Working Paper 8611

ERRORS IN RECORDED SECURITY PRICES AND THE TURN-OF-THE-YEAR EFFECT

By James B. Thomson

James B. Thomson is an economist at the Federal Reserve Bank of Cleveland. This paper is based on the author's Ph.D. dissertation at The Ohio State University. The author thanks his dissertation committee, Ed Kane (Chairman), Rene Stulz, and Randy Olsen, as well as Andy Chen, Ray Degennaro, Pat Hess, Bill Gavin, Jim Moser, Bill Osterberg, and Eric Rosengren for helpful comments and suggestions.

December 1986

ABSTRACT

Errors in recorded security prices are a source of misspecification in the market model. If recorded-price errors are sufficiently nonrandom, they result in biased returns and biased and inconsistent estimates of market model regression coefficients. This paper argues that tax-induced flow-supply pressures result in end-of-the-year recorded-price errors that are nonrandom enough to cause the appearance of anomalous turn-of-the-year stock return behavior. Empirical tests of returns and market model regression coefficients during the turn-of-the-year period cannot reject this errors-in-variables explanation of the turn-of-the-year effect.

ERRORS IN RECORDED SECURITY PRICES AND THE TURN-OF-THE-YEAR EFFECT

The turn-of-the-year (TOY) effect (or January effect) refers to the anomalous behavior of stock returns during the last five trading days in December and the first five trading days in January. This anomaly is of particular interest to financial researchers because it appears to be a small-firm effect and the source of the majority of size-related anomalies (see [16], [21], [23], [24]). The interest in the TOY effect is justified because of its implications concerning the validity of the Capital Asset Pricing Model (CAPM) and market efficiency.

In this paper we show that the TOY effect is a low-priced security effect where size proxies for share price. It is an errors-in-variables problem due to the use of the one-eighth pricing convention in recording security prices. This explanation of the TOY effect is consistent with both the CAPM and market efficiency.

Section I of the paper discusses possible sources of errors in recorded security prices. Section II looks at recorded-price errors as a source of bias in stock returns and as a source of specification error in the market model. Section III outlines the hypothesis that the TOY effect is a low-priced security effect. Sections IV and V present the data and the test of the low-priced security hypothesis. The paper's conclusions are presented in Section VI.

I. Sources of Price-Related Errors in Recorded Security Prices

The use of the one-eighth pricing convention in recording stock prices results in measurement errors in observed stock prices. The relative size of the measurement error is inversely related to the level of the stock price. Therefore, any bias in stock returns resulting from the use of one-eighth pricing conventions in recording stock prices is inversely related to stock price levels.

Even though stock prices are recorded at intervals of one-eighth of a dollar, movements in actual trading prices are not restricted to one-eighth intervals,[1] Investors can circumvent the one-eighth price movement restriction by splitting their order between pricing points one-eighth of a dollar apart. For example, if an investor negotiates a price of $2.1875 with the market specialist in the stock, the specialist books half of the order at $2.25 and the other half at $2.125. By shifting the order between pricing points, the investor can buy and sell the stock as if the price movements were continuous. However, when split orders are booked by the specialist, they are recorded as separate transactions at each one-eighth pricing point. If the price of the stock is on a downward (upward) trend, the last recorded price is the lower (higher) of the two prices from the split order.

Another source of errors in recorded stock prices is Blume and Stambaugh's [3] bid-asked bias. These authors argue that bias in recorded returns can result from differences in the size of the bid-asked spreads on the stocks of small and large firms. Blume and Stambaugh introduce bid-asked bias as an explanation of the small-firm effect found by Reinganum [21]. These authors argue that the difference between the bid-asked spreads of small firms and large firms may cause the returns of the small firms to be overstated. Their analysis hinges on the role of the market specialist as the buyer (seller) of last resort in the stock market. If stock is purchased (sold) by an investor, one of two transactions may have occurred. If the specialist has lined up a seller (buyer) for the security at the quoted sales price, the transaction price is the market-clearing price. On the other hand, if the specialist sold (bought) the stock to (from) the investor from (for) his inventory at his asking (bid) price, the transaction price at which the investor buys (sells) the stock is not a market-clearing price. The size of the bid-asked bias is directly related to the width of the bid-asked spread.

Blume and Stambaugh show that the use of the one-eighth pricing convention in security markets increases the degree of bid-asked bias for low-priced stocks relative to high-priced stocks. For example, the one-eighth pricing convention sets the minimum bid-asked spread at one-eighth of one dollar.[2] The minimum percentage spread for a stock priced at $2 per share is 6.252, while the minimum spread for a stock priced at $20 per share is 0.625%. It is clear that in the absence of trading volume and other considerations, the relative width of the bid-asked spread decreases as share price increases. In fact, the negative relationship between price and the relative width of the bid-asked spread is empirically documented by Branch and Freed [5] and Demsetz [9]. Therefore, the degree of bid-asked bias in recorded prices is inversely related to price.

II. Effects of Recorded-Price Errors on Measures of Risk and Return

Measurement errors in recorded stock prices can lead to biases in holding-period returns when the returns are calculated over short holding periods characterized by flow-supply (flow-demand) pressures. Flow-supply (flow-demand) pressures can lead to nonrandom recorded-price errors. If the recorded-price errors are sufficiently nonrandom, then returns computed from recorded stock prices will be biased. A reduction in the length of the holding period over which the returns are computed increases the probability that the prices used to compute returns are subject to measurement error and thereby increases the likelihood that the holding-period returns are biased.

Recorded-price bias in holding-period returns occurs when one or both of the prices used in calculating holding-period returns are measured with error. For example, let P_{it} be the true equilibrium price of firm i's stock at time t, let p_{it} be the recorded price of firm i's stock at time t, and let δ_{it} be the measurement error in p_{it} (that is, $p_{it} = P_{it} + \delta_{it}$). The observed holding-period return for firm i at time t, r_{it}, equals the true holding-period return R_{it} plus the measurement error λ_{it}.

$$(1) \qquad \lambda_{it} = r_{it} - R_{it} = (P_{it} + \delta_{it})/(P_{it-1} + \delta_{it-1}) - P_{it}/P_{it-1}.$$

Observed portfolio returns should be less sensitive to recorded-price errors because the magnitude and sign of λ_{it} varies across the firms in the portfolio. As seen in equation (2), the measurement error in portfolio returns, Λ_{pt}, is the weighted sum of the measurement errors of the securities in the portfolio.

$$(2) \qquad r_{pt} = \sum_{i=1}^{k} w_i r_{it} = \sum_{i=1}^{k} w_i (R_{it} + \lambda_{it}) = R_{pt} + \sum_{i=1}^{k} w_i \lambda_{it} = R_{pt} + \Lambda_{pt}.$$

One hopes that by grouping firms into portfolios, the pricing errors will cancel out. However, during periods of flow-supply and flow-demand pressures, the pricing errors may become nonrandom in the time series of the individual firms and in the cross section of the firms in the portfolio. In this case, grouping will remove relatively little of the recorded-price error from observed portfolio returns.

Recorded-price errors in individual firm stock returns and portfolio returns cause the market model to be misspecified. As seen in equation (3), the error term in the market model, e_{pt}, now consists of the standard error term, ε_{pt} (which measures unexpected returns), the measurement error in the portfolio return, Λ_{pt}, and the measurement error in the return of the market portfolio, Λ_{mt}, scaled by the regression slope coefficient, β_p.[3]

$$r_{pt} - R_{ft} = a_p + \beta_p(r_{mt} - R_{ft}) + \Lambda_{pt} - \beta_p\Lambda_{mt} + \varepsilon_{pt}. \tag{3}$$

Under the classical conditions, $E(\Lambda_{mt}) = E(\Lambda_{pt}) = 0$ and $Cov(R_{pt},\Lambda_{mt}) = Cov(R_{pt},\Lambda_{pt}) = Cov(R_{mt},\Lambda_{mt}) = Cov(R_{mt},\Lambda_{pt}) = Cov(\Lambda_{pt},\Lambda_{mt}) = 0$, nonzero Λ_{pt} causes the estimate of a_p to be a high-biased estimate of the true a, but it does not affect the estimate of β. Unfortunately, because $\beta\Lambda_{mt}$ is correlated with r_{mt}, the measurement error in the market portfolio causes the estimate of β_p to be low-biased. However, if one or more of the classical conditions fail to hold, the direction of the bias in the estimates of a and β is generally ambiguous (see Maddala [20], chapter 13).

During periods not characterized by flow-supply or flow-demand pressures, the classical conditions should hold. Indeed, we argue that in the absence of flow-supply and flow-demand pressures, recorded-price errors are random enough across securities that Λ_{mt} is insignificant. Therefore, the remaining source of bias in the estimated coefficients of equation (3) is Λ_{pt} (λ_{it} for individual stock returns), which only affects estimates of a.

During periods of flow-supply or flow-demand pressures, both Λ_{mt} and Λ_{pt} will be sources of bias in regressions on the market model. In addition, the flow-supply or flow-demand pressures will cause Λ_{mt} and Λ_{pt} to be positively correlated and the estimate of β to be a high-biased estimate of the true β.[4] β estimates are high-biased because the positive correlation between Λ_{mt} and Λ_{pt} causes the observed returns r_{pt} and r_{mt} to be more highly correlated than the true returns R_{pt} and R_{mt}.[5]

III. The Low-Priced Security Hypothesis

The low-priced security hypothesis (LPSH) is a general version of the tax-selling hypothesis (see [4], [7], [8], [10], [15], [21], [23], [25], [27], [28]) and the price-pressure hypothesis (Harris and Gurel [13]). The LPSH argues that flow-supply pressures at the end of the calendar year cause the recorded-price errors in security returns to be nonrandom during the turn-of-the-year period. The LPSH is a tax-selling hypothesis because it views tax-selling by investors to optimally exercise tax-timing options at the end of the tax year as the source of the flow-supply pressures at the end of the calendar year.[6] The LPSH is a price-pressure hypothesis because it views returns earned by liquidity traders (such as market specialists) who accommodate flow pressures to be consistent with market efficiency. That is, liquidity traders are paid for the risk-bearing services associated with accommodating flow pressures.

The LPSH argues that the TOY effect is a low-priced security effect and not a size effect. LPSH predicts that the largest TOY effects will be associated with low-priced stocks because the relative magnitude of the recorded-price error is inversely related to price. The LPSH predicts that recorded-price errors will cause both observed returns in January and the estimated ß to be high-biased. The LPSH contends that the size-related TOY effect documented by Reinganum [22] and others (see [2], [6], [15], [23]) is really a low-priced security effect with size proxying for price during the TOY period. The positive relationship (found by Basu [1] and Kross [17]) between price variables and size is consistent with size proxying for price during the TOY period. Roll's [23] finding that the largest TOY return is associated with stocks priced under $2 per share is further evidence consistent with the LPSH. In addition, Thomson [29] shows that low-priced security portfolios exhibit more factor-related seasonality during the TOY period than do the small-firm portfolios.

IV. The Data

The data used in the tests of the LPSH are from the 1982 versions of the Center for Research in Security Prices (CRSP) daily returns file, daily index file, monthly master file, and AMEX master file. The sample consists of daily stock returns of all firms listed on the CRSP daily returns file from July 1962 through December 1982. The firms are grouped into portfolios on the first trading day of July on the basis of market capitalization and on the basis of stock price on the last trading day in June (in every year but 1962). To disentangle the effects of grouping from the TOY effect, we utilize a July-to-June year, rather than a January-to-December year. All firms in the sample in a given year were listed on the CRSP daily return file and had price and share information on the CRSP monthly master file or AMEX master file on the last trading day in June (in every year but 1962). The portfolios are updated each July to capture new listings. Firms delisted during the sample period are treated as liquidations. We assume that stockholders receive the full market value of their shares and invest the proceeds in the risk-free asset (the weekly Treasury bill rate is used to proxy for the return on the risk-free asset).' The delisted firm is dropped from the sample when the portfolios are updated at the beginning of the next sample (July to June) year.

