0.5th (99.5th) percentile, where the mean and standard deviation are calculated from the empirical distribution.

	Equal-Weight				Value-Weight			
	0.5	2.5	97.5	99.5	0.5	2.5	97.5	99.5
Mergers								
Empirical Distribution	-4.3%	-2.9%	5.9%	7.0%	-6.1%	-4.1%	9.1%	10.6%
Assuming Normality	-5.6%	-3.3%	5.5%	7.8%	-7.3%	-4.1%	8.5%	11.7%
Seasoned Equity Offerings								
Empirical Distribution	-3.6%	-2.4%	5.9%	7.5%	-10.5%	-8.2%	10.2%	21.6%
Assuming Normality	-4.8%	-2.7%	5.8%	7.9%	-14.6%	-9.7%	10.0%	14.9%
Share Repurchases								
Empirical Distribution	-4.5%	-3.3%	7.0%	8.9%	-6.5%	-4.6%	15.3%	21.3%
Assuming Normality	-6.2%	-3.5%	7.0%	9.6%	-11.4%	-6.4%	13.7%	18.7%

Table IV
Corrected *t*-Statistics for Average Three-Year Buy and Hold Abnormal Returns (*BHAR*s)

The "corrected" *t*-statistics are adjusted to account for cross-correlation of individual *BHAR*s using the correlation structure described in Appendix A. Average *BHAR*s are calculated as the difference between the equal-weight average three-year return for the sample of event firms and the benchmark portfolios. Three-year returns begin the month following completion of the event. The benchmark portfolios are 25 value-weight non-rebalanced portfolios formed on size and book-to-market equity based on NYSE breakpoints. The empirical distribution is created by simulating 1,000 pseudo-samples with similar characteristics to those of the event sample firms and then calculating the mean *BHAR* for each pseudo-sample. The *t*-statistic that assumes independence is calculated as the average *BHAR* minus the mean of the empirical distribution, all divided by the standard deviation of the empirical distribution. The "corrected" *t*-statistics are adjusted using the following approximation:

$$\frac{\sigma_{BHAR}(Independence)}{\sigma_{BHAR}(Dependence)} \approx \frac{1}{\sqrt{1+(N-1)\overline{\rho_{i,j}}}}$$

Descriptive Statistics	Mergers	SEOs	Repurchases
Average *BHAR*	-0.010	-0.102	0.145
Mean of Empirical Distribution	0.0111	0.0111	0.0172
Standard Deviation of Empirical Distribution	0.0222	0.0187	0.0263
Average Correlation of Annual *BHAR*s with Complete Overlap	0.0175	0.0258	0.0175
***t*-statistics**			
Assuming Independence	-0.95	-6.05	4.86
Using Average Correlation of Annual *BHAR*s	-0.42	-1.49	1.91
Using Average Correlation of Annual *BHAR*s + 25%	-0.38	-1.34	1.73
Assuming Average Correlation = 0.01	-0.52	-2.28	2.39
Assuming Average Correlation = 0.02	-0.40	-1.67	1.80
Assuming Average Correlation = 0.03	-0.34	-1.38	1.51

Table V
Intercepts from Excess Stock Return Regressions of 25 Size and Book-to-Market Equity Portfolios on the Fama and French 3-Factor Model (July 1963 – December 1993)

Dependent variables are 25 size and book-to-market equity portfolio returns, *Rp*, in excess of the one-month Treasury bill rate, *Rf*, observed at the beginning of the month. The 25 size and book-to-market equity portfolios are formed on NYSE size and book-to-market equity quintiles. The three factors in the Fama and French model are zero-investment portfolios representing the excess return of the market, *Rm - Rf*; the difference between a portfolio of "small" stocks and "big" stocks, SMB; and the difference between a portfolio of "high" book-to-market stocks and "low" book-to-market stocks, HML. See Fama and French (1993) for details on the construction of the factors.

