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The January Effect

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Analysis of broad samples of value-weighted and equal-weighted returns of U.S. equities documents that abnormally high rates of return on small-capitalization stocks continue to be observed during the month of January. This January effect in small-cap stock returns is remarkably consistent over time and does not appear to have been affected by passage of the Tax Reform Act of 1986. This finding brings new perspective to the tax-loss selling hypothesis and suggests that behavioral explanations are relevant to the January effect. After a generation of intensive study, the January effect continues to present a serious challenge to the efficient market hypothesis.

In a seminal article, Rozeff and Kinney (1976), using an equal-weighted index of NYSE prices, reported evidence of a seasonal pattern in stock market returns.[1] From 1904 through 1974, the average stock market return during the month of January was 3.48 percent whereas the monthly return during the remaining 11 months of the year was 0.42 percent. January returns appeared to be more than eight times higher than returns for a typical month. Because the equal-weighted NYSE index represented a simple average of the stock prices for all listed companies, the Rozeff and Kinney methodology gave small companies greater relative influence than would be true in a value-weighted index, where large companies dominate. Indeed, subsequent research by Reinganum (1983) and Roll (1983), among others, confirmed that this January effect is a small-capitalization phenomenon.

Various studies have suggested that the January effect may arise from the prevalence of end-of-year "window dressing" by professional investors seeking to eliminate embarrassing losers from their portfolios prior to the end of important reporting periods. For example, Lakonishok, Shleifer, Thaler, and Vishny (1991) argued that portfolio returns are noisy, so sponsors examine individual portfolio holdings to gain additional perspective on an investment manager's investment philosophy and execution. According to this window-dressing hypothesis, institutional investors are evaluated on both their investment results and the consistency of their investment philosophy. At the end of the calendar year or any important reporting period, institutional investors may be prone to sell losers and buy winners to improve perceived performance. A reasonable assumption, however, is that window dressing by large institutional investors, if present, would be a large-cap phenomenon. So, the window-dressing hypothesis may have limited relevance for explaining the January effect if the effect is restricted to small-cap stocks.

At the level of the individual investor, Ritter (1988) found that end-of-year price movements of small companies tend to be related to the buying and selling habits of "small" investors. He argued that during December, individuals apparently sell stocks that have declined in price to realize the tax losses. These investors then apparently wait until January to reinvest (in a broad cross-section of small-cap stocks) because January buying can be augmented by cash infusions from year-end bonuses or from the sales of large-cap stocks on which long-term capital gains are being realized. By focusing on the abrupt switch to net buying by individual investors at the turn of the year, Ritter offered a "parking the proceeds" explanation as to why the January effect is largely confined to small-cap stocks, especially small-cap stocks that performed poorly during the prior year.

In findings consistent with this hypothesis, D'Mello, Ferris, and Hwang (2003) observed abnormal selling pressure prior to the year-end for stocks that experienced large capital losses and observed that individual investors postpone the sale of stocks that experienced capital gains until after the New Year. There also appears to be a significant decrease in the average trade size for stocks with large capital losses before the year-end and for stocks with capital gains in the New Year. These findings suggest that individuals, rather than institutional investors, are the major sellers around the year-end and that individual tax-loss selling is the fundamental explanation for abnormal January returns.

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Although tax effects have long presented a plausible explanation of the January effect in the United States, international evidence also suggests a January effect in stock returns for countries with different tax years and in countries with no capital gains tax. Moreover, in the United States, any seasonal tendency related to tax-motivated selling by institutional investors is sure to have been influenced by the Tax Reform Act of 1986 (see Bhabra, Dhillon, and Ramirez 1999). Since passage of the Tax Reform Act, mutual funds have been required to distribute at least 98 percent of realized capital gains and dividend income generated during the 12-month period ending 31 October. Since 1986, net capital gains distributions to mutual fund shareholders have been determined without regard to capital losses attributable to transactions occurring during the last two months of the calendar year. Capital losses incurred by mutual funds during the last two months of the year are carried over to the subsequent taxable year. Any seasonal tendencies related to tax-motivated selling by institutional investors after 1986 should thus occur well before the end of the calendar year. Because many mutual funds have retained a January–December reporting period despite a new November–October tax period, the potential exists to use post–Tax Reform Act data to distinguish between window dressing and tax-motivated seasonality caused by institutional investors. If a January effect largely confined to small-cap stocks has persisted since 1986, such a finding would offer support for Ritter's (1988) conjecture concerning the anomalous buying and selling behavior of individual investors at the turn of the year.

In the study reported here, we sought to make a number of contributions. First, using 1802–1926 data from Schwert (1990) and 1927–2004 data on CRSP value-weighted portfolios, we have updated evidence on the January effect among large-cap stocks with 202 years of evidence. Second, we used the 78 years of CRSP equal-weighted portfolio returns (1927–2004) to analyze the January effect in small-cap stocks. In particular, we studied whether the January effect for small-cap stocks persisted after passage of the Tax Reform Act of 1986. Third, using Fama and French (1993) size, book-to-market, and momentum factors, we have explored the underlying causes of the January effect.

