Mimickers of Corporate Insiders Who Make Large-Volume Trades

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Most prior research shows that corporate insiders can systematically earn abnormal returns by buying or selling their own securities. Also, several studies have investigated whether outsiders can earn abnormal returns by mimicking the trades of insiders after the latter report their trades. The findings of these studies suggest that they cannot. In contrast, this study's results indicate that outsiders can earn significant abnormal returns by mimicking such trades. This conclusion is consistent with a growing body of empirical literature that suggests that the market is not efficient in the semistrong form (i.e., is not efficient with respect to all publicly available information).

Most academic studies have concluded that corporate insiders can systematically earn abnormal returns (e.g., see Lorie and Niederhoffer 1968, Jaffe 1974, Finnerty 1976a and 1976b, Pratt and Devere 1978, Seyhun 1986 and 1992, and Rozeff and Zaman 1988).[1] The academic evidence also suggests that outsiders who mimic the trading activities of insiders cannot earn such returns. This evidence is inconsistent with the anecdotal evidence frequently cited in the financial press. Many press articles have cited anecdotal evidence suggesting that those who mimic the trades of insiders can earn abnormal returns (e.g., see Jasen 1992; Peers 1992, 1993a, and 1993b; Giplin 1994; and O'Brien 1996).

Initially, Jaffe and Pratt–Devere provided evidence that an insider-trading anomaly exists (i.e., that outsiders who mimic insiders can earn abnormal returns). This result is inconsistent with semistrong-form market efficiency. More recently, Seyhun and Rozeff–Zaman studied outsider profitability and concluded that no insider-trading anomaly exists.[2] Their evidence implied that outsiders who mimic insiders are unable to earn abnormal returns, a result consistent with semistrong-form efficiency.

Given the importance of the insider-trading anomaly and the growing body of empirical literature suggesting that the market is not semistrong-form efficient (e.g., see Bernard 1991 and Brock, Lakonishok, and LeBaron 1992),[3] we reinvestigated whether outsiders who mimic the trading activities of insiders can earn abnormal returns by using publicly available insider-trading data. We found that they can mimic certain transactions of NYSE and Amex corporate insiders and earn significant abnormal returns (net of assumed transaction costs) for both long and short positions. These results suggest that traders can profit from using publicly available insider-trading data, lend credence to the anecdotal evidence mentioned above, and are inconsistent with semistrong-form efficiency.

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RELEVANT PRIOR STUDIES

In contrast to the procedures of Seyhun and of Rozeff–Zaman, we used the CDA/Investnet database to identify more precisely the dates insider-trading data became widely publicly available during our sample period. This database includes the dates that data pertaining to insider transactions were keypunched for reporting in CDA/Investnet's Insider Trading Monitor (an online database that reports insider-trading data immediately after keypunching).[4]

Specifically, we considered the keypunch date (usually one or two days following the SEC filing date) of a given insider trade to be the date that knowledge of the details of the trade became widely publicly available. Additionally, we assumed that outsiders did not begin to mimic the trades of insiders until the Wednesday of the week following the week of the keypunch date. This procedure introduces a slightly conservative bias to our findings because outsiders could have begun trading immediately after insider data were keypunched. This procedure also helped minimize the likelihood that any information that might have been conveyed by the insider trades studied was

impounded in prices via sources other than insider-trading data before outsiders could have mimicked the trading behaviors of the related insiders.

Seyhun and Rozeff–Zaman were forced to use less-precise dates of widespread public availability because at the times of their studies, services that conveyed insider-trading data to investors on a timely basis were not available. In both of those studies, the authors assumed that it took several months for the related data to become widely publicly available and, thus, for outsiders to have been able to trade using the data. Specifically, the reporting delay Seyhun assumed was three months, on average, after the insider transaction occurred, and the reporting delay Rozeff and Zaman assumed was two months after the end of the month in which the insider transaction occurred.[5] Although justified, these assumptions may have biased their results because relatively early possible (i.e., very likely) trading by outsiders was ignored and because their assumed dates may have occurred after the information related to the insider trades was impounded in prices via sources other than insider-trading data.