$$Rp_t - Rf_t = a + b(Rm_t - Rf_t) + sSMB_t + hHML_t + e_t$$

Panel A: Intercepts

	Equal-Weight Portfolios					Value-Weight Portfolios				
	Low	2	3	4	High	Low	2	3	4	High
Small	-0.37	0.02	0.06	0.23	0.26	-0.49	-0.09	-0.05	0.06	0.01
2	-0.21	-0.01	0.11	0.11	-0.04	-0.09	0.03	0.13	0.14	0.03
3	-0.14	0.07	-0.01	0.14	-0.03	-0.06	0.10	0.02	0.12	-0.04
4	0.09	-0.14	0.02	0.01	0.03	0.14	-0.15	0.02	0.01	0.01
Large	0.10	0.00	-0.02	-0.07	-0.09	0.15	-0.01	-0.02	-0.04	-0.24

Panel B: t-statistics

	Equal-Weight Portfolios					Value-Weight Portfolios				
	Low	2	3	4	High	Low	2	3	4	High
Small	-2.65	0.21	0.67	2.75	2.52	-4.80	-1.22	-0.76	1.09	0.18
2	-2.54	-0.09	1.61	1.79	-0.60	-1.02	0.43	1.94	2.29	0.46
3	-1.85	1.00	-0.13	2.18	-0.41	-0.88	1.34	0.33	1.82	-0.45
4	1.15	-1.69	0.26	0.19	0.33	1.94	-1.93	0.29	0.16	0.12
Large	1.67	-0.02	-0.29	-1.05	-0.87	2.37	-0.08	-0.27	-0.61	-2.13

Table VI.a
Calendar-Time Fama and French Three-Factor Model
Portfolio Regressions of Acquirers (7/61-12/93)

Dependent variables are event portfolio returns, *Rp*, in excess of the one-month Treasury bill rate, *Rf*, observed at the beginning of the month. Each month we form equal and value-weight portfolios of all sample firms that have completed the event within the previous three years. The event portfolio is rebalanced monthly to drop all companies that reach the end of their three-year period and add all companies that have just executed a transaction. The three factors are zero-investment portfolios representing the excess return of the market, *Rm* - *Rf*; the difference between a portfolio of "small" stocks and "big" stocks, SMB; and the difference between a portfolio of "high" book-to-market stocks and "low" book-to-market stocks, HML. See Fama and French (1993) for details on the construction of the factors. The intercept, *a*, measures the average monthly abnormal return, given the model. The adjusted intercept measures the difference between the intercept estimated using the event portfolio and the average intercept estimated from 1,000 random samples of otherwise similar (based on size and book-to-market) non-event firms. A minimum of 10 firms in the event portfolio is required. The number of monthly observations, *N*, is reported in square brackets, and *t*-statistics are in parenthesis.

$$Rp_t - Rf_t = a + b(Rm_t - Rf_t) + sSMB_t + hHML_t + e_t$$

	Equal-Weight			Value-Weight		
	a (*t*-stat)	Adj. *a* (*t*-stat)	Adj. R^2 [N]	*a* (*t*-stat)	Adj. *a* (*t*-stat)	Adj. R^2 [N]
Full sample (Post-Event)	-0.20	-0.14	0.97	-0.03	-0.04	0.95
	(-3.70)	(-2.61)	[390]	(-0.48)	(-0.68)	[390]
Full sample (Pre-Event)	0.49	0.52	0.97	0.19	0.15	0.94
	(9.49)	(10.09)	[378]	(3.05)	(2.48)	[378]
Financed with Stock	-0.33	-0.25	0.95	-0.14	-0.12	0.93
	(-4.64)	(-3.59)	[390]	(-2.00)	(-1.81)	[390]
Financed without Stock	-0.09	-0.04	0.94	0.14	0.10	0.89
	(-1.14)	(-0.54)	[388]	(1.68)	(1.19)	[388]
Growth Firms	-0.37	-0.18	0.90	-0.13	-0.20	0.87
	(-3.64)	(-1.76)	[373]	(-1.23)	(-1.95)	[373]
Value Firms	0.00	-0.08	0.85	0.06	0.03	0.73
	(0.01)	(-0.51)	[355]	(0.32)	(0.14)	[355]
Hot Market	0.00	0.04	0.97	-0.12	-0.13	0.95
	(0.01)	(0.08)	[390]	(-0.89)	(-0.93)	[390]
Cold Market	0.11	0.14	0.97	0.19	0.19	0.95
	(0.85)	(1.12)	[390]	(1.42)	(1.40)	[390]

Table VI.b
Calendar-Time Fama and French Three-Factor Model
Portfolio Regressions of Seasoned Equity Issuers (7/61 - 12/93)

	Equal-Weight			Value-Weight		
	a (*t*-stat)	Adj. *a* (*t*-stat)	Adj. R^2 [N]	*a* (*t*-stat)	Adj. *a* (*t*-stat)	Adj. R^2 [N]
Full sample (Post-Event)	-0.33	-0.22	0.96	-0.03	0.00	0.90
	(-5.19)	(-3.51)	[390]	(-0.44)	(-0.02)	[390]
Full sample (Pre-Event)	1.28	1.30	0.95	0.19	0.17	0.90
	(17.39)	(17.62)	[378]	(2.55)	(2.30)	[378]
Excluding Utilities	-0.37	-0.26	0.97	0.06	0.09	0.91
	(-5.58)	(-3.86)	[390]	(0.77)	(1.08)	[390]
Growth Firms	-0.32	-0.07	0.94	0.19	0.16	0.88
	(-3.45)	(-0.76)	[390]	(1.62)	(1.39)	[390]
Value Firms	-0.31	-0.34	0.86	-0.24	-0.15	0.66
	(-2.24)	(-2.52)	[314]	(-1.36)	(-0.87)	[314]
Hot Market	-0.12	0.02	0.96	0.07	0.07	0.90
	(-0.81)	(0.15)	[390]	(0.40)	(0.41)	[390]
Cold Market	0.07	-0.05	0.96	0.32	0.42	0.90
	(0.45)	(-0.31)	[390]	(1.79)	(2.34)	[390]

Table VI.c
Calendar-Time Fama and French Three-Factor Model
Portfolio Regressions of Equity Repurchasers (7/61 - 12/93)

	Equal-Weight			Value-Weight		
	a (*t*-stat)	Adj. *a* (*t*-stat)	Adj. R^2 [N]	*a* (*t*-stat)	Adj. *a* (*t*-stat)	Adj. R^2 [N]
Full sample (Post-Event)	0.08	0.08	0.96	-0.01	-0.04	0.90
	(1.24)	(1.17)	[374]	(-0.11)	(-0.43)	[374]
Full sample (Pre-Event)	-0.07	-0.05	0.95	-0.05	-0.08	0.90
	(-0.91)	(-0.72)	[378]	(-0.57)	(-0.94)	[378]
Open Market	0.14	0.18	0.95	0.04	0.01	0.90
	(1.82)	(2.39)	[334]	(0.42)	(0.06)	[334]
Tender Offer	0.14	0.01	0.87	-0.02	-0.09	0.73
	(1.14)	(0.07)	[323]	(-0.12)	(-0.50)	[323]
Growth Firms	-0.20	0.03	0.87	-0.08	-0.13	0.79
	(-1.35)	(0.20)	[286]	(-0.50)	(-0.80)	[286]
Value Firms	0.65	0.48	0.86	0.29	0.17	0.75
	(4.99)	(3.72)	[306]	(1.65)	(0.96)	[306]
Hot Market	0.14	0.04	0.96	0.22	0.17	0.90
	(0.92)	(0.26)	[374]	(1.16)	(0.88)	[374]
Cold Market	-0.13	-0.20	0.96	0.27	0.06	0.90
	(-0.86)	(-1.31)	[374]	(1.37)	(0.31)	[374]

Table VII.a
Mean Calendar-Time Portfolio Abnormal Returns (*CTAR*s) for Acquirers (7/61 - 12/93)

*CTAR*s are calculated each month as the difference between the event portfolio return and the expected return on the portfolio, standardized by the portfolio residual standard deviation. Each month we form equal and value-weight event portfolios containing all sample firms that have completed the event within the previous three years. The event portfolio is rebalanced monthly to drop all companies that reach the end of their three-year period and add all companies that have just executed a transaction. The portfolio expected returns are proxied by both 25 value-weight portfolios formed on size and book-to-market equity based on NYSE breakpoints (25 Size-BE/ME), and the Fama and French three-factor model (FF 3-Factor), which amounts to estimating individual firm factor loadings over a five-year post-event estimation period (requiring at least 36 months of valid returns), and then averaging these to form the monthly portfolio factor loadings. We calculate event portfolio residual variances using 60 months of residuals. Residuals are calculated from portfolio regressions on the FF three-factor model and as monthly differences of event portfolio returns and size-BE/ME portfolio returns. Mean *CTAR*s and standard errors are calculated from the time-series of monthly *CTAR*s. The number of monthly observations are reported in square brackets, and *t*-statistics are in parenthesis.

	Equal-Weight		Value-Weight	
	FF 3-Factor	25 Size-BE/ME	FF 3-Factor	25 Size-BE/ME
Full sample (post-event)	-0.14	-0.04	-0.07	-0.03
	(-2.70)	(0.78)	(-1.75)	(-0.58)
	[366]	[389]	[366]	[389]
Full sample (pre-event)	0.43	0.50	0.17	0.14
	(8.77)	(9.59)	(3.33)	(2.95)
	[364]	[389]	[364]	[389]
Financed with Stock	-0.23	-0.16	-0.13	-0.15
	(-3.59)	(-1.22)	(-2.51)	(-2.27)
	[366]	[388]	[366]	[388]
Financed without Stock	-0.07	0.07	0.04	0.09
	(-0.80)	(1.86)	(0.34)	(0.98)
	[366]	[371]	[366]	[371]
Growth Firms	-0.19	-0.16	-0.05	-0.14
	(-2.05)	(-1.06)	(-0.57)	(-1.23)
	[338]	[338]	[338]	[338]
Value Firms	0.01	0.01	-0.08	-0.17
	(-0.24)	(0.48)	(-0.76)	(-1.40)
	[346]	[346]	[346]	[346]

Table VII.b
Mean Calendar-Time Portfolio Abnormal Returns (*CTAR*s)
for Seasoned Equity Issuers (7/61 - 12/93)

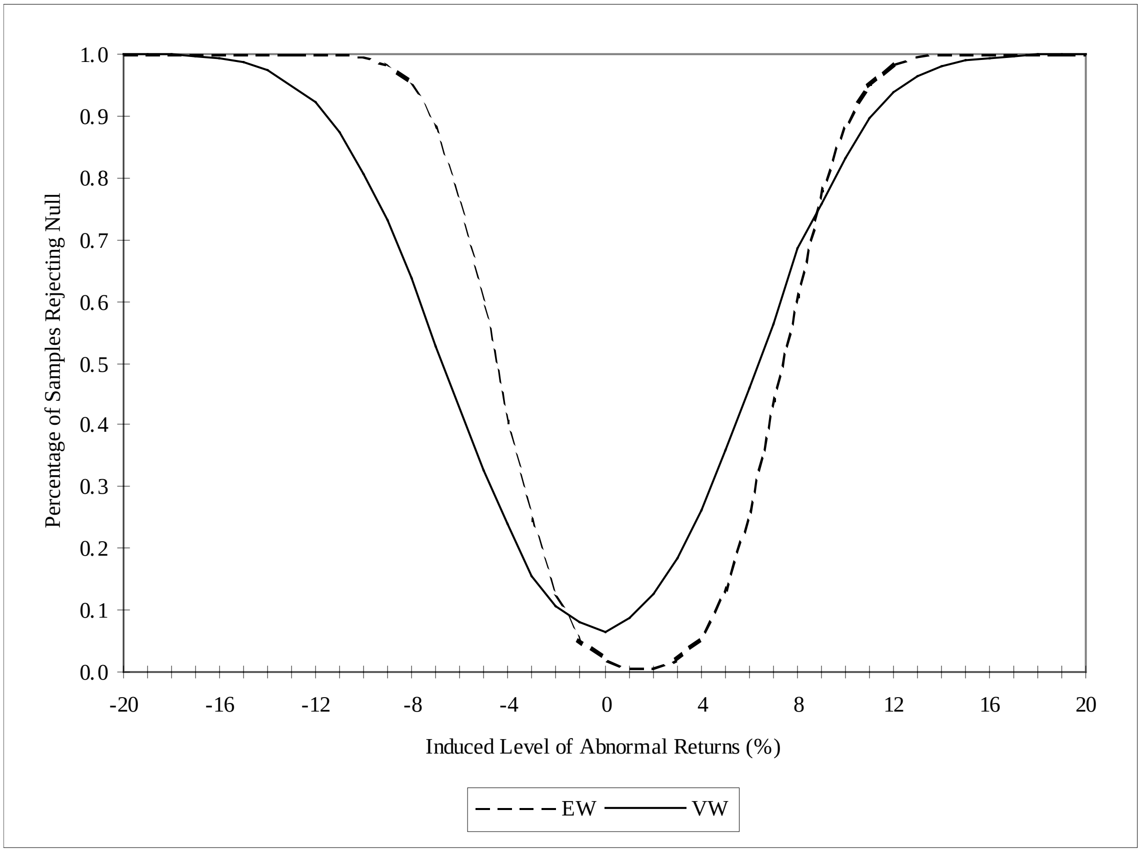
	Equal-Weight		Value-Weight	
	FF 3-Factor	25 Size-BE/ME	FF 3-Factor	25 Size-BE/ME
Full sample (post-event)	-0.25	-0.21	-0.01	-0.08
	(-4.62)	(-3.38)	(-1.01)	(-1.36)
	[366]	[390]	[366]	[390]
Full sample (pre-event)	1.09	1.33	0.19	0.19
	(17.34)	(21.84)	(2.62)	(2.62)
	[365]	[389]	[365]	[389]
Excluding Utilities	-0.28	-0.20	0.03	-0.02
	(-4.65)	(-2.68)	(-0.52)	(-0.62)
	[366]	[390]	[366]	[390]
Growth Firms	-0.14	-0.02	0.04	0.00
	(-1.41)	(-0.24)	(0.07)	(-0.13)
	[301]	[301]	[301]	[301]
Value Firms	-0.13	-0.03	-0.06	-0.06
	(-1.22)	(-0.07)	(-0.75)	(-0.57)
	[257]	[257]	[257]	[257]

Table VII.c
Mean Calendar-Time Portfolio Abnormal Returns (*CTAR*s) for Equity Repurchasers (7/61 - 12/93)

	Equal-Weight		Value-Weight	
	FF 3-Factor	25 Size-BE/ME	FF 3-Factor	25 Size-BE/ME
Full sample (post-event)	0.09	0.16	-0.04	0.02
	(2.06)	(3.65)	(-0.14)	(0.68)
	[366]	[373]	[366]	[373]
Full sample (pre-event)	0.08	0.04	0.03	-0.13
	(1.94)	(2.21)	(0.78)	(-1.35)
	[363]	[379]	[363]	[379]
Open Market	0.14	0.25	0.02	0.05
	(2.20)	(4.26)	(0.44)	(1.01)
	[332]	[334]	[332]	[334]
Tender Offer	0.03	0.11	-0.15	0.09
	(0.21)	(1.06)	(-1.15)	(0.67)
	[315]	[320]	[315]	[320]
Growth Firms	0.13	0.26	0.01	0.01
	(1.09)	(2.19)	(0.15)	(0.00)
	[249]	[250]	[249]	[250]
Value Firms	0.24	0.12	-0.02	-0.04
	(1.78)	(1.20)	(0.02)	(-0.28)
	[244]	[244]	[244]	[244]

Table VIII.a
Simulated Power of EW and VW Calendar-Time Portfolio Regressions Test Statistics

The percentage of 1,000 random samples of 2,000 firms rejecting the null hypothesis of no abnormal performance at various induced levels of abnormal returns.

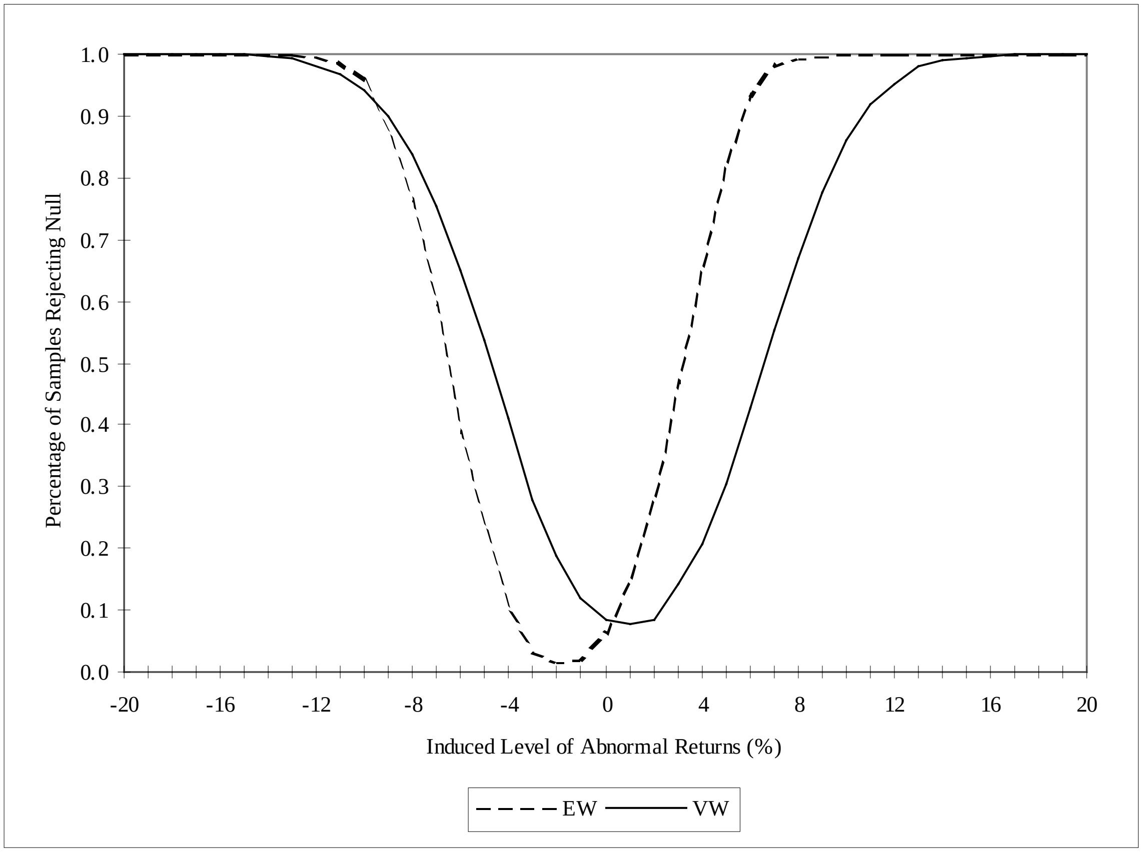


Percentage of Samples Rejecting the Null Based on Calendar-Time Portfolio Regressions

	Induced Level of Abnormal Return (%) over Three Years								
	-20	-15	-10	-5	0	5	10	15	20
EW	1.00	1.00	1.00	0.60	0.02	0.13	0.89	1.00	1.00
VW	1.00	0.99	0.81	0.33	0.07	0.36	0.83	0.99	1.00

Table VIII.b
Simulated Power Function for Calendar-Time Abnormal Returns (*CTAR*s)

The percentage of 1,000 random samples of 2,000 firms rejecting the null hypothesis of no abnormal performance at various induced levels of abnormal returns. *CTAR*s are calculated relative to the Fama and French three-factor model.

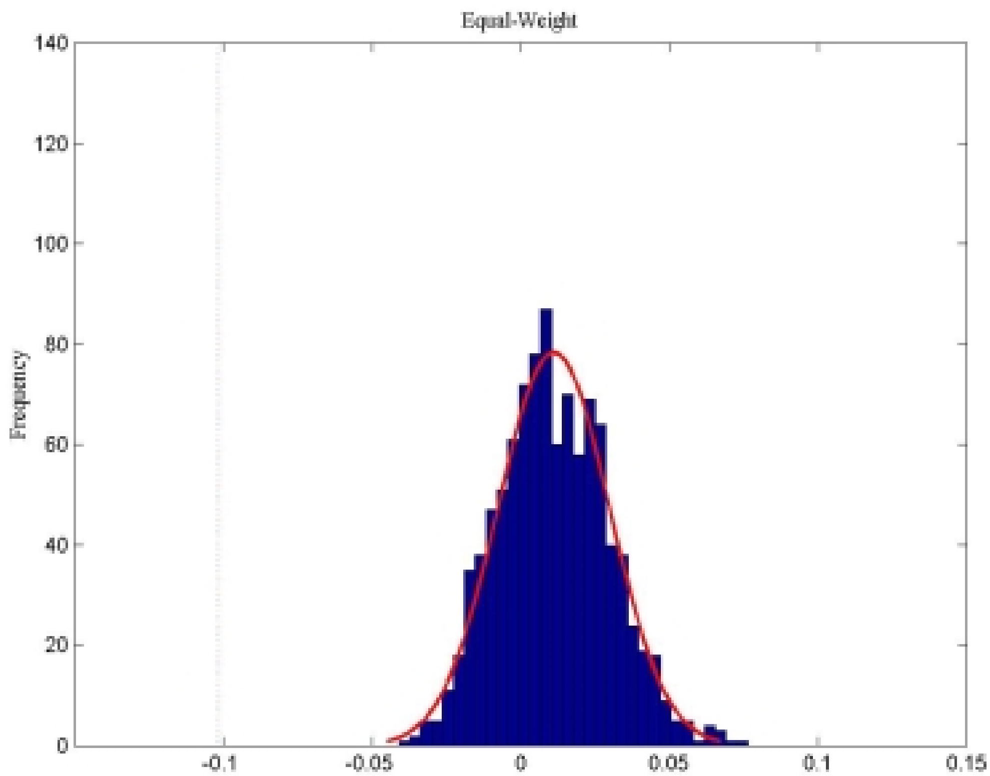


Percentage of Samples Rejecting the Null Based on *CTAR*s.

	Induced Level of Abnormal Return (%) over Three Years								
	-20	-15	-10	-5	0	5	10	15	20
EW	1.00	1.00	0.96	0.24	0.06	0.83	1.00	1.00	1.00
VW	1.00	1.00	0.94	0.54	0.09	0.30	0.86	1.00	1.00

Figure 1
Empirical Distribution for Mean *BHAR* for Seasoned Equity Issue Sample

The histogram plots the empirical distribution of equal-weight average three-year *BHAR*s for 1,000 bootstrap samples. Each bootstrap sample is created by assigning the completion date of each original sample firm to a randomly selected firm with the same size-BE/ME portfolio assignment at the time of the event. This procedure yields a pseudo-sample that has the same size-BE/ME distribution, the same number of observations, and the same calendar time frequency as the original event sample. We then calculate the mean *BHAR* for this pseudo-sample in the same way as for the original sample. This results in one mean *BHAR* under the null of the model. We repeat these steps until we have 1,000 mean *BHAR*s, and thus an empirical distribution of the mean *BHAR* under the null. A *p*-value is calculated as the fraction of the mean *BHAR*s from the pseudo-samples that are larger in magnitude (but with the same sign) than the original mean *BHAR*.



$\overline{BHAR}$