Value-Weighted Returns

Studying monthly returns for the DJIA, a popular price-weighted market index of 30 giant corporations, Lakonishok and Smidt (1988) found no evidence of a January effect.[2] Similarly, Schwert (1990) found little evidence of monthly seasonality in value-weighted indices, such as CRSP, in which small-cap stocks have little weight. The absence of a January effect in large-cap returns supports the notion that the anomalous January effect is largely a small-cap phenomenon. It is also consistent with the hypothesis that measurement errors, rather than inefficiencies in market pricing, are a root cause for the perception of abnormally high rates of return for small-cap stocks during the month of January. Lakonishok and Smidt argued that small trading volumes and large bid–ask spreads among small-cap stocks make profitable trading on the January effect difficult. Without profitable trading opportunities, the January effect becomes more a statistical oddity than compelling evidence of market inefficiency.[3]

A related statistical explanation for the January effect is commonly referred to as the "data-snooping hypothesis" (Lo and MacKinlay 1990; Sullivan, Timmerman, and White 1999). Much, if not all, of the January effect may be a statistical artifact tied to investment period selection. In the past century in the United States, there have been notable periods of outstanding relative performance for large-cap stocks during the month of January; one such period was the 1980s. In other periods, such as the 1990s, January returns on large-cap stocks were unremarkable. Such deviations may be well within the realm of typical statistical variation. Historically brief advantages for investing during January may simply represent the type of inexplicable pattern in stock market returns that can be uncovered by diligent data snooping. Unfortunately for those seeking risk-free arbitrage opportunities tied to the calendar, anomalous evidence of above-average January returns may be both inexplicable and inherently fragile. In a vigorously competitive equity market, positive (or negative) abnormal January returns would tend to reverse over a reasonably brief time frame (see Fama 1998; Malkiel 2003).

Table 1 and **Table 2** show value-weighted portfolio returns for various periods in our study.[4] For these value-weighted data, monthly returns were calculated from Schwert's (1990) indices of U.S. stock prices for 1802–1926 and from CRSP value-weighted portfolio returns for 1927–2004. In light of the potential for unpredictable and transitory influences, such a long-term perspective is useful when considering the possibility of a January effect in stock returns. Sullivan et al. (2001) argued that the stability of returns across subperiods provides important information about the robustness of the January effect. For example, if abnormally high January returns are observed over a range of important subperiods, the implication is that the anomalous returns could form the basis for

Table 1. Value-Weighted Portfolio Returns for January vs. the Other 11 Months of the Year, 1802–2004 and Subperiods

Period and Measure	January	Other 11 Months	Paired Difference
1802–2004			
n	202	2,229	202
Mean	1.10%	0.70%	0.40%
Standard deviation	4.09%	4.58%	4.24%
Median	0.55%	0.57%	–0.09%
Percent positive	60.9%	61.4%	48.0%
Sign test (*z*-stat.)	3.10	10.78	–0.56
Sign test (prob.)	0.0010	0.0000	0.7132
1802–1926			
n	124	1,371	124
Mean	0.65%	0.60%	0.07%
Standard deviation	3.61%	3.85%	3.70%
Median	0.51%	0.46%	–0.20%
Percent positive	57.3%	61.3%	44.4%
Sign test (*z*-stat.)	1.62	8.35	–1.26
Sign test (prob.)	0.0530	0.0000	0.8957
1927–2004			
n	78	858	78
Mean	1.81%	0.87%	0.94%
Standard deviation	4.70%	5.55%	4.97%
Median	1.57%	1.31%	0.29%
Percent positive	66.7%	61.7%	53.8%
Sign test (*z*-stat.)	2.94	6.83	0.68
Sign test (prob.)	0.0016	0.0000	0.2485
1927–1952 (May)			
n	26	286	25
Mean	1.93%	0.79%	1.16%
Standard deviation	4.26%	7.54%	5.64%
Median	1.57%	1.25%	–0.82%
Percent positive	73.1%	60.5%	44.0%
Sign test (*z*-stat.)	2.35	3.55	–0.60
Sign test (prob.)	0.0093	0.0002	0.7257
1952 (June)–2004			
n	52	572	52
Mean	1.75%	0.92%	0.83%
Standard deviation	4.94%	4.21%	4.72%
Median	1.65%	1.33%	1.32%
Percent positive	63.5%	62.2%	57.7%
Sign test (*z*-stat.)	1.94	5.85	1.11
Sign test (prob.)	0.0261	0.0000	0.1336
1952 (June)–1986			
n	34	374	34
Mean	1.53%	0.91%	0.61%
Standard deviation	5.25%	4.04%	4.87%
Median	0.94%	1.13%	0.82%
Percent positive	58.8%	61.5%	52.9%
Sign test (*z*-stat.)	1.03	4.45	0.34
Sign test (prob.)	0.1517	0.0000	0.3658
1987–2004			
n	18	198	18
Mean	2.16%	0.92%	1.25%
Standard deviation	4.40%	4.54%	4.52%
Median	2.56%	1.53%	1.32%
Percent positive	72.2%	63.6%	66.7%
Sign test (*z*-stat.)	1.89	3.84	1.41
Sign test (prob.)	0.0297	0.0001	0.0786