The portfolios are equally weighted at the beginning of each sample year and are not rebalanced until the portfolios are updated at the beginning of the next sample year. The portfolios are set up as mutual funds, in which the portfolio weights are adjusted to reflect the firms' performance in the portfolio relative to that of the portfolio. That is, the portfolio weight of firm i at time t, w_{it}, in the portfolio is 1/n for t=1 and

(4) $$w_{it} = w_{it-1}(1 + r_{it-1} - r_{pt-1}), \text{ for } t = 2, \ldots, n.$$

This portfolio weighting scheme assumes that if a firm pays a dividend, the full amount of the dividend is reinvested without cost into the stock of the firm. However, this portfolio weight adjustment is more realistic than one that rebalances the portfolio daily to an equally weighted portfolio. In addition, this approach avoids factor-related biases that can arise from rebalancing portfolios to equally weighted portfolios on a daily basis (see Roll [24] and Blume and Stambaugh [3]).

V. An Investigation of the Low-Priced Security Hypothesis

To test whether the TOY effect is a size-related effect or a low-priced security effect, the sample is grouped into 10 MV portfolios on the basis of the market value of the firm and into 10 PR portfolios on the basis of share price. The portfolios are numbered on the basis of market value (price); MV1 (PR1) is made up of the firms in the lowest market-value (price) decile and MV10 (PR10) is constructed from the firms in the highest market-value (price) decile. In addition, 15 portfolios are constructed on the basis of size and price. The data is sorted twice, first into size quintiles and then into price quintiles. Five SIZE (PRICE) portfolios are formed from firms that are in each size (price) quintile but not in the corresponding price (size) quintile. For example, SIZE1 (PRICE1) comprises firms in the lowest market-value (price) quintile that are not in the lowest price (market-value) quintile. Five MVPR portfolios are formed from the firms that are excluded from the SIZE and PRICE portfolios. For example, SIZE1 (PRICE1) and MVPR1 contain the firms in the lowest market-value (price) quintile.[8] The SIZE (PRICE) portfolios are constructed to disentangle price (size) effects from size (price) effects in the MV (PR) portfolios.

To investigate the presence of factor-related TOY premiums in the returns of the portfolios, mean returns are computed for the MV, PR, SIZE, PRICE, and MVPR portfolios over five subsample periods:

1) the sample period = all but the last five observations in the sample;
2) the pre-yearend period = last five trading days of each calendar year;
3) the post-yearend period = first five trading days of each calendar year;
4) the TOY period = pre-yearend period + post-yearend period;
5) adjusted-year period = sample period - TOY period.

The last five observations are dropped when computing mean returns for each subsample because they correspond to the pre-yearend period for 1982 and there is no corresponding post-yearend period for 1983 in the sample. This particular partitioning of the sample is done for three reasons. First, the empirical evidence of Reinganum [22] and Keim [15] indicates that the bulk of the TOY premium lies in the first five trading days of January. Second, Roll [23] uses the 10 trading days centered on the end of the calendar year as the TOY period. Finally, prior to the Tax Reform Act cf 1986, an installment-sale option for capital gains was available to investors during the last five trading days of December.[9]

Table 1 indicates that there is a significant size- or price-related effect in the returns of the MV and PR portfolios during the TOY and post-yearend periods. We are unable to reject the hypothesis that the mean returns are equal across size (price) deciles for the sample period, the adjusted-year period, and the pre-yearend period for both the MV and PR portfolios. Table 2 shows that once price is accounted for, the significant size effect found during the TOY and the post-yearend periods disappears.

Once size is accounted for, a significant price effect still exists during the TOY and post-yearend periods. The SIZE portfolios do not exhibit significant size-related effects in any of the subsamples, while the PRICE and MVPR portfolios exhibit a significant price-related effect during the TOY and post-yearend periods. This result is consistent with the LPSH, which argues that size proxies for price during the TOY period.

Tables 1 and 2 show that mean daily returns are higher for all portfolios during the pre-yearend period, post-yearend period and, therefore, the TOY period. Although the pre-yearend mean daily returns do not exhibit any factor-related bias, they are roughly 10 times larger than the sample period returns for all of the portfolios. This would indicate that the adjustment in stock prices from their tax-depressed lows occurs before the end of the calendar year. In fact, Roll [23] finds the anomalously high returns at the turn of the year begin the last trading day of December. Note that the anomalously high returns for all the portfolios (except MV10, PR10, and MVPRS during the post-yearend period) during the 10 trading days centered on the end of the calendar year can be explained by recorded-price errors. An investigation of the absolute price movements during the TOY period supports this conclusion. Thomson [29] shows that the change in prices during the 10 days surrounding the end of the year is within the bounds predicted by the LPSH. This is further evidence that the TOY effect is a price-related effect and not a size-related effect.

An alternative explanation for the anomalous TOY returns is that systematic risk increases during the TOY period. If systematic risk increases, then returns should increase to compensate market participants for the additional risk-bearing services provided. In other words, the abnormally high TOY returns are not anomalous if risk-adjusted returns are no higher during the TOY period than during the rest of the year. If systematic risk increases during the TOY period, the market model slope coefficient, ß, would exhibit an upward shift during the TOY period.[10]

On the other hand, the empirical observation that the market model slope coefficient exhibits TOY-related seasonality may not be the result of an increase in systematic risk. One consequence of nonrandom recorded-price errors is that the estimated regression coefficients from the market model will be biased and inconsistent. In fact, we argue earlier in this paper that nonrandom recorded-price errors may result in high-biased estimates of ß. Therefore, TOY-related shifts in the estimates of α and/or ß support the LPSH. If TOY-related seasonality is present in the regression coefficients of the market model, then there are two hypotheses to test. First, we must test the LPSH versus the hypothesis that the TOY is a size-related effect. Second, we should test the LPSH against the hypothesis that the anomalous TOY returns are the result of an increase in systematic risk during the TOY period.

To test the LPSH against the two alternative hypotheses, the following modified market model regression is estimated for the MV, PR, SIZE, PRICE, and MVPR portfolios using version 3.0.2 of SHAZAM [32]:

$$(5) \quad r_{it} - R_{ft} = \alpha_{1i}D_1 + \alpha_{2i}D_2 + \alpha_{3i} + \beta_{1i}S_1 + \beta_{2i}S_2 + \beta_{3i}(r_{mt} - R_{ft}) + e_{it}.$$

Equation (5) is equation (3) modified to include intercept- and slope-dummy variables for the pre- and post-yearend periods to test for changes in the observed risk-return relationship during the TOY period. D_1 (D_2) is the intercept-dummy variable for the pre-yearend (post-yearend) period, and S_1 (S_2) is the slope-dummy variable for the pre-yearend (post-yearend) period. D_1 (D_2) equals one during the pre-yearend (post-yearend) period and is zero otherwise. S_1 (S_2) equals D_1 (D_2) times the return on the market portfolio, r_{mt}.

As seen in tables 3 through 5, there appears to be significant seasonality in the estimate of ß during the beginning of the calendar year. The estimate of the slope-dummy variable for the post-yearend period, $\hat{\beta}_2$, is positive and significant for all portfolios except for MV10 and PRICE5 where $\hat{\beta}_2$ is positive and insignificant, MVPR5 where $\hat{\beta}_2$ is negative and insignificant, and PR10 where $\hat{\beta}_2$ is negative and significant. However, we find very little evidence of pre-yearend slope seasonality. $\hat{\beta}_1$ is significantly different from zero only for MV10, PR9, PR10, PRICES, MVPR5, and PRICE1. $\hat{\beta}_1$ is negative and significant for the first four and positive and significant for PRICE1. F-Tests for the equality of β_1 and β_2 fail to reject the restriction only for PR10, PRICE1, and MVPRS. Because mean daily returns are higher during both the pre- and post-yearend periods, rejecting the hypothesis that $\beta_2 = 0$ while failing to reject the hypothesis $\beta_1 = 0$ is inconsistent with the hypothesis that increased systematic risk during the TOY is the source of anomalous TOY returns.

The insignificance of β_1 in the majority of the regressions is not inconsistent with the LPSH's error-in-variables explanation for observed increases in ß during the TOY period. The insignificance of B, may indicate that the majority of recorded-price decreases, on stocks that are tax-loss selling candidates, have already occurred by the pre-yearend period. This would reduce the degree of recorded-price bias in the portfolio returns and the market proxy return, and therefore the bias in ß. In fact, Roll [23] provides evidence that the recorded prices of tax-loss selling candidates start to readjust toward their true price on the last trading day of the year.

For the LPSH to be accepted, β_2 should show a price-related bias. That is, we should observe more slope seasonality for low-priced portfolios than for high-priced portfolios. In addition, we should not observe size-related slope seasonality in β_2 in the SIZE portfolios where we control for price-related effects. To test for size- and price-related slope seasonality, we test cross-equation equality restrictions on the βs in the regression of equation (5) for the MV, PR, SIZE, PRICE, and MVPR portfolios. The test results appear in table 6.

As seen in tables 3, 5, and 6, there are significant size-related effects in the estimates of β_2 for the MV portfolios, although β_2 does not exhibit a significant size-related effect for the SIZE portfolios. The rejection of the cross-equation equality restriction on β_2 for the MV portfolios, combined with the inability to reject the cross-equation equality restriction on β_2 for the SIZE portfolios, is evidence that the size-related effect in β_2 for the MV portfolios is actually a price-related effect. In contrast, looking at tables 4 through 6, we see a significant price-related effect in the estimates of β_2 for the PR and PRICE portfolios. The failure to reject the cross-equation equality restriction on β_2 for the SIZE portfolios while rejecting it for the MV, PR, PRICE, and MVPR portfolios is evidence that the factor-related slope seasonality is an effect related to price but not size." This is consistent with the LPSH.

One could argue that we are overstating the significance of the tests of the cross-equation equality restrictions for β_2 because β_2 is the shift in β during the post-yearend period and we reject the cross-equation equality restriction for β_3 (which is our estimate of the market model β exclusive of the TOY shifts). This may indicate that the relative and not the absolute shifts in β_2 are important in determining whether size or price is driving the seasonality in β. However, closer inspection of the results in tables 3 and 5 indicates that this is not a problem. The coefficient for β_3 for the lowest (highest) market-value quintile of the MV portfolios is close to that of β_3 for SIZE1 (SIZE5). On the other hand, the coefficient for β_2 for the lowest (highest) market-value quintile of the MV portfolios is twice (between one-fifth and one-tenth) the magnitude of β_2 for the SIZE portfolios. A look at the other market-value quintiles shows that β_3 is roughly the same for MV and SIZE portfolios in each specific market-value quintile, while β_2 tends to be higher (lower) for the SIZE portfolios than the MV portfolios in the low (high) market-value quintiles. Therefore, the failure of the cross-equation equality restriction for β_3 cannot account for the disappearance of the size-related shift in β during the post-yearend period once price is accounted for.[12]

VI. Conclusion

Recorded-price errors are potential sources of misspecification in joint tests of the CAPM and market efficiency. We show that if the recorded-price errors are sufficiently nonrandom, they can lead to biases in returns and in the estimated coefficients of the market model. From this standpoint this paper is an extension of the work of Blume and Starnbaugh.

The second contribution of this paper is that it provides an explanation of the TOY effect that is consistent with both the CAPM and market efficiency. We find that the TOY effect is a price-related effect and that size appears to be proxying for price during the TOY period. We propose and test the LPSH, which argues that the TOY effect is due to nonrandomness in recorded-price errors induced by tax-related flow-supply pressures at the end of the calendar year. Tests of both raw returns and regression coefficients from the market model fail to reject recorded-price errors as the source of the TOY effect. This errors-in-variables explanation for the anomalous behavior of stock returns during the TOY period is consistent with both the CAPM and market efficiency.

Failure to reject the LPSH as an explanation of the TOY effect has implications for future research into stock market behavior. More research needs to be done on the nature and severity of recorded-price errors as a source of specification error in tests of risk-return generating models such as the CAPM. Recorded-price errors may be the source of abnormal returns surrounding events, such as stock splits and dividend payments, which may be accompanied by flow-supply and/or flow-demand pressures.

REFERENCES

Basu, Sanjoy. "The Relationship Between Earnings' Yield, Market Value, and Return for NYSE Common Stocks," Journal of Financial Economics, vol. 12 (19831, 129-56.

2. Berges, Angel, John J. McConnell, and Gary G. Schlarbaurn. "The Turn-of-the-Year Effect in Canada," Journal of Finance, vol. 39, no. 1 (March 1984), 185-92.

3. Blume, Marshall E., and Robert F. Stambaugh. "Biases in Computed Returns: An Application to the Size Effect," Journal of Financial Economics, vol. 12 (1983), 387-404.

4. Branch, Ben. "A Tax Loss Trading Rule," The Journal of Business, vol. 50, no. 2 (April 1977), 198-207.

5. ________, and Walter Freed. "Bid-Asked Spreads on the AMEX and the Big Board," Journal of Finance, vol. 32, no. 1 (March 1977), 159-63.

6. Brown, Philip, Donald B. Keim, Allan W. Kleidon, and Terry A. Marsh. "Stock Return Seasonalities and the Tax-Loss Selling Hypothesis: Analysis of the Arguments and Australian Evidence," Journal of Financial Economics, vol. 12 (1983), 105-27.

Chan, K.C. "Can Tax-Loss Selling Explain the January Seasonal in Stock Returns?" Journal of Finance, vol. 41 (December 1986), 1115-1128.

8. Constantinides, George M. "Optimal Stock Trading with Personal Taxes: Implications for Prices and the Abnormal January Returns," Journal of Financial Economics, vol. 13 (19841, 65-89.

9. Demsetz, Harold. "The Cost of Transacting," The Quarterly Journal of Economics, vol. 82, no. 1 (19681, 33-53.

10. Dyl, Edward A. "Capital Gains Taxation and Year-End Stock Market Behavior," Journal of Finance, vol. 32, no. 1 (March 1977), 165-75.

11. Givoly, Dan, and Arie Ovadia. "Year-End Tax-Induced Sales and Stock Market Seasonality," Journal of Finance, vol. 38, no. 1 (March 19831, 171-85.

12. Gultekin, Mustafa N., and N. Bulent Gultekin. "Stock Market Seasonality: International Evidence," Journal of Financial Economics, vol. 12 (19831, 469-81.

13. Harris, Lawrence, and Eitan Gurel. "Price and Volume Effects Associated with Changes in the S&P 500 List: New Evidence for the Existence of Price pressures," Journal of Finance, vol. 41, no. 4 (September 1986), 815-29.

14. Jaffe, Jeffrey, and Randolph Westerfield. "Patterns in Japanese Common Stock Returns: Day of the' Week and Turn of the Year Effects," Journal of Financial and Quantitative Analysis, vol. 20, no. 2 (June 1985), 261-72.

15. Keim, Donald B. "Size-Related Anomalies and Stock Return Seasonality: Further Empirical Evidence," Journal of Financial Economics, vol. 12, no. 1 (June 19831, 13-32.

16. ________________. "Dividend Yields and Stock Returns: Implications of Abnormal January Returns," Journal of Financial Economics, vol. 14, no. 3 (September 1985), 473-89.

17. Kross, William. "The Size Effect Is Primarily a Price Effect," Journal of Financial Research, vol. 8, no. 3 (19851, 169-79.

18. Lakonishok, Josef, and Seymour Smidt. "Volume and Turn-of-the-Year Behavior," Journal of Financial Economics, vol. 13, no. 3 (September 1984), 435-55.

19. ________________. "Volume for Winners and Losers: Taxation and Other Motives for Stock Trading," Journal of Finance, vol. 41, no. 4 (September 19861, 951-74.

20. Maddala, G.S. Econometrics. New York: McGraw-Hill, Inc., 1977.

21. Reinganum, Marc R. "Misspecification of Capital Asset Pricing: Empirical Anomalies Based on Earnings' Yields and Market Values," Journal of Financial Economics, vol. 9, no. 1 (March 19811, 19-46.

22. ________________. "The Anomalous Stock Market Behavior of Small Firms in January: Empirical Tests for Tax-Loss Selling Effects," Journal of Financial Economics, vol. 12, no. 1 (June 19831, 89-104.

23. Roll, Richard. "Vas Ist Das?: The Turn-of-the-Year Effect and the Return Premia of Small Firms," Journal of Portfolio Management, vol. 9, no. 2 (1983), 18-28.

24. ________________. "On Computing Mean Returns and the Small Firm Premium," Journal of Financial Economics, vol. 12, no. 3 (November 19831, 371-86.

25. Rozeff, Michael S. "The Tax-Loss Selling Hypothesis: New Evidence From Share Shifts," College of Business Administration Working Paper Series No. 85-16, The University of Iowa, May 1985.

26. ________________, and William R. Kinney, Jr. "Capital Market Seasonality: The Case of Stock Returns," Journal of Financial Economics, vol. 3, no. 4 (October 19761, 379-402.

27. Schultz, Paul. "Personal Income Taxes and the January Effect: Small Firm Stock Returns Before the War Revenue Act of 1971: A Note," Journal of Finance, vol. 40 (March 1985), 333-43.

28. Slemrod, Joel. "The Effect of Capital Gains Taxation on Year-End Stock Market Behavior," National Tax Journal, vol. 35, no. 1 (March 19821, 69-77.

29. Thomson, James B. "Tax-Selling Pressure and Errors in Recorded Security Prices: An Empirical Investigation of the Turn-of-the-Year Effect," Unpublished Ph.D. dissertation, The Ohio State University, August 1984.

30. Tinic, Seha M. "The Economics of Liquidity Services," Quarterly Journal of Economics, vol. 86, no. 1 (February 1972), 79-93.

31. ______________, and Richard R. West. "Risk and Return: January Vs. the Rest of the Year," Journal of Financial Economics, vol. 13, no. 4 (December 1984), 561-74.

32. White, Kenneth J. "A General Computer Program for Econometric Methods - SHAZAM," Econometrica, vol. 46, no. 1 (January 1978), 239-40.

NOTES

1. The one-eighth pricing convention applies to stocks priced $1.00 or higher. The minimum recorded price movement allowed for stocks priced between $0.50 and $1.00 is 1/16th of a dollar. The minimum price change for stocks priced under $0.50 is 1/32nd of a dollar.

2. The minimum spread for stocks priced between $0.50 and $1.00 is $0.0625, while for stocks priced under $0.50 the minimum spread is $0.03125.

3. This assumes that the risk-free rate of return, R_{ft}, is measured without error. The failure of this assumption should only affect estimates of a, from regressions on equation (3).

4. The recorded-price errors may also have nonzero means if the returns are calculated over holding periods subject to flow-supply or flow-demand pressures (that is, $E(\Lambda_{mt}) \neq 0$ and $E(\Lambda_{pt}) \neq 0$).

5. Note the bias in the estimate of β can be positive because the positive correlation between $E(\Lambda_{mt})$ and $E(\Lambda_{pt})$ violates the classical condition $Cov(\Lambda_{mt},\Lambda_{pt}) = 0$. If all of the classical conditions hold, then the bias in the β estimate would be negative.

6. Lakonishok and Smidt [18] and Thomson [29] discuss why it may be optimal for investors to exercise tax-timing options at the end of the tax year.

7. The use of the weekly Treasury bill rate as the daily risk-free rate of return assumes that the weekly term structure of interest rates is flat.

8. We use size and price deciles for the MV and PR portfolios in an attempt to replicate the experiments of previous papers in this area (see [15], [22], and [23]). Size and price quintiles are used for the SIZE, PRICE, and MVPR portfolios to ensure adequate diversification of these portfolios and because Chow tests fail to reject the pooling restriction for adjacent MV (PR) deciles.

9. If an investor sells a stock for a capital gain and receives payment for the stock in a different tax year from that of the sale, the investor has the option to declare the sale an installment sale. This gives the investor the option (which expires on April 15 of the year the payment is received) of realizing the gain in the tax year the sale was made or deferring the gain one additional year. Because trades are not settled for five days, stocks sold for capital gains during the last five trading days of the year qualify for treatment as installment sales. See Thomson [29] for a more thorough discussion of the installment-sale option and its implications for tax-gain selling at the end of the calendar year. The Tax Reform Act of 1986 removes this option for sales of stocks and bonds on organized exchanges.

10. Because the intercept term, a, is a projection of the regression line onto the Y-axis, a shift in a may simply reflect a shift in the market model β. This implies that if TOY-related slope seasonality is present then one must be very careful in interpreting the TOY-related shifts in intercept terms from regressions on the market model found by Keim [15] and Reinganum [22].

1 For β_1 we cannot reject the cross-equation equality restriction that β_1 equals zero for the MV, SIZE, and MVPR portfolios. We do reject the restriction that β_1 is equal across equations for the PR (PRICE) portfolios at the 5% (1%) significance level.

12. This argument can be made even stronger by noting that the estimated regression coefficients and test results for the MVPR portfolios are very close to those for the MV portfolios.

Table 1: Mean Daily Returns for MV and PR Portfolios
(Basis Points)

	MV1	MV2	MV3	MV4	MV5	MV6	MV7	MV8	MV9	MV10	F-TEST MV1=...=MV10
SAMPLE[a]	0.0520	0.0511	0.0472	0.0470	0.0405	0.0413	0.0400	0.0313	0.0307	0.0192	F(9,51190) = 0.837
ADJ YEAR[b]	0.0245	0.0294	0.0278	0.0296	0.0244	0.0273	0.0281	0.0214	0.0222	0.0131	F(9,49190) = 0.133
TOY[c]	0.7282	0.5839	0.5232	0.4729	0.4368	0.3856	0.3313	0.2736	0.2398	0.1703	F(9,1990) = 5.819†
PRE-YRND[d]	0.3286	0.3526	0.3408	0.3033	0.2966	0.3285	0.2994	0.3079	0.2761	0.2494	F(9,990) = 0.179
PST-YRND[e]	1.1278	0.8151	0.7057	0.6425	0.5771	0.4427	0.3632	0.2393	0.2036	0.0913	F(9,990) = 7.141†
	PR1	**PR2**	**PR3**	**PR4**	**PR5**	**PR6**	**PR7**	**PR8**	**PR9**	**PR10**	**F-TEST PR1=...=PR10**
SAMPLE	0.0521	0.0418	0.0479	0.0472	0.0431	0.0411	0.0329	0.0337	0.0325	0.0297	F(9,51190) = 0.458
ADJ YEAR	0.0179	0.0180	0.0279	0.0301	0.0280	0.0280	0.0225	0.0244	0.0263	0.0256	F(9,49190) = 0.140
TOY	0.8943	0.6278	0.5410	0.4667	0.4148	0.3618	0.2890	0.2615	0.1863	0.1304	F(9,1990) = 9.762'
PRE-YRND	0.4647	0.3083	0.3601	0.3511	0.3061	0.3070	0.2519	0.2677	0.2461	0.2374	F(4,495) = 1.038
PST-YRND	1.3420	0.9472	0.7213	0.5822	0.5237	0.4165	0.3262	0.2552	0.1265	0.0233	F(4,495) = 10.446'

a. SAMPLE = sample period: 5,120 observations.

b. ADJ YEAR = adjusted-year period: 4,920 observations.

c. TOY = turn-of-the-year period: 200 observations.

d. PRE-YRND = pre-yearend period: 100 observations.

e. PST-YRND = post-yearend period: 100 observations.

* Significant at 5%.

Significant at 1%.

Table 2: Mean Daily Returns for SIZE, PRICE, and MVPR Portfolios
(Basis Points)

	SIZE1	SIZE2	SIZE3	SIZE4	SIZE5	F-TEST SIZE1=...=SIZE5
SAMPLE[a]	0.0548	0.0474	0.0399	0.0388	0.0269	F(4,25595) = 0.864
ADJ YEAR[b]	0.0390	0.0290	0.0245	0.0276	0.0154	F(4,24595) = 0.585
TOY[c]	0.4278	0.5002	0.4182	0.3141	0.3086	F(4,995) = 1.550
PRE-YRND[d]	0.2954	0.3312	0.3197	0.3181	0.3073	F(4,495) = 0.042
PST-YRND[e]	0.5605	0.6686	0.5166	0.3102	0.3099	F(4,495) = 1.997

	PRICE1	PRICE2	PRICE3	PRICE4	PRICE5	F-TEST PRICE1=...=PRICE5
SAMPLE	0.0406	0.0478	0.0418	0.0348	0.0410	F(4,25595) = 0.160
ADJ YEAR	0.0123	0.0291	0.0280	0.0251	0.0346	F(4,24595) = 0.513
TOY	0.7376	0.5078	0.3827	0.2744	0.1994	F(4,995) = 8.361'
PRE-YRND	0.4343	0.3862	0.3105	0.2510	0.2563	F(4,495) = 1.038
PST-YRND	1.0408	0.6295	0.4550	0.2978	0.1425	F(4,495) = 8.054'

	MVPR1	MVPR2	MVPR3	MVPR4	MVPR5	F-TEST MVPR1=...=MVPR5
SAMPLE	0.0505	0.0464	0.0425	0.0302	0.0246	F(4,25595) = 0.934
ADJ YEAR	0.0213	0.0283	0.0282	0.0203	0.0201	F(4,24595) = 0.137
TOY	0.7685	0.4923	0.3946	0.2741	0.1342	F(4,995) = 10.494'
PRE-YRND	0.3597	0.3031	0.2968	0.2775	0.2307	F(4,495) = 0.366
PST-YRND	1.1773	0.6814	0.4923	0.2707	0.0378	F(4,495) = 12.387'

a. SAMPLE = sample period: 5,120 observations.

b. ADJ YEAR = adjusted-year period: 4,920 observations.

c. TOY = turn-of-the-year period: 200 observations.

d. PRE-YRND = pre-yearend period: 100 observations.

e. PST-YRND = post-yearend period: 100 observations.

* Significant at 5%.

† Significant at 1%.

Table 3: OLS Regression Results Using MV Portfolios

	α_1	α_2	α_3	β_1	β_2	β_3	$\bar{R}^2$
MV1	.00133* (.00066)[a]	.00999† (.00064)	.00016 (.00009)	.01084 (.09571)	.46875† (.07072)	.66124† (.01148)	.4352
MV2	.00115* (.00059)	.00675† (.00056)	.00019* (.00008)	.04291 (.08460)	.36867† (.06251)	.78742† (.01015)	.5664
MV3	.00092 (.00053)	.00569† (.00051)	.00017* (.00007)	.08644 (.07600)	.30872† (.05616)	.83474† (.00911)	.6417
MV4	.00057 (.00047)	.00500† (.00046)	.00018† (.00006)	.03636 (.06841)	.28919† (.05054)	.90557† (.00820)	.7199
MV5	.00062 (.00043)	.00441† (.00041)	.00013* (.00006)	-.02335 (.06216)	.27277† (.04593)	.91990† (.00745)	.7583
MV6	.00099* (.00039)	.00305† (.00037)	.00015† (.00005)	-.05582 (.05584)	.24410† (.04126)	.92876† (.00670)	.8001
MV7	.00071* (.00034)	.00232† (.00033)	.00017† (.00005)	-.06980 (.04912)	.20426† (.03629)	.89398† (.00589)	.8266
MV8	.00086† (.00028)	.00121† (.00027)	.00010* (.00004)	-.07086 (.04109)	.12363† (.03036)	.91750† (.00493)	.8768
MV9	.00043 (.00023)	.00089† (.00022)	.00010† (.00003)	-.03605 (.03361)	.07418† (.02483)	.91944† (.00403)	.9142
MV10	.00014 (.00013)	-.00012 (.00012)	.00001 (.00002)	-.04253* (.01830)	.01550 (.01372)	.96933† (.00219)	.9755

a. Standard error ( ).

* Significant at 5%.

† Significant at 1%.

Table 4: **OLS** Regression Results Using PR Portfolios

	α_1	α_2	α_3	β_1	β_2	β_3	$\bar{R}^2$
PR1	.00218$^+$	.01178$^+$	.00008	.12956	.54036$^+$	.78469$^+$	.4598
	(.00075)[a]	(.00072)	(.00010)	(.10834)	(.08005)	(.01299)	
PR2	.00045	.00805$^+$	.00007	.12682	.40242$^+$	.89180$^+$	.6223
	(.00059)	(.00057)	(.00008)	(.08547)	(.06312)	(.01024)	
PR3	.00087	.00573$^+$	.00017*	.10083	.35123$^+$	.90863$^+$	.6994
	(.00050)	(.00057)	(.00007)	(.07254)	(.05360)	(.00870)	
PR4	.00096*	.00439$^+$	.00019$^+$	.02874	.29763$^+$	.89472$^+$	.7447
	(.00439)	(.00042)	(.00006)	(.06341)	(.04866)	(.00760)	
PR5	.00084*	.00388$^+$	.00017$^+$	-.03098	.28262$^+$	.87628$^+$	.7864
	(.00038)	(.00037)	(.00005)	(.05515)	(.04075)	(.00661)	
PR6	.00101$^+$	.00290$^+$	.00017$^+$	-.06247	.20376$^+$	.84463$^+$	.8092
	(.00034)	(.00033)	(.00005)	(.04929)	(.03642)	(.00591)	
PR7	.00044	.00207$^+$	.00012$^+$	-.05415	.19311$^+$	.83231$^+$	.8339
	(.00031)	(.00030)	(.00004)	(.04456)	(.03293)	(.00534)	
PR8	.00057*	.00138$^+$	.00013$^+$	-.06468	.13824$^+$	.85072$^+$	.8610
	(.00028)	(.00027)	(.00004)	(.04088)	(.03020)	(.00490)	
PR9	.00030	.00011	.00015$^+$	-.09634*	.07426$^+$	.88358$^+$	.8796
	(.00027)	(.00026)	(.00004)	(.03895)	(.02878)	(.00467)	
PR10	.00009	-.00084$^+$	.00013$^+$	-.13239$^+$	-.05914*	.96367$^+$	.9245
	(.00023)	(.00022)	(.00003)	(.03266)	(.02413)	(.00392)	

a. Standard error ( ).

* Significant at 5%.

Significant at 1%.

Table 5: OLS Regression Results with SIZE, PRICE, and MVPR Portfolios

	α_1	α_2	α_3	β_1	β_2	β_3	$\bar{R}^2$
SIZE1	.00088 (.00052)[a]	.00436† (.00050)	.00030† (.00007)	-.02591 (.07516)	.22259† (.05554)	.67654† (.00902)	.5437
SIZE2	.00096 (.00049)	.00533† (.00048)	.00018† (.00007)	.05058 (.07124)	.26976† (.05264)	.85009† (.00855)	.6768
SIZE3	.00085* (.00041)	.00380† (.00039)	.00013* (.00005)	-.04228 (.05865)	.25889† (.04334)	.92874† (.00704)	.7841
SIZE4	.00084† (.00031)	.00180† (.00030)	.00016† (.00004)	-.08375 (.04475)	.15634† (.03306)	.94055† (.00537)	.8633
SIZE5	.00071† (.00024)	.00195† (.00023)	.00004 (.00003)	.01762 (.03499)	.14694† (.02586)	.91570† (.00420)	.9075
PRICE1	.00115 (.00069)	.00895† (.00067)	-.00001 (.00009)	.24994* (.10002)	.35829† (.07390)	1.0405† (.01201)	.6189
PRICE2	.00123† (.00044)	.00485† (.00043)	.00018† (.00006)	.05516 (.06421)	.30812† (.04745)	.90412† (.00771)	.7443
PRICE3	.00103† (.00033)	.00325† (.00032)	.00017† (.00005)	-.05562 (.04831)	.23499† (.03569)	.84056† (.00580)	.8143
PRICE4	.00419 (.00028)	.00179† (.00027)	.00014† (.00004)	-.06434 (.04065)	.15620† (.03004)	.83698† (.00488)	.8584
PRICE5	.00056 (.00036)	-.00025 (.00035)	.00024† (.00005)	-.17048† (.05187)	.05322 (.03833)	.84677† (.00623)	.7902
MVPR1	.00135* (.00067)	.01032† (.00064)	.00012 (.00009)	.06352 (.09614)	.53039† (.07104)	.74065† (.01154)	.4909
MVPR2	.00040 (.00051)	.00534† (.00050)	.00017* (.00007)	.07349 (.07420)	.34605† (.05482)	.90062† (.00891)	.6848
MVPR3	.00070 (.00041)	.00355† (.00040)	.00017† (.00006)	-.03448 (.05971)	.24840† (.04412)	.90204† (.00717)	.7677
MVPR4	.00069* (.00032)	.00154† (.00031)	.00010* (.00004)	-.05387 (.04659)	.17790† (.03443)	.84367† (.00559)	.8246
MVPR5	-.00004 (.00017)	-.00069† (.00016)	.00008† (.00002)	-.07877† (.02460)	-.02117 (.01819)	.96679† (.00295)	.9560

a. Standard error ( ).

* Significant at 5%.

† Significant at 1%.

Table 6: F-Tests of Cross-Equation Equality Restrictions

F-TEST: $\alpha_{1,1} = \alpha_{1,2} = \cdots = \alpha_{1,10}$

MV Portfolios: F(9,51190) = 1.584
PR Portfolios: F(9,51190) = 1.501

F-TEST: $\alpha_{1,1} = \alpha_{1,2} = \cdots = \alpha_{1,5}$

SIZE Portfolios: F(4,25595) = 0.071
PRICE Portfolios: F(4,25595) = 0.970
MVPR Portfolios: F(4,25595) = 2.192

F-TEST: $\alpha_{2,1} = \alpha_{2,2} = \cdots = \alpha_{2,10}$

MV Portfolios: F(9,51190) = 70.073†
PR Portfolios: F(9,51190) = 71.400†

F-TEST: $\alpha_{2,1} = \alpha_{2,2} = \cdots = \alpha_{2,5}$

SIZE Portfolios: F(4,25595) = 17.233†
PRICE Portfolios: F(4,25595) = 44.348†
MVPR Portfolios: F(4,25595) = 109.542†

F-TEST: $\alpha_{3,1} = \alpha_{3,2} = \cdots = \alpha_{3,10}$

MV Portfolios: F(9,51190) = 3.402†
PR Portfolios: F(9,51190) = 0.348

F-TEST: $\alpha_{3,1} = \alpha_{3,2} = \cdots = \alpha_{3,5}$

SIZE Portfolios: F(4,25595) = 3.678†
PRICE Portfolios: F(4,25595) = 1.528
MVPR Portfolios: F(4,25595) = 0.839

F-TEST: $\beta_{1,1} = \beta_{1,2} = \cdots = \beta_{1,10}$

MV Portfolios: F(9,51190) = 0.695
PR Portfolios: F(9,51190) = 2.292*

F-TEST: $\beta_{1,1} = \beta_{1,2} = \cdots = \beta_{1,5}$

SIZE Portfolios: F(4,25595) = 1.605
PRICE Portfolios: F(4,25595) = 4.307†
MVPR Portfolios: F(4,25595) = 1.393

F-TEST: $\beta_{2,1} = \beta_{2,2} = \cdots = \beta_{2,10}$

MV Portfolios: F(9,51190) = 16.111†
PR Portfolios: F(9,51190) = 17.620†

F-TEST: $\beta_{2,1} = \beta_{2,2} = \cdots = \beta_{2,5}$

SIZE Portfolios: F(4,25595) = 2.221
PRICE Portfolios: F(4,25595) = 6.740†
MVPR Portfolios: F(4,25595) = 29.296†

F-TEST: $\beta_{3,1} = \beta_{3,2} = \cdots = \beta_{3,10}$

MV Portfolios: F(9,51190) = 134.923†
PR Portfolios: F(9,51190) = 73.063†

F-TEST: $\beta_{3,1} = \beta_{3,2} = \cdots = \beta_{3,5}$

SIZE Portfolios: F(4,25595) = 183.451†
PRICE Portfolios: F(4,25595) = 74.240†
MVPR Portfolios: F(4,25595) = 167.667†

Notes: * Significant at 5%.

† Significant at 1%.