In contrast to the earlier studies, we focused only on large-volume trades (i.e., purchases and sales of at least 10,000 shares) by high-ranking insiders. These procedures were used because (1) Seyhun (1992) used them and found larger abnormal returns for insiders than those obtained under his earlier approach and that of Rozeff and Zaman (the cluster approach)[6] and (2) some outsiders, including insider-trading analysts, reportedly used this strategy during our sample period.[7] Additionally, we restricted our attention to NYSE and Amex firms because transaction-cost assumptions can be applied more meaningfully in relation to trades from these exchanges and because related abnormal returns can be measured more reliably than for samples that include very infrequently traded shares.

Next, we calculated separately abnormal returns related to insider purchases and insider sales for both insiders and outsiders.[8] Seyhun used this approach, but Rozeff and Zaman did not. Separating purchases and sales of shares is potentially important because, in general, insiders' decisions to purchase and their decisions to sell are systematically different. For example, insiders often make option-related sales of shares, such as sales of shares previously obtained through employer-granted options, to pay related taxes (see Linney and Marshall 1987).[9] Such sales are particularly attractive because no transaction costs are incurred. Thus, insider purchase decisions are likely to be more closely aligned with assessments of firm value than are insider sales decisions. Consequently, insider purchase decisions are expected to be more value relevant to outsiders than insider sell decisions, especially given timely purchase–sale actions. This view is supported by Seyhun's (1986 and 1992) empirical results.

We also applied event-study methodology, similar to that of Seyhun (1992), which used control portfolios to control for firm size and to adjust prediction errors (in part of our study) for assumed transaction costs (see Kritzman 1994). We also calculated abnormal returns on a weekly basis to ensure the capture of intramonth abnormal returns. Neither Seyhun (1986) nor Rozeff and Zaman used the Seyhun (1992) event-study procedures, and both measured abnormal returns on a monthly basis. Rozeff and Zaman, but not Seyhun, used control portfolios to control for size and adjusted prediction errors for transaction costs.[10]

DATA AND RESEARCH METHOD

This section describes our data and sample, methods for selecting the firms and securities studied, and procedures used in calculating abnormal returns.

Data and Sample

Our sample period is January 1985 through December 1990. January 1985 is the beginning month because it is the first month of the first full year that CDA/Investnet had a contract to compile insider-trading data for the SEC and that keypunch verified the data. The sample period ends in December 1990 to enhance comparability between the reporting requirements related to our study and those in effect during the periods Seyhun and Rozeff–Zaman studied.

Only companies with large-volume trades and sufficient return data for both our estimation and test periods available from the CRSP daily returns tape were studied. The CRSP daily returns were used in calculating weekly returns for all weeks during our sample and estimation periods.[11] Also, we studied only companies that had portfolio numbers assigned by CRSP.

Table 1 provides information about our sample of insider transactions for the two-week holding period. The sample information pertaining to insiders and outsiders for the remaining holding periods is similar. There were 7,231 large-volume transactions by NYSE and Amex high-ranking insiders related to this holding period. Of this number, 6,037 (1,812 purchases and 4,225 sales) had a CRSP-assigned portfolio number. Of those, return data were insufficient for either our estimation or test periods for 387 purchases and 628 sales. Thus, our sample contained 5,022 insider transactions (1,425 purchases and 3,597 sales) for the two-week holding period.

Table 1. Sample Size and Description

Determination of sample, two-week holding period (insiders)	
Large-volume transactions by high-ranking insiders	7,231
Large-volume transactions by high-ranking insiders without CRSP portfolio numbers	1,194
Large-volume transactions by high-ranking insiders with CRSP portfolio numbers	6,037
Large-volume transactions by high-ranking insiders with CRSP portfolio numbers but inadequate CRSP return data	1,015
Large-volume transactions for high-ranking insiders studied[a]	5,022
Partial breakdown of sample by transaction type, two-week holding period (insiders)	
Purchase transactions with CRSP portfolio numbers	1,812
Purchase transactions with inadequate CRSP return data	387
Purchase transactions[a]	1,425
Sales transactions with CRSP portfolio numbers	4,225
Sales transactions with inadequate CRSP return data	628
Sales transactions[a]	3,597

[a]The number of insider observations for the two-week holding period. The numbers of observations vary between insider and outsider portfolios, and they vary across holding periods. For example, there are 1,359 purchases and 3,511 sales in the 2-week holding period for outsider portfolios (in contrast to 1,425 and 3,597 for insider portfolios), and there are 1,417 purchases and 3,576 sales in the 52-week holding period for insider portfolios.

Abnormal Returns, Firm-Size Control, and Transaction Costs

Our event-study methodology focuses on weekly returns, controls explicitly for firm size, and reflects adjustments for transaction costs. The first event week (i.e., $t = 1$) used in measuring abnormal returns to outsiders begins the Wednesday of the week following the week the related insider-trading data were entered into the CDA/Investnet database. For insiders, the first event week ($t = 1$) also begins on Wednesday of the week following the week of the insider transaction.

The market model was used to estimate expected returns from securities. In general, the market model parameters, α_i and β_i, were calculated for each estimation period more than 200 weeks long using ordinary least squares, excluding the 52 weeks before and 78 weeks after each event week. As a minimum, however, we required 100 weeks of return data for estimation purposes. The market model is

$$r_{i,t} = \alpha_i + \beta r_{m,t} + \varepsilon_{i,t} \tag{1}$$

from $t = -152$ to -53 and 79 to 178, where $r_{i,t}$ is the simple with-dividend return to security i in week t, $r_{m,t}$ is the simple with-dividend return to the equal-weighted portfolio of all NYSE stocks in week t, and $\varepsilon_{i,t}$ is the security-specific component of the return (i.e., the residual).[12]

Equation 2 gives the formula for calculating the basic prediction error (or abnormal return), $PE_{i,t}$, for security i in event week t.

$$PE_{i,t} = [r_{i,t} - (\alpha_i + \beta_i \times r_{m,t})] \times H \tag{2}$$

for $t = 1, \ldots, 52$, where $H = 1$ (-1) if the insider transaction is a purchase (sale). This equation ignores size and transaction costs.

The average purchase (sale) portfolio prediction error for event week t, APE_t, is the simple average of all purchase- (sale-) related security-prediction errors for that week:

$$APE_t = \left(\sum_{i=1}^{K_t} PE_{i,t} \right) / K_t \tag{3}$$

for event week $t = 1, \ldots, 52$, where K_t is the number of security-prediction errors in week t. The abnormal return to outsiders and insiders from event week t through holding-period week, T, CAPE(T), was calculated by summing the weekly average prediction errors:

$$CAPE(T) = \sum_{t=1}^{T} APE_t \tag{4}$$

for holding-period weeks $T = 2, 4, 13, 26, 39$, and 52.

Given parameter stationarity, market-model prediction errors have an expected value of zero for firms of any size, which avoids a firm-size bias (see Banz 1981, Brown, Barry, and French 1984, Cook and Rozeff 1984, Rozeff and Zaman, and Strong 1992). To ensure that our results are not driven by misspecification, we used a methodology similar to that of Seyhun (1992) to control explicitly for firm size by adjusting each $PE_{i,t}$. We subtracted the average prediction errors of control portfolios consisting of firms of similar size for each $PE_{i,t}$ to produce $SPE_{i,t}$ (see the appendix for further discussion). Next, we calculated the average size-adjusted prediction error for each event week, $ASPE_t$, using Equation 3 with $SPE_{i,t}$ substituted for $PE_{i,t}$. We then calculated the cumulative average size-adjusted prediction error for each holding-period week, CASPE(T), via Equation 4 with $ASPE_t$ substituted for APE_t. Note that CASPE(T), as defined above, ignores transaction costs.[13]

In calculating the second version of CASPE(T), we subtracted assumed transaction costs from each $SPE_{i,t}$. Subsequently, we calculated APE_t and CASPE(T). We used the estimated mean transaction costs defined by Stoll and Whaley (1983) and subtracted these mean values from the $SPE_{i,t}$.[14] The Stoll and Whaley mean transaction cost for a specific $SPE_{i,t}$ is the sum of the mean bid–ask spreads and the mean of two commissions (i.e., the round-trip costs) calculated with reference to the control portfolio reflecting the same size decile as the $SPE_{i,t}$ (see the appendix for a discussion of control portfolios). This procedure is conservative, in general, because it tends to produce very large assumed transaction costs.

RESULTS AND ANALYSIS

We report results related to insider profits (ignoring transaction costs) and outsider profits (both ignoring and net of transaction costs). We also discuss implications pertaining to semistrong-form market efficiency and related matters and the likely reason the results of our study are different from those of Seyhun and Rozeff–Zaman.

Insider Profits, Ignoring Transaction Costs

Abnormal returns, ignoring transaction costs, were available to large-volume, high-ranking NYSE and Amex firm insiders during our sample period. These results provide both a benchmark for viewing the significance of the abnormal returns available to outsiders and a prelude to discussing insider-trading behaviors, which are of interest to those who might mimic (actually or otherwise) or study these behaviors.

Table 2 shows CASPE values for the insiders for both purchases and sales. Results are reported for various holding periods, which begin in Event Week 1 and end with Holding-Period Week T = 2, 4, 13, 26, 39, and 52. The CASPE values for purchases grow to 7.72 percent after 26 weeks and to 11.67 percent after 52 weeks. For sales, these values are 5.51 percent and 8.52 percent after 26 and 52 weeks, respectively. As expected, most of the CASPE values in Table 2 are larger than the related values for outsiders (see Tables 3 and 4). Nevertheless, outsiders who mimicked insiders still had opportunities to earn significant abnormal returns during the sample period. Note also that these values are smaller than, but consistent with, those of Seyhun (1992), who studied purchase and sale transactions for NYSE, Amex, and Nasdaq firms from 1984 to 1989. He reported cumulative average abnormal returns for combined purchases and sales and for 26- and 52-week holding periods of 9.06 percent and 14.8 percent, respectively.[15]

We used t-tests to assess the statistical significance of the CASPE values. The CASPE values in Table 2 are significant for purchases at 0.1 percent for all holding periods from 4 weeks to 52 weeks and at 1 percent for the two-week holding period. For sales, these values also are significant at the 0.1 percent

Table 2. Cumulative Average Size-Adjusted Prediction Errors in Excess of Equal-Weighted, Dividend-Adjusted Index for High-Ranking Insiders: NYSE and Amex Firms, 1985–90
(t-statistics in parentheses)

Holding Period (weeks)	Actual		Annualized	
	Purchases	Sales	Purchases	Sales
2	0.88%***	–0.75%	22.98%	–19.47%
	(2.50)	(–2.38)		
4	1.74****	–0.31	22.63	–3.97
	(3.70)	(–0.85)		
13	4.64****	2.41****	18.58	9.65
	(5.55)	(3.77)		
26	7.72****	5.51****	15.44	11.02
	(6.15)	(6.06)		
39	9.35****	6.53****	12.47	8.71
	(6.44)	(5.73)		
52	11.67****	8.52****	11.67	8.52
	(6.87)	(6.45)		

Notes: There are 1,425 observations for purchases and 3,597 observations for sales in the two-week holding period. All t-tests are one-tailed.

***Significant at the 1 percent level.
****Significant at the 0.1 percent level.

level for all holding periods following the four-week holding period. Additionally, these values appear to be highly significant from an economic perspective. Consistent with Seyhun (1986 and 1992), the cumulative average abnormal returns for insider purchases are consistently larger than for insider sales.

Given the magnitudes of the abnormal returns available to insiders, questions about the legality and the nature of their trades are pertinent to individuals who might mimic (actually or academically) or study their behaviors. Park, Lang, and Loeb (1995) concluded that insiders generally avoid engaging in transactions immediately preceding the public release of annual earnings information. They believe that their evidence supports the hypothesis that regulatory review affects insider-trading activity. Sanders and Zdanowicz (1992) and Bettis (1995) drew similar conclusions with respect to takeover-target insider activity in the periods preceding takeover announcements. Thus, the extant evidence suggests that insider trades are not illegal behaviors occurring in response to insider information about earnings and takeovers. The courts have been successful in finding individuals guilty of illegal insider trading primarily in cases related to earnings and changes in control (see Seyhun 1992). It is reasonable to infer that other trades insiders make are largely based on their expectations about the general prospects of their firms (i.e., expectations related to private information about, and intimate knowledge of, their firms' strategies and operating environments in general).

Circumstantial evidence, however, indicates that some insider trading is illegal. For example, Gosnell, Keown, and Pinkerton (1992) and Bradley and Seyhun (1993) extended Loderer and Sheehan's (1989) work and found evidence that insiders engage in significant sales of their firms' shares in the months preceding bankruptcy petitions. Also, John and Lang (1991) studied the information content of insider trading around dividend announcements and concluded that insiders appear to act on private value-relevant information that is likely to be conveyed by dividends. In this regard, there may be a great deal of undetected illegal insider trading, especially in cases in which the courts would have difficulty establishing guilt.

Outsider Profits, Ignoring Transaction Costs

Could outsiders have earned abnormal returns by mimicking the trades of insiders—again ignoring transaction costs? Table 3 shows that the related CASPE values grew steadily from the 2-week holding period to the 52-week holding period. For purchases, the CASPE value was 7.01 percent after 26 weeks and grew steadily to 11.00 percent after 52 weeks. For sales, the CASPE value was 5.49 percent after 26 weeks and grew to 8.28 percent after 52 weeks. The CASPE values in Table 3 for purchases are significant at the 0.1 percent level for all holding periods from 2 weeks to 52 weeks. For sales, these

Table 3. Cumulative Average Size-Adjusted Prediction Errors in Excess of Equally Weighted, Dividend-Adjusted Index for Outsiders Mimicking High-Ranking Insiders: NYSE and Amex Firms, 1985–90
(*t*-statistics in parentheses)

Holding Period (weeks)	Actual		Annualized	
	Purchases	Sales	Purchases	Sales
2	0.93%****	0.40%*	24.19%	10.31%
	(2.98)	(1.63)		
4	1.62****	1.08****	21.05	14.08
	(3.66)	(3.17)		
13	4.45****	3.25****	17.81	13.02
	(5.25)	(5.00)		
26	7.01****	5.49****	14.02	10.98
	(5.92)	(5.96)		
39	7.78****	6.47****	10.38	8.63
	(5.42)	(5.65)		
52	11.00****	8.28****	11.00	8.28
	(6.37)	(6.25)		

Notes: There are 1,359 observations for purchases and 3,511 observations for sales in the two-week holding period. All *t*-tests are one-tailed.

*Significant at the 10 percent level.
****Significant at the 0.1 percent level.

values are significant at the 0.1 percent level for all holding periods from 4 to 52 weeks and at the 10 percent level for the 2-week holding period. Consistent with the results for insiders, purchases were uniformly more profitable to outsiders than sales.

Outsider Profits and Semistrong-Form Efficiency

Transaction-cost-adjusted CASPE values are reported in Table 4. For sales, the CASPE values increased from 2.95 percent after 26 weeks to 6.96 percent after 52 weeks for purchases and from 2.05 percent after 26 weeks to 4.86 percent after 52 weeks. Again, the CASPE values for purchases strictly dominate those for sales. The CASPE values for both purchases and sales still are significant for outsiders at the 0.1 percent level for the 52-week holding period. For purchases, these values also are significant, at the 1 percent level, for both the 26- and 39-week holding periods. For sales, the CASPE values are significant at 5 percent (1 percent) for the 26- (39-) week holding period. The CASPE values referred to above are extremely significant economically in addition to being statistically significant.

The Table 4 results imply that traders could have profited from using publicly available insider-trading data during our sample period (consistent with the findings of Jaffe), that anecdotal evidence of the type cited could have been credible during this period, and that the market was not semistrong-form efficient for all of this period. The results also suggest that conclusions of this sort may hold in the contemporary environment. This observation is of importance because it may lead to fruitful attempts (1) to mimic (actually or academically) the behaviors of insiders, (2) to study these behaviors, and (3) to identify the factors that lead to semistrong-form market inefficiency in the current setting.

The Table 4 results also imply that the conclusions of Seyhun and Rozeff–Zaman do not generalize to our sample period. This lack of general applicability raises the issue of why the results of our study are so dramatically different from those of the earlier studies. The most obvious possible reason is the differing reporting-delay time of the three studies. Recall that Seyhun assumed a reporting delay of three months, on average, after the insider transaction occurred, and Rozeff and Zaman assumed a reporting delay of two months after the end of the month in which the insider transaction occurred. Therefore, for each insider transaction, the assumed reporting delays were three months and approximately two and a half months, respectively, in the earlier studies. In contrast, our mean reporting delay is approximately three weeks and three days.

Given that our outsiders, on average, could begin earning profits, which would offset transaction costs completely, by the end of the 13-week holding period and also could begin avoiding

Table 4. Transaction-Cost-Adjusted Cumulative Average Size-Adjusted Prediction Errors in Excess of Equal-Weighted, Dividend-Adjusted Index for Outsiders Mimicking High-Ranking Insiders: NYSE and Amex Firms, 1985–90
(*t*-statistics in parentheses)

Holding Period (weeks)	Actual		Annualized	
	Purchases	Sales	Purchases	Sales
2	–4.86%	–4.68%	–126.41%	–121.56%
	(–12.71)	(–15.65)		
4	–3.19	–3.04	–41.47	–39.50
	(–6.46)	(–7.96)		
13	0.30	–0.28	1.21	–1.12
	(0.34)	(–0.41)		
26	2.95***	2.05**	5.89	4.10
	(2.44)	(2.19)		
39	3.70***	3.03***	4.94	4.04
	(2.54)	(2.62)		
52	6.96****	4.86****	6.96	4.86
	(3.99)	(3.63)		

Notes: There are 1,359 observations for purchases and 3,511 observations for sales in the two-week holding period. All *t*-tests are one-tailed.

**Significant at the 5 percent level.

***Significant at the 1 percent level.

****Significant at the 0.1 percent level.

losses, which would virtually eliminate these costs by the end of this period (i.e., compare the Table 3 and Table 4 results for holding periods $T = 2$, 4, and 13), it is clear that the longer the assumed reporting delay, the smaller the outsider abnormal returns net of transaction costs (at least over the remaining holding periods $T = 26$, 39, and 52).

To focus more clearly on the effects of reporting delays, we assumed a reporting delay similar to those of the earlier studies (13 weeks) and recalculated the CASPE values, net of transaction costs, for outsiders. The results are reported in Table 5. In this case, the CASPE values increase from 0.56 percent after 26 weeks to 4.20 percent after 52 weeks for purchases. They increase from 0.62 percent after 26 weeks to 4.76 percent after 52 weeks for sales. From an overall perspective, the Table 4 results strictly dominate those of Table 5. Thus, the effect of the longer assumed reporting delay is to reduce abnormal returns to outsiders. Furthermore, this analysis implies that the difference between the results of this study and those of the earlier ones is likely to be at least partially attributable to the differing reporting delays. In this regard, if the studies of Seyhun and Rozeff–Zaman were replicated over our sample period and using CDA/Investnet data and our reporting delays, their findings might more closely match ours.

Similar to the results of Table 4, the CASPE values of Table 5 are significant at the 5 percent level for purchases and sales for the 39-week holding period (the related Table 4 values are significant at the 1 percent level). They are significant at 1 percent (0.1 percent) for purchases (sales) for the 52-week holding period (the related Table 4 values are significant at 0.1 percent). For the first time, the purchases results do not strictly dominate the sales results. This phenomenon is a function of the assumed reporting delay and the differing patterns of potential abnormal returns from purchases and sales. In this regard, the superiority of using insider-purchases data (versus insider-sales data) to earn abnormal returns through mimicking behavior is likely to depend on the timeliness of the mimicking action. The Table 5 CASPE values also are extremely significant economically in addition to being statistically significant. Thus, the conclusions drawn in relation to our Table 4 results also apply in relation to the Table 5 findings.

IMPLICATIONS FOR FUTURE RESEARCH

Several recent studies have concluded that outsiders cannot earn abnormal returns by mimicking the trades of insiders after the latter report their trades. This study's results suggest that outsiders can earn such returns. From an overall perspective, it suggests that traders can profit from using publicly

Table 5. Transaction-Cost-Adjusted Cumulative Average Size-Adjusted Prediction Errors in Excess of Equal-Weighted, Dividend-Adjusted Index, Assuming 13-Week Outsider-Trading Delay for Outsiders Mimicking High-Ranking Insiders: NYSE and Amex Firms, 1985–90
(*t*-statistics in parentheses)

Holding Period (weeks)	Actual		Annualized	
	Purchases	Sales	Purchases	Sales
2	–5.39%	–4.45%	–140.18%	–115.77%
	(–14.23)	(–13.90)		
4	–4.22	–3.15	–54.89	–40.91
	(–7.99)	(–7.82)		
13	–1.18	–0.46	–4.72	–1.82
	(–1.33)	(–0.68)		
26	0.56	0.62	1.11	1.25
	(0.46)	(0.65)		
39	2.93**	2.66**	3.90	3.54
	(1.96)	(2.27)		
52	4.20***	4.76****	4.20	4.76
	(2.37)	(3.52)		

Notes: There are 1,359 observations for purchases and 3,511 observations for sales in the two-week holding period. All *t*-tests are one-tailed.

**Significant at the 5 percent level.
***Significant at the 1 percent level.
****Significant at the 0.1 percent level.

available insider-trading data, lends credence to the anecdotal evidence in the financial press, and implies that the market is not efficient in the semi-strong form.

Most academic researchers investigating the relationship between insider trades and publicly released information focus on corporate disclosures occurring relatively soon after the insider actions. As shown in this study, insiders' abnormal returns appear to persist for extended time periods. Thus, a fruitful area of future research would be to investigate this relationship while focusing on information that is released publicly longer after insider trades. Other possible areas of future research include comparison of the abnormal returns possible through mimicking behavior with those available through fundamental analysis and attempts to determine the extent to which these techniques work because of possible associations with insider-trading activities. Finally, in general, the results of this study may lead to fruitful attempts to study mimicking behavior, the specific behaviors of insiders that produce abnormal returns, and the factors that lead to semistrong-form market inefficiency (or efficiency).[16]

APPENDIX: Control Portfolios

The CRSP daily returns tape has a portfolio number, from 1 to 10, assigned to each company each year. Using these portfolio numbers each year, we created 10 control portfolios ($p = 1, \ldots, 10$) using a random selection of 200 firms (excluding sample firms) from each size decile for which complete data were available.[17] The market model was used to estimate the prediction errors for each of the portfolio stocks each week. The prediction error of control portfolio p in a given week t, $PE_{p,t}$, is the equally weighted arithmetic average of the prediction errors of the stocks in portfolio p in that week. $PE_{p,t}$ is then treated as the benchmark return for each of the sample firms in the same size decile. The size-adjusted prediction error, $SPE_{i,t}$, is given by

$$SPE_{i,t} = PE_{i,t} - PE_{p,t}, \tag{5}$$

where

$PE_{i,t}$ = estimated prediction error for test firm security i in week t

$PE_{p,t}$ = estimated prediction error for i's control portfolio in week t ($p = 1, \ldots, 10$)

The use of $SPE_{i,t}$ in Equations 3 and 4 is explained in the main body of the text.

NOTES

1. In this study, corporate insiders are those persons who are required to report their transactions pursuant to Section 16(b) of the Securities Exchange Act of 1934. Generally, this group includes directors, officers, beneficial owners (owners of 10 percent or more of the outstanding voting stock), trustees, and affiliated persons.
2. Unless specifically noted otherwise, references to Seyhun are to Seyhun 1986.

   Fowler and Rorke (1988) also studied the ability of insiders and outsiders to earn abnormal returns. They used more-complex selection criteria than either Seyhun or Rozeff–Zaman and found abnormal returns for both insiders and outsiders. Also, they controlled for size. Because their study is based on transactions from the Toronto Stock Exchange, reporting is governed by a different set of regulations and requirements than is reporting related to the NYSE and Amex. For this reason, we do not discuss Fowler's and Rorke's work in the body of the text.
3. Although the findings of Seyhun and Rozeff–Zaman are the same with respect to outsiders, Rozeff and Zaman concluded that the Seyhun "finding of no insider trading anomaly is less than fully convincing because it can be associated with the initial already-low level of insider trading profits that he reports" (p. 26). Also, in an initial step, they applied the stock-selection criterion and methodology of Jaffe in a different sample period and found that outsiders can earn abnormal returns. When they extended their methodology to control for size and earnings/price effects, however, no abnormal returns to outsiders were found.
4. The Insider Trading Monitor went online in June 1984. A number of other sources of insider-trading data became available to investors at about the same time (e.g., the Washington Service, Vicker's Stock Research, and Value Line).
5. As indicated, the CDA/Investnet keypunch date usually occurs one or two days following the SEC filing date. The mean number of days between insider transactions and keypunch dates for our sample (i.e., our reporting delay) is approximately three weeks and three days. Thus, the mean number of days between insider transactions and the dates that outsiders might have begun trading is much shorter in our study than in the studies of Seyhun and Rozeff–Zaman.
6. Seyhun (1992) investigated changes in the regulatory environment governing insiders in three subperiods of the 1970s and 1980s. He found that high-ranking insiders who bought or sold more than 10,000 shares earned significant abnormal returns. He also conducted a brief analysis to determine whether Nasdaq outsiders could mimic the trades of insiders and earn abnormal returns (because of the magnitudes of the abnormal returns to insiders he found). As implied, his analysis was of secondary purpose and was limited in scope. For example, he did not separate purchases and sales, did not measure performance across multiple holding periods, limited his investigation to a subsample of firms for which bid–ask spread data were available from the Nasdaq tapes, and made the same assumptions about the availability of the insider-trading data as in his 1986 paper. Given these factors, it is not surprising that he did not find significant abnormal returns for Nasdaq outsiders. We did not review his results in the body of the text for these reasons.
7. The use of this procedure is reported in various CDA/Investnet newsletters to clients during the period September 1987 to December 1990.

8. Section 16(b) of the 1934 Securities Exchange Act prevents short selling securities and forces insiders who acquire shares to hold the shares for at least six months before selling them or risk being forced to disgorge related profits. Thus, calculating abnormal returns in relation to sell decisions is a way of identifying the losses that insiders have avoided by selling before periods of security price declines. In this sense, avoiding losses by selling shares already in their possession is just as important as buying them before prices rise.
9. The CDA/Investnet database identifies option-related sales, but we found that fewer than 1 percent of the sales of more than 10,000 shares were option related, and consequently, these sales were not excluded from our sample.
10. Given that our control portfolios were formed on a weekly basis (in relation to new, or newly observed, insider transactions) and that we separated purchases and sales, we could not use the portfolio-formation methodology of Jaffe and of Rozeff and Zaman. Those procedures would have produced an insufficient sample size in our study. Our methodology appears to be at least as rigorous as that of those authors. Additionally, as indicated, it allowed us to capture abnormal returns from intramonth transactions.
11. The Securities Enforcement Remedies and Penny Stock Reform Act of 1990 became effective in May 1991. This law drastically changed the reporting environment for insiders. For example, it changes the definition of an "insider" under Section 16 of the Securities Exchange Act of 1934, and it provides for penalties and fines against insiders and their corporations for delinquent filings.
12. Weekly returns were accumulated for each stock using daily returns (beginning with Day 0) as follows:

$$R_w = \left[\sum_{t=0}^{6}(1 + R_t)\right] - 1,$$

where R_t = return for day t.

13. Brown and Warner (1985) suggested that an equal-weighted index is more appropriate than a value-weighted index.
14. We tested for serial correlation for each holding-period week using the APE_ts. Autocorrelations were insignificant in each of the first three lag weeks, as well as in Lag Weeks 6, 12, and 18. These results indicate that our prediction errors are uncorrelated. Thus, the general formula for the variance of a sum is used to compute the standard error of both versions of CASPE(T).
15. The CASPE values reported in Table 2 appear to be positive increasing functions of time (e.g., for long positions, the 52-week CASPE value is greater than the 39-week CASPE value, which is greater than the 26-week CASPE value, etc.). Thus, our results could stem from model misspecification. To consider this possibility, we calculated the CASPE values for insiders for 44- and 48-week holding periods and used t-tests to assess the significance of these values. The resulting long-position CASPE values are 10.93 percent and 11.81 percent, respectively, and the related t-statistics are 6.96 and 6.83 (both significant at the 0.1 percent level). The short-position CASPE values are 7.88 percent and 8.48 percent, respectively, and the related t-statistics are 6.21 and 6.56 (both significant at the 0.1 percent level). These results imply that the CASPE values stabilize between 44 and 52 weeks (and, thus, are not positive increasing functions of time). Consequently, our results are not likely to be the result of model misspecification.
16. We thank William Duncan, Robert Jennings, and Gerald Salamon for their helpful comments. We also thank our graduate assistant, Gayatri Gopalan, for her diligent assistance. We thank CDA/Investnet for supplying our insider-trading data.
17. Each portfolio was re-formed at the end of each year during the estimation period. Obtaining the size portfolios and assigning stocks by year is important because of secular changes in size levels.

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