Table 2. January Return Premium for Value-Weighted Returns by Decade, 1803–2004

Decade	Return Premium
1803–1809	–0.98%
1810–1819	–0.18
1820–1829	–0.54
1830–1839	1.46
1840–1849	–3.54
1850–1859	–0.79
1860–1869	0.53
1870–1879	1.91
1880–1889	–0.41
1890–1899	3.34
1900–1909	–0.22
1910–1919	–0.29
1920–1929	0.25
1930–1939	2.33
1940–1949	0.10
1950–1959	0.00
1960–1969	0.65
1970–1979	1.61
1980–1989	2.45
1990–1999	0.27
2000–2004	–0.38

successful trading rules that would outperform market benchmarks. Such consistency would also suggest that investors could have adopted a recursive decision rule to generate genuinely superior out-of-sample performance.

In Table 1, the results for the 1802–1926 and 1927–2004 subperiods show the amount of return consistency between January returns and January return premiums (paired differences) estimated from Schwert's (1990) indices of U.S. stock prices at a time when Saturday trading was allowed and more recent CRSP value-weighted portfolios since the five-day trading week was adopted. CRSP value-weighted portfolio results for 1927 through May 1952 and for 1952 through June 2004 reflect this consistency between the old and modern eras. The division of CRSP value-weighted results into the June 1952–1986 and 1987–2004 sample subperiods allowed us to analyze any effects of the Tax Reform Act of 1986, which should have eliminated any tendencies related to year-end tax-motivated selling by institutional investors. With this plausible explanation for the January effect in the post-1986 period isolated by the choice of periods, we have the basis for a natural experiment to distinguish between window-dressing and tax-motivated seasonality in the 1987–2004 period.

Over the entire 1802–2004 period, as Table 1 shows, the mean value-weighted return during the month of January was only 1.10 percent and the median, at 0.55 percent, was about half that amount. High volatility is evident. The January return premium was positive 60.9 percent of the time, and the sign test is statistically significant under conventional criteria. Of course, stocks usually go up, so the typically positive value-weighted January returns should not be surprising.

It is interesting that January returns and January return premiums for large-cap stocks (CRSP value-weighted portfolio returns) are more uniformly positive in the 1927–2004 time frame than in the early period. The fact that value-weighted January returns and January return premiums remained unusually positive during the period following the Tax Reform Act of 1986 offers some support for the window-dressing hypothesis. Beginning in 1987, institutional investors have no tax motivation for selling losers at the turn of the year, so the persistence of abnormally higher January returns for large-cap stocks since 1987 is consistent with the notion that professional investors seek to eliminate embarrassing losers from their portfolios prior to the end of important reporting periods.

Table 2 shows by decade (except the 2000s) value-weighted return premiums for the month of January relative to the other months in the year—that is, the value-weighted monthly rate of return earned during January minus the average rate of return earned in the 11 other months of that year. As shown, value-weighted portfolio return premiums for the month of January have been at various times both sharply negative and robustly positive. The worst decade for value-weighted January return premiums was the 1840s (–3.54 percent); the best decade was the 1890s (+3.34 percent). In terms of these relative-return data, January returns in recent years have provided only a modest advantage.

Equal-Weighted Returns

Table 3 shows equal-weighted January returns minus the average rate of return earned in the other 11 months of the year by decade (three years for the 1920s and five years for the 2000s). Abnormally high equal-weighted January return premiums are observable for the entire 1927–2004 period. The worst full decade for equal-weighted January return premiums was the 1950s, and the best decade was the 1970s. Note that the explosion of research in financial economics concerning calendar anomalies in general, and the January effect in particular, coincides with the best decade for January return premiums. When measured as equal-weighted returns, the January effect remained persistently positive despite the attention focused on the issue.

Table 3. January Return Premium for Equal-Weighted Returns by Decade, 1927–2004

Decade	Return Premium
1927–1929	1.84%
1930–1939	7.56
1940–1949	3.73
1950–1959	2.47
1960–1969	4.04
1970–1979	8.77
1980–1989	5.63
1990–1999	4.11
2000–2004	6.44

For the entire 1927–2004 period, **Table 4** shows that equal-weighted January returns averaged a whopping 6.05 percent and January return premiums averaged a stunning 5.14 percent. As with the larger period, high volatility is evident. Again, the positive sign test for equal-weighted January returns should not be surprising, because stocks usually go up. What is surprising is that neither Wall Street attention nor popular press coverage has eliminated January effect arbitrage opportunities. Equal-weighted January return premiums have remained uniformly above average throughout the entire period for which CRSP equal-weighted portfolio returns are available.

Note in particular that the premiums remained positive after enactment of the Tax Reform Act of 1986 and cannot be attributed to seasonally motivated tax selling by institutional investors. The persