Are Insider Sales Always Bad News? Evidence On Large Sales By Key Insiders

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Abstract

Investors often scrutinize stock trades by corporate insiders, hoping to infer the nature of any privileged information which may have motivated the trades. Conventional wisdom suggests that sales of stock by insiders reveal negative information; this interpretation is supported by empirical work such as the series of papers by Seyhun. However, this common interpretation fails to distinguish between sales by atomistic insiders and sales by controlling blockholders. In this paper, I present evidence which suggests that sales by controlling insiders should not be considered bad news. Using both a series of logit regressions and traditional event-study tests, I examine the relationship between a firm's performance and the willingness of its controlling shareholder to sell a significant proportion of his shares. I find that firm value is just as likely to rise on the news of large insider sales as it is to fall, so that large sales need not imply negative private information. One possible explanation for a positive response to a controlling blockholder's large sale is that such a sale makes the insider vulnerable to meaningful oversight by outside shareholders. Thus, a large sale may be a signal of the insider's willingness to expose himself to shareholder monitoring and discipline. However, regardless of the interpretation, the empirical evidence presented in the paper forces the conclusion that it is inappropriate to interpret all insider sales as bad news: insider sales occur in a variety of contexts, and creating buy/sell rules which ignore those contexts is simplistic and erroneous.

Introduction

Investors often scrutinize the trades insiders make in the stocks of their own firms. Advisory services have been created to monitor insider activity; mutual funds have been established to emulate it. This interest is at least partially motivated by the conventional wisdom which suggests that insider trades are responses to private information, and that outsiders can infer the nature of that information by monitoring insiders' trading behavior. The simplest inference is that insider purchases signal good news about the firm while insider sales signal bad news.[1] However, such simple trading strategies based on insider trades are doomed to failure if they ignore the context in which those trades occur.

In this paper, I attempt to provide some of the necessary context for the consideration of insider sales by examining a very specific type of sale: voluntary divestitures by controlling insiders. Most of the insider trading literature to date has focused either on voluntary sales with no control implications (such as the series of papers by Seyhun) [2] or on involuntary control divestitures (such as Johnson, Magee, Nagarajan, and Newman [1985]). These two strains of research have found diametrically opposed relationships between an insider's release of stock and the subsequent price reaction; these contrasting results suggest that studying a hybrid transaction can increase our understanding of the market's response to insider sales.

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Seyhun's (1986) study of a large sample of insider trades supports conventional wisdom, finding that stock price falls after sales by insiders. He also finds that the magnitude of the abnormal profit which insiders earn from

their trading is positively related both to the decision-making power of the insider (so that insiders with access to better information *trade* on better information) and to the proportion of the firm traded (so that better information leads to heavier trading). However, there is a possible link here which should be considered: Selling a larger proportion of the firm today may affect an insider's decision-making power tomorrow.

The identity of the insider and the proportion of the firm traded are characteristics determining the control implications of the sale. However, trades in Seyhun's sample are not evaluated as control events. The market may evaluate insider transactions very differently if they alter the control hierarchy of the firm, and the transactions with the greatest potential to alter this hierarchy are those involving higher-ranking insiders and those in which a larger proportion of the firm's voting rights are traded. Yet a study concentrating on the control potential of exactly these factors finds a result quite different from Seyhun's. Johnson, *et al.* find that stock prices rose significantly after the unexpected deaths of certain managers who had owned controlling amounts of stock in their firms. These insiders had been using their voting control to insulate themselves from shareholder discipline, running their firms as "personal fiefdoms." Upon the deaths of these insiders, these fiefdoms were emancipated, and firm value rose accordingly. Clearly, and in contrast to the sales in Seyhun's study, these control transfers were involuntary. However, any transfers which improve the ability of outsiders to meaningfully monitor managers should be good news for a firm's shareholders. Thus, explicit consideration of potential control implications should improve our understanding of insiders' *voluntary* stock sales.

In this paper, I consider large insider sales as control events. I identify specific, key management insiders who have large blocks of stock in their firms, and examine the effect that significant sales from these blocks have on the firms' stock prices. If sales are merely reactions to negative inside information, stock price should go down when these insiders sell stock, as the conventional interpretation suggests; if, however, the sales open the firm to improved outside oversight, share value should rise. The predicted response to an insider sale is thus ambiguous, and depends upon the control implications of that sale. To provide some context for the examination of these control implications, I also consider how characteristics of the firm and characteristics of the insider himself are related to the insider's willingness to sell significant amounts of stock. I find that several characteristics, such as the manager's age and the firm's level of operating cash flow, may be systematically related to the probability that a manager will make such a sale.

The paper proceeds as follows. Section 2 describes the sample selection procedures and explains the benchmark ownership levels used throughout the paper to quantify control potential. Section 3 presents the event-study evidence on stock price reaction to significant insider sales events. It then begins to provide the context for the interpretation of these results by considering the control implications of these sales. Section 4 presents additional context by linking accounting measures of firm performance with the likelihood that a manager will make a large sale. Section 5 concludes.

Sample Selection and Control Benchmarks

I use two samples to study large insider sales. I will refer to these as the primary sample and the event-study sample.

The primary sample is composed of firms with potentially dominant insiders. These insiders had both significant blocks of stock and significant tenure with their firms, suggesting that they had control positions which could be affected by sales of stock. I identified this sample by first selecting from the 1980 *Value Line* all the firms whose "insider ownership" exceeded 10%.[3] From these 753 firms, I removed those firms which were not also listed in the 1994 *Value Line*. Finally, for the 318 firms listed in both the 1980 and 1994 *Value Line*, I used proxy statements to identify specifically the inside owners, removing any firms which did not have at least one owner holding 4.7% or more of one class of the firm's voting securities.[4] The 81 remaining firms with concentrated, persistent insider ownership make up the primary sample.[5] This is the sample which I will use in section 4 to test the relationship between firm characteristics and the probability of large insider sales.[6]

To consider value changes following large share sales, I use a subset of this primary sample. For each of the 318 firms, I identified every insider who owned more than 4.7% of his firm's stock. I then searched every monthly issue of the *Official Summary of Transactions and Holdings*[7] from December 1979 through January 1987, identifying every large sale (approximately 20,000 shares or more) by a tracked insider. Each of these sales was a possible event. For each firm with at least one such sale, I checked every proxy listed in the *SEC Index* between 1980 and 1988 to verify that the insider had at least a three percentage point change in ownership over the period. I required that this significant ownership change resulted from a very few sales events over a brief time, eliminating sales programs which dissipated holdings gradually. Finally, to focus on the valuation consequences of a voluntary reduction in a control block instead of the consequences of a retirement, I required that the selling insider remain with the firm for at least two years after the sales events. The 10 firms that meet these criteria are described in Table 1.

Table 1
Sample firms (n=10)

Firm Name	Owner	Initial Ownership	Final Ownership	Event Dates
Applied Magnetics	Harold Frank	40.60%	20.10%	7-20-82
Cummins Engine	J. Irwin Miller	13.62	6.83	10-13-83
Knight-Ridder	James L. Knight	28.65	14.11	5-13-85
				5-16-85*
				5-20-85
Loctite	Robert H. Kreible	21.81	7.80	11-6-85
Loew's	Lawrence A. Tisch	22.00	16.80	5-20-85
Meredith	E.T. Meredith III 17.70	14.50	2-9-84	
Outboard	MarineRalph Evinrude	12.38	6.57	6-8-84
Stop and Shop	Sidney H. Rabb	10.01	5.55	9-7-83*
				10-5-83
Winn Dixie	James E. Davis	18.78	9.07	1-2-81
W.W. Grainger	David W. Grainger	23.39	17.51	4-25-83

*This is the date of the largest sale. It was used in the event studies using only one event date per firm.

In each of the tests on both samples, a specific ownership level is defined as a control benchmark. A benchmark represents a threshold above which the manager is presumed to be insulated from outside shareholder discipline, but below which he is vulnerable. Thus, a sale which causes a manager's ownership to fall below the threshold could affect the control structure of the firm. An obvious candidate for such a threshold is 50%. However, Holderness and Sheehan (1988) point out that holdings above 50% are inefficient for a blockholder, since they limit the amount of his potential diversification without strengthening his decision-making power. In addition, other research suggests that much smaller voting concentrations can affect control significantly. For example, Morck, Shleifer, and Vishny (1988), in their study of the relationship between ownership and Tobin's q, find that voting concentrations of 5% and 25% are important control "turning points." As a manager's ownership rises above 25%, his interests may become aligned with those of the outside shareholders, causing firm value to rise with insider ownership. However, a manager with ownership below 25% may have enough voting concentration to retain control despite any misalignment of his interests with his principals'; this "entrenchment" effect, which depresses firm value, begins at insider ownership levels as low as 5%. Further evidence of the significance of voting blocks of less than 50% is found in Barclay and Holderness (1991). Their study shows that negotiated block trades involving only 10% to 15% of the firm's stock are associated with significant management turnover, suggesting that the new blockholder is able to influence the choice of management. Finally, even the SEC recognizes the control potential of holdings as low as

5%: In 1977, they lowered their benchmark for mandatory reporting from 10% to 5%. Given this evidence, the empirical tests which follow use a set of benchmark ownership levels below 50%.

Each test considers three possible benchmarks: 5%, 10%, and 25%. I use multiple benchmarks for several reasons. First, the relevant control threshold is probably firm-specific. Without a theory to guide the identification of each firm's threshold, I chose to use ownership levels suggested by prior research such as that discussed above. Second, considering several benchmarks may allow us to identify different behavior around different thresholds (as Morck, Shleifer, and Vishny [1988] found). Finally, using multiple thresholds allows more firms to be included in the samples, increasing the power of the empirical tests.

Stock Price Behavior after Insider Sales

Event study tests

In this section, I present evidence on the behavior of stock prices after a sale by a controlling insider. Each of these sales events caused the insider's holdings to fall below a control threshold. If these sales were interpreted as being motivated by negative private information, we would expect stock prices to fall after the sale. If, however, the market anticipated improved oversight after the sale, price could rise. I performed a traditional event study to study the price response, using both a market model (with the CRSP equally weighted average as the market proxy) and a decile-adjusted return model to calculate abnormal returns. [8] Because two firms' sales events were spread over several days, tests for both models used the prediction error for the single largest selling day, the sum of the prediction errors on all selling dates, and the cumulative prediction error over the entire selling period. (This methodology follows Partch [1987].) Both one- and two-day event periods were used, where the one-day tests used the sales dates from the *Official Summary*, and the two-day tests added the next day.

For the market model, the estimation period was 110 days, beginning 200 days before the event date. (This also follows Partch [1987].) The test statistic was:

$$t = \frac{1}{\sqrt{N}} \sum_{j=1}^{N} \sum_{t=t1}^{t2} PE_{jt} / \left(\sum_{t=t1}^{t2} \text{var}(PE_{jt}) \right)^{1/2}$$

Here, t_1 is the first day of the selling period, t_2 is the last day, N is the number of firms, and var(PE_{jt}) is the variance of the prediction error of firm j on day t. This variance was calculated as:

$$V_j^2 \left[1 + \frac{1}{ED} + (R_{mt} - R_m)^2 / \sum_{i=1}^{ED} (R_{mi} - R_m)^2 \right].$$

In this expression, V_j^2 is the residual variance from the market model regression, ED is the number of days in the estimation period (110), R_{mt} is the market return on day t, and R_m is the mean market return over the estimation period.

For the decile model, the prediction error for firm j on day t was simply the firm's return minus the decile's return for day t. The variance of the prediction error was the residual variance from the decile adjustment, V_j^2. For the multi-day tests, this variance was simply multiplied by the number of days in the test period to determine the denominator of the test statistic. (This assumes that the series of decile-adjusted returns exhibits no serial correlation. [9])

The results from the event tests are presented in Table 2. Results for the firms whose sales events happened over several days are presented separately for the multi-day tests. (Firms having only one sales date are not affected by the adjustments made in panels (B) and (C) below.)

Table 2 reveals no clear pattern to the stock-price response to these large insider sales. Only 16 of the 56 individual standardized errors (29%) have absolute values greater than one; [10] they come from six of the ten firms.

Only the test statistics for the one-day cumulative models are significant at conventional significance levels (p for the market model is .125; p for the decile model is .084).

Table 2
(A) Standardized prediction errors[11] for largest sale day

	Market model		Decile model	
	1-day event period	2-day event period	1-day event period	2-day event period
Averages	0.0989	-0.3000	0.3332	-0.3924
Individual firms:				
Applied Magnetics	-1.0100	-2.5300	-0.9116	-2.3375
Cummins Engine	0.1210	-0.2474	0.1745	-0.1713
Knight-Ridder	-0.4011	0.5615	-0.5711	0.4506
Loctite	-0.5284	-0.4999	-0.6962	-0.7706
Loew's	0.3322	0.0798	0.7839	-0.0899
Meredith	-0.1054	1.0850	0.4447	1.1312
Outboard Marine	-0.2580	0.5612	-0.1105	0.6331
Stop & Shop	-0.9407	-0.0848	-0.7681	0.2260
Winn Dixie	0.6728	-0.5212	0.1493	-1.2513
W.W. Grainger	2.4303	0.6472	2.5587	0.9388

(B) Standardized prediction errors for sum of all sales days

	Market model		Decile model	
	1-day event period	2-day event period	1-day event period	2-day event period
Averages	0.2510	0.0807	0.4951	-0.0677
Individual firms:				
Knight-Ridder	0.0294	1.3506	-0.0962	1.1265
Stop and Shop	-0.8903	0.3299	-0.7310	0.5770

(C) Standardized prediction errors for cumulative period

	Market model		Decile model	
	1-day event period	2-day event period	1-day event period	2-day event period
Averages	1.2275	0.1840	1.5167	0.0835
Individual firms:				
Knight-Ridder	1.6446	1.2044	1.4251	0.9882
Stop and Shop	0.5825	0.8029	0.9782	1.1935

While these event study tests failed to find evidence of significant positive response to large sales by insiders, they also failed to find the large negative response predicted by the conventional wisdom that insider sales are bad news. 11 of the 12 test statistics are positive. 32 of 56 (57%) firm statistics are positive; a test on the proportion of positive statistics cannot reject the null hypothesis that π=.50. In addition, between four and seven of the ten statistics per test are positive, and the average magnitude of the positive statistics is greater than the average magnitude of the negative.[12] Therefore it appears just as likely that a large insider sale is good news as bad. In the next section, I consider a possible explanation for these ambiguous results.

Control transition consideration of the sample firms

With one exception, the results just presented are not statistically significant. Both methodological and theoretical explanations could be offered for this lack of significance.[13] In this section, I discuss a critical theoretical consideration: the control transition status of the sample firms at the time of the event. Large percentage sales are often associated with control transitions (see, for example, Barclay and Holderness [1991]). Requiring that the selling manager remain with the firm for two years after the sale may not have been adequate to ensure management continuity. If the sale event was simply an expected step in a predetermined control transition, we should expect no abnormal return on the date of the actual sale. Inclusion of such firms in the sample would make it more difficult to detect any post-sale price response.

To determine if there were any control issues affecting the sample firms, I searched the *Wall Street Journal Corporate Index* for news items on all sample firms between 1978 and 1988.[14] This search revealed that four of the sample firms were going through periods of profound managerial change, involving the tracked executive, over the sample period. [15] Only three of the sample firms appear to have been completely uncontaminated by management transitions. To determine if these control issues may have caused systematic differences in the two groups of firms over the event period, I plotted cumulative abnormal returns for the entire sample, the control-issue subsample (seven firms), and the no control-issue subsample (3 firms). These plots are presented in Figure 1.

Figure 1 (A). Cumulative abnormal returns, days –91 through –21

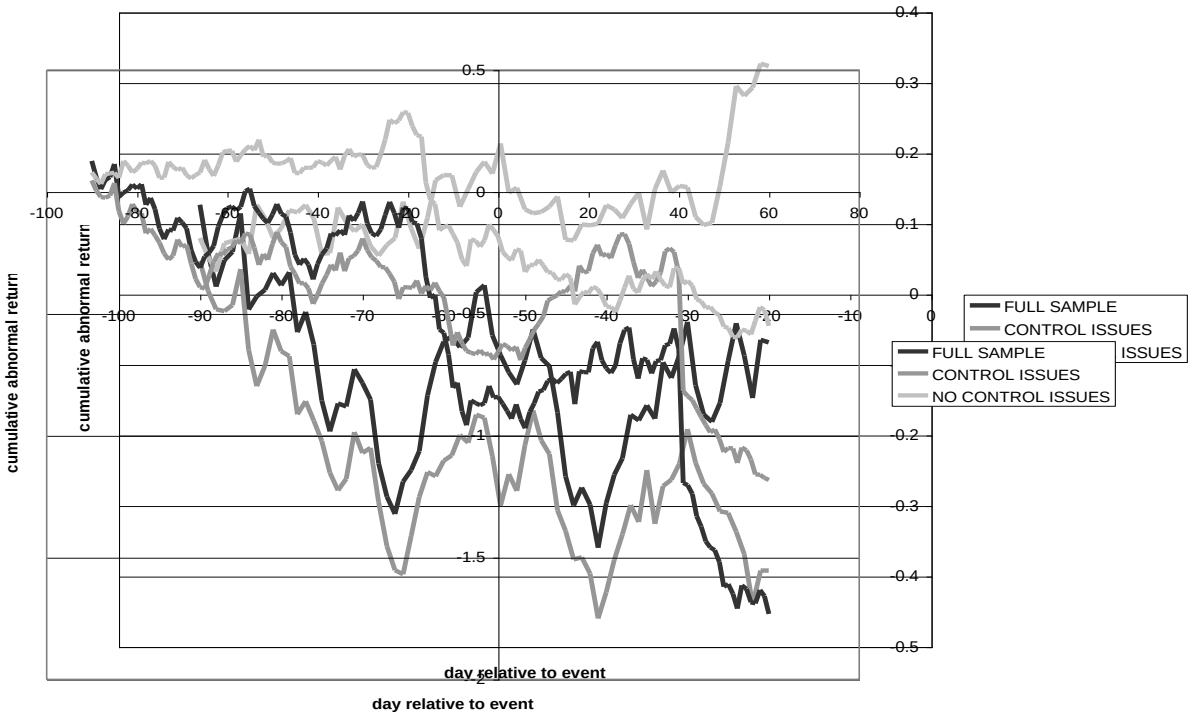

Figure 1 (B). Cumulative abnormal returns, days 0 through 30

Figure 1 (C). Cumulative abnormal returns, days -91 through 60

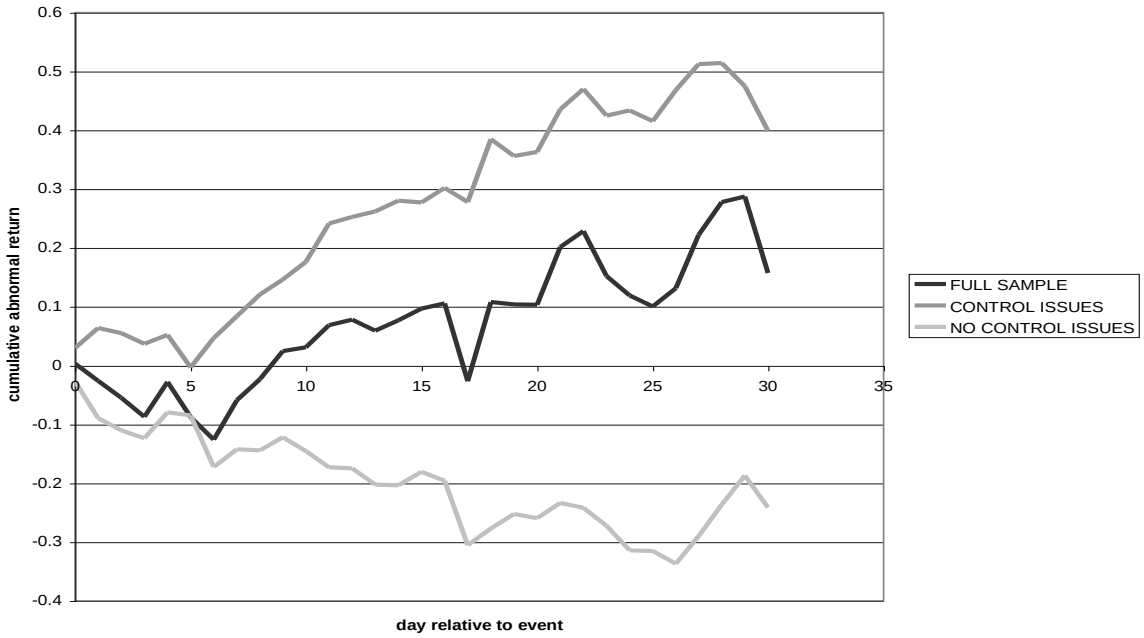

Figure 1. All of the graphs in Figure 1 show the cumulative average returns for the 10 event-study sample firms ("full sample"), for the seven firms which have control issues at the time of the insider share sale ("control issues"), and for the three firms with no control issues ("no control issues").

Figure 1 shows that the two subsamples behave very differently. The magnitudes of the CARs for the control-issue subsample are greater than for the no-issues subsample throughout the [-90, 30] period. From 90 to 20 days before the event, CARs for the control-issue subsample are consistently negative; CARs for the no-issues subsample are more volatile and consistently positive. However, over the 20 days before the event, the no-issues subsample's CARs begin to decline drastically, and after the sales event are consistently negative. In fact, the behavior of the subsamples after the event is completely reversed from the earlier pattern, with the CARS for the control-issue subsample increasingly positive and those for the no-issue subsample negative. [16]

One possible interpretation of these results is that conventional wisdom holds for the no-control-issue subsample: Sales events by insiders for these firms imply bad private information. For the control-issues subsample, however, the poor performance of the firm before the event could reflect either the inability of outsiders to monitor the controlling manager or the uncertainty surrounding the control issue, both of which would be resolved after the sale. However, whatever the interpretation of this behavior, the CAR data does suggest that there are two different types of firms, each responding differently to the sales event. This could have been partially responsible for the lack of statistical significance in the earlier event-study tests. It also underscores the need for consideration of a sale's context when interpreting its effect on stock price. In the next section, I consider another related aspect of this context.

Age

The previous section demonstrated that the stock prices of firms undergoing announced control transitions respond differently to large sales events than do the stock prices of firms with no control issues. For those tests, control firms were identified using *Wall Street Journal* announcements. In this section, I consider a more subtle indica-

tor of transition potential: the age of the controlling insider. If the probability of retirement increases with age, and if managers prepare for retirement by reducing their stock holdings, we would expect to find older managers to be more likely to sell significant amounts of stock.

I tested the relationship between age and the probability of selling large amounts of stock by creating an indicator variable, VINCIBLE, which took the value of 1 if the insider sold enough stock to fall below one of the control benchmarks, and 0 otherwise. Using the primary, 81-firm sample, I first considered the mean ages for the managers at the 10% ownership benchmark. Selling managers (those for whom VINCIBLE=1) were the older group (with a mean age of 71.1, compared to 68.8 for the nonselling group). This is consistent with share reductions being a part of a transition process. A logit of VINCIBLE on age likewise showed a positive relationship, as did a logit of VINCIBLE on a dummy variable assuming a value of 1 for managers whose age was over 70 (OVER70). Finally, a regression of the percentage ownership in 1994 (OWNER94) on OVER70 showed a negative relationship: Older managers tend to have lower holdings in their firms.

While none of these relationships was statistically significant, all of them suggest that older managers are more likely to sell a significant portion of their holdings. This is consistent with the idea that managers have a pattern of selling behavior over their careers. This is part of the context in which insider sales must be considered.

Prior Performance and the Likelihood of Large Insider Sales

The event-study evidence presented in section 3 considered firm performance after a large sale by a controlling insider. In this section, I consider how the prior performance of the firm may be related to a manager's decision to sell a significant proportion of his firm's shares. This observable pre-sale behavior should affect outsiders' interpretation of a sales event. Consistent with Johnson, *et al.*'s evidence on involuntary divestitures, I expect that firms which are performing poorly will have managers who are less likely to allow their holdings to fall below a control benchmark. Managers in such firms may have more to fear from outside shareholder oversight, and may protect their access to corporate resources by maintaining voting control.

With the primary sample of 81 firms, I use Kaplan's (1989) methodology to examine accounting performance over the 1980-81 period. This methodology uses three accounting measures of performance, which I assume are positively related to firm performance: operating income (before depreciation), capital expenditures, and net operating income (operating income less capital expenditures). I use the growth rates in these three measures, as well as growth rates in variables created by dividing the three measures by total assets and by annual sales, as the dependent variables in a series of logit regressions. VINCIBLE, the dummy variable indicating that the manager has fallen below a benchmark, is the independent variable in these regressions. Many of these tests are then duplicated using an industry-adjusted dependent variable, which deducted from the original firm variable the industry's median percentage change for the measure.[17] 5%, 10%, and 25% are again used as benchmarks. The results of these tests are presented in Table 3.

As discussed earlier, if managers in poorly performing firms act defensively, we would expect a positive relationship between these measures and the probability that the managers' holdings would fall below a control benchmark. However, the results of the tests are mixed. For the capital expenditures variable, all of the fifteen coefficients from the tests are negative. Of the 27 total t-statistics which are greater than one, the only negative ones are the ten associated with the capital expenditures variable.

Table 3
Regressions of accounting measures on VINCIBLE

Regressions on independent variable VINCIBLE, which equals 1 if a manager's holdings fall below a given benchmark over the sample period. Dependent variables are based on COMPUSTAT data items d13 (operating income), d128 (capital expenditures), and [d13-d128] (net operating income). These data items are used to construct

three sets of dependent variables: a set using the values of the data items ("level"), a set using the data items divided by total assets ("percent of total assets," where total assets are measured by COMPUSTAT item d6), and a set using the data items divided by total sales ("percent of annual sales," where annual sales are measured by COMPUSTAT item d12). This methodology follows Kaplan (1989). Only primary sample firms with values of OWNER80 above the given benchmark are used for any test.

Numbers in boldface type are results for the tests duplicated using an industry-adjusted dependent variable. This variable was created by subtracting from the original firm variable the industry's median percentage change for the measure. Industry groupings were defined using 3-digit SIC codes.

	Observations		Vincible				L.R. Test
	Total	Vincible=1	Coefficient	T-Ratio	Constant	T-Ratio	(df=1)
OPERATING INCOME							
Benchmark=5%							
Operating income measured as:							
Level	70	14	-0.0087	-0.0413	1.3835	-4.5169	0.1729
			-0.0164	**-0.0764**	**-1.3831**	**-4.5891**	**0.0060**
Percent of total asset			-0.0203	-0.4025	1.3661	-4.5598	0.4180
			-0.0189	**-0.3971**	**-1.3661**	**-4.5590**	**0.4019**
Percent of annual sales			0.0142	0.0509	-1.3879	-4.6183	0.0025
			0.0065	**0.0244**	**-1.3870**	**-4.6189**	**0.0006**

Table 3 (Continued)

	Observations		Vincible				L.R. Test
	Total	Vincible=1	Coefficient	T-Ratio	Constant	T-Ratio	(df=1)
OPERATING INCOME							
Benchmark=10%							
Level	50	15	-0.0607	-0.2811	-0.8262	-2.6132	0.0893
			-0.2232	**-0.5611**	**-0.8075**	**-2.5882**	**0.5911**
Percent of total assets			-0.0991	-0.3742	-0.8326	-2.6840	0.1710
			-0.1884	**-0.5478**	**-0.8194**	**-2.6369**	**0.4374**
Percent of annual sales			-0.0609	-0.2173	-0.8381	-2.6954	0.0500
			-0.2224	**-0.5362**	**-0.8127**	**-2.6018**	**0.4108**
Benchmark=25%							
Level	22	8	1.2282	1.1759	-0.8118	-1.6192	1.7224
			1.3373	**1.2934**	**-0.8011**	**-1.6158**	**2.0797**
Percent of total assets			1.3164	1.1094	-0.6163	-1.3379	1.4867
			0.7133	**0.7513**	**-0.6409**	**-1.3815**	**0.5870**
Percent of annual sales			2.4677	1.2462	-0.6417	-1.3632	2.5651
			1.3531	**1.0514**	**-0.7651**	**-1.5462**	**1.2485**
CAPITAL EXPENDITURES							
Benchmark=5%							
Capital expenditures measured as:							
Level	67	14	-0.4824	-0.9054	-1.1848	-3.5988	0.9579
			-0.5486	**-1.1393**	**-1.3064**	**-4.2993**	**1.4965**
Percent of total assets			-0.5749	-0.8705	-1.2467	4.0718	1.3557
Percent of annual sales			-0.6381	-0.9743	-1.2499	-4.0842	1.2386
			-0.6157	**-1.0845**	**-1.3040**	**-4.2909**	**1.4682**
Benchmark=10%							
Level	49	15	-0.9075	1.4795	-0.5212	-1.4801	2.8987
			-0.4577	**-1.0515**	**-0.7634**	**-2.4257**	**1.2660**

Percent of total assets			-1.6652	-1.8954	-0.6402	-1.9600	5.1744
Percent of annual sales			-1.1208	1.5148	-0.6351	-1.9496	3.2958
			-0.4601	**-0.9254**	**-0.7578**	**-2.4055**	**1.0547**
CAPITAL EXPENDITURES							
Benchmark=25%							
Level	21	7	0.8750	-1.0772	-0.3191	-0.5793	1.5333
			-1.3053	**-1.3954**	**-0.5160**	**-1.0359**	**3.1013**
Percent of total assets			-1.3571	-1.2738	-0.4157	-0.8288	2.3835
Percent of annual sales			-0.7011	-0.8674	-0.4793	-0.9518	1.1108
			-1.6558	**-1.4837**	**-0.4900**	**-0.9693**	**3.6817**
NET OPERATING CASH FLOW							
Benchmark=5%							
Net operating cash flow measured as:							
Level	67	14	0.0721	0.6500	-1.3404	-4.4292	0.4011
			0.0435	**0.3570**	**-1.3357**	**-4.4306**	**0.1224**
Percent of total assets			-0.0055	-0.4744	-1.3146	-4.3660	0.3501
			-0.6489	**0.3945**	**-1.3134**	**-4.3611**	**0.3816**
Percent of annual sales			0.1303	0.8557	-1.3215	-4.3714	0.7051
			0.8331	**0.5523**	**-1.3352**	**4.4248**	**0.2916**
NET OPERATING CASH FLOW							
Benchmark=10%							
Level	49	15	0.1647	1.1516	-0.8351	-2.6456	1.5401
			0.1458	**1.0779**	**-0.8455**	**-2.6780**	**1.3038**
Percent of total assets			0.1931	1.2125	-0.8169	-2.5865	1.6915
			0.1756	**1.1576**	**-0.8532**	**-2.6928**	**1.5044**
Percent of annual sales			0.2112	1.2357	-0.8115	-2.5667	1.7726
			0.1832	**1.1377**	**-0.8478**	**-2.6800**	**1.4413**
Benchmark=25%							
Level	21	7	0.8726	1.0759	-0.6961	-1.3641	4.0649
			1.2311	**1.4522**	**-0.7524**	**-1.4166**	**5.6025**
Percent of total assets			1.0081	1.0716	-0.5602	-1.0439	4.0006
			1.1346	**1.3483**	**-0.7321**	**-1.4073**	**4.8767**
Percent of annual sales			1.4145	1.1744	-0.4835	-0.8830	4.5775
			1.3616	**1.4203**	**-0.7464**	**-1.4141**	**5.3491**

Thus, in this sample, firms which have higher capital expenditures at the beginning of the sample period are less likely to have managers reduce their holdings significantly. [18] Results for the other two accounting variables differ from these capital expenditures results. The test statistics from the operating income tests are mixed, and the signs of the coefficients depend upon the benchmark used. None of these results is significant. However, results for the operating cash flow variable are predominantly positive. Of the 18 measures for this variable, 16 (88.9%) are positive. All of the t-ratios for the 10% and 25% benchmarks are greater than one (these represent 44.4% of the large t-statistics from the tests). Again, while these results are not statistically significant, they do suggest that controlling managers of firms with higher operating cash flow are more likely to reduce their share holdings significantly. This is consistent with the idea that good performance alone helps insulate managers from shareholder discipline; large stock holdings are then redundant and costly. The results for this variable are especially interesting given Barclay and Holderness's (1989) evidence suggesting that blocks are more valuable in firms with higher cash flow. Managers in our sample appear to be voluntarily allowing outside oversight in just those situations where access to corporate resources is most valuable.

Conclusions

The results just presented suggest that there is a relationship in our sample between performance and the controlling manager's willingness to sell a large proportion of his shares. Managers in firms performing well may have less of a need to insulate themselves from shareholder oversight. Having a manager reduce his holdings enough to voluntarily accept such oversight could therefore be considered good news. The interpretation of some insider sales as good news is consistent with the event-study results presented in section 3, which show that firm value is just as likely to rise on the news of a large insider sale as it is to fall. Large sales need not imply negative private information.

It must be emphasized that these tests are not powerful. The sample size for the event study, in particular, is small, since it is apparently rare for insiders to retain their positions after large sales (for consistent evidence, see Barclay and Holderness [1991]). However, the weight of the qualitative results of these tests allow us to draw the conclusion that it is inappropriate to interpret all insider sales as bad news: Insider sales occur in a variety of contexts, and they must be considered in those contexts. Some sales are simply required steps in a planned succession process, for example, and are not motivated by negative private information. Simple buy/sell rules based on insider behavior therefore are simplistic and flawed. Unfortunately for investors, however, these are the types of rules that are often espoused in the popular press.

As discussed in the introduction, many analysts and investors place considerable emphasis on insider trading behavior. A recent *Wall Street Journal* article calls data on insider trades "some of Wall Street's most juicy data" because it is "potentially market moving." The article continues: "The information, which can yield clues to the moves of a company, industry, or even the broader market, is so sought after by many individual and professional investors that it has spawned a whole cottage industry to track it."[19] Because of this interest in timely reporting of insider trading data, the SEC is currently considering a proposal to require insiders to report their trades electronically. In fact, some observers believe that faster and more predictable dissemination of insider transaction data will facilitate market adjustment to the increased flow of corporate information resulting from the SEC's new Regulation FD.

However, the message of this paper is that not all inside transactions are news *or* newsworthy. The greatly increasing accessibility of insider trading data, coupled with the plausible conventional wisdom that it *should* be important, may encourage investors to mine transactions data indiscriminately. Increased scrutiny of insider behavior is valuable to the outsider only if he fully understands the underlying context. If this context is not provided by summary trading statistics, then these statistics, regardless of the timeliness of their reporting, are an insufficient basis for reasonable trading strategies. 🕮

Suggestions for Future Research

While the results of this study suggest strongly that not all insider sales are bad news, the small sample size means that the statistical power is low. There simply were not many of the necessary events during the period studied here. It is possible that this type of control event was more common during the latter part of the 1990s, as founders of high-tech firms reduce their stakes. However, even if the frequency has not changed, extending the sample period of this study would increase the sample size, improving the power of the tests.

Perhaps a more interesting extension would be to consider more carefully the unique factors that define a firm's control benchmark. Previous research, including this paper, has been hampered by a lack of theoretical support for specific benchmarks, and have resorted to fairly arbitrary estimates. Proper identification of control thresholds is obviously essential to the recognition of control events. Such identification would be a real contribution, not only for the interpretation of insider sales, but also for the evaluation of any corporate control event (for example, as the countries of the eurozone attempt to reconcile their disparate thresholds regulating takeovers).

Endnotes

1. For example, the Schwab Analytics Fund has managers "review insiders' reports of purchases and sales, and jettison any issues where there's been heavy selling," since insider selling is seen as "negative—a warning sign steering us away from a stock." (Praveen Gottipalli, quoted in Vanessa O'Connell, "Some Stock Funds Beat Rivals by Following Insiders' Trades," *Wall Street Journal*, 1/29/97.) (See also Rozeff and Zaman [1988], footnote 5, for evidence on advisory services' using insider trading information.)
2. See, for example, Seyhun (1986), (1988a), and (1988b).
3. This follows the sample selection procedure in Denis and Denis (1993).
4. 5% ownership is the minimum concentration for the SEC's insider reporting requirements. This is also the point around which Morck, Shleifer, and Vishny (1988) find evidence of managerial entrenchment. Lowering this benchmark slightly to 4.7% allowed me to include 2 more firms in the primary sample.
5. The specific insiders held the titles of CEO (34), chairman of the board (43), president (21), executive committee chairman (6), vice president (3), and director (5). Several insiders held more than one title.
6. I did not search the *Official Summary* to determine if there were significant sales events over the sample period for the 435 firms eliminated from the 1980 *Value Line* set. If there were such events, and if the eliminated firms differ systematically from the firms retained in the sample, then the empirical results will be biased. I have no *a priori* reason for expecting such a systematic difference. Further consideration of possible selection biases is contained in Livingston (1996).
7. This compendium, published by the SEC, lists every insider stock transaction. It provides the name of the insider, his relationship to the firm, the date and size of the transaction, the trading price, and the insider's final holdings. Consideration of the information content of the *Official Summary* can be found in Jaffe (1974) and Rozeff and Zaman (1988), among others.
8. Sample firms were assigned to deciles based on their total market capitalization. All firms on the CRSP tape were used to determine the decile cutoffs.
9. For all ten firms in the event-study sample, the residuals from the decile adjustment were checked for autocorrelation using Shazam's DIAGNOS function. This provides, among other things, the Durbin-Watson statistic for up to 23 lags. For our tests, the null hypothesis of no serial correlation would be rejected for values of d less than 1.654 or greater than 2.346. Only Cummins Engine showed evidence of first-order serial correlation ($\rho = .2440$; $d = 1.4699$); values of ρ for Cummins for the next three lags were negative and insignificant.
10. The largest of these is 2.7588, from Knight-Ridder under the two-day decile model for the single largest selling day.
11. Individual standardized prediction errors are defined by $\sum_{t=t1}^{t2} PE_{jt} / \left(\sum_{t=t1}^{t2} \text{variance}(PEjt) \right)^{(1/2)}$; averages are $\frac{1}{\sqrt{N}} \sum_{j=1}^{N} \sum_{t=t1}^{t2} PE_{jt} / () \sum_{t=t1}^{t2} \text{variance}(PEjt)^{(1/2)}$
12. However, this difference is only marginally significant: the average of the positives is .9045, the average of the negatives is -.6858; p=. 118 in a differences in means test.
13. One procedural consideration is the measurement of the event period. To ensure that my event period was not too short to capture the price response, I also ran tests run using a 31-day event window. These tests produced the same qualitative results as those presented in the paper. However, the overriding methodological concern is the small sample size. I will discuss this later in the paper.
14. This search also verified that there were no confounding events, other than the control considerations discussed, for any of the sample firms.
15. W.W. Grainger elected a new president and created the new post of vice chairman; Knight-Ridder announced a new chairman and the date of his planned retirement; Outboard Marine named a new president/COO; and Loctite announced Kenneth Butterworth's election to CEO and his eventual replacement of Robert Kreible (the sample's tracked executive) as chairman. A complete list of the relevant news items for all sample firms is provided in the appendix.

16. These observations can be summarized by the following results from comparisons of the prediction errors (PEs): the mean PE for the control subsample is significantly higher after the event; the mean PE for the no-issue subsample is significantly lower after the event; the PEs between the subsamples are not significantly different before the event, but they are significantly different after the event (with the no-issues PEs smaller). (All tests used a significance level of 5%.)
17. Missing data precluded the running of the industry comparison tests for the percent of total assets and the percent of annual sales measures for the capital expenditures variable.
18. Kaplan, following Jensen's free cash flow hypothesis, predicts that his sample of buyout firms will have higher values associated with lower capital expenditures, since after the buyout these companies will be forced to reduce investments in negative net present value projects. It is possible that the discipline imposed by becoming vulnerable to shareholders is similar to the discipline imposed by large debt service requirements, and that top managers previously having voting control may have used their control rights to invest in negative NPV projects. However, our managers internalize less of the effects of their investment after the sale, while MBO managers internalize more. Therefore, a positive relationship between capital expenditures and VINCIBLE is more consistent with the current model than is a negative relationship.
19. "SEC weighs electronic filing of insider trades to help push the system into the internet age," *Wall Street Journal*, November 8, 2000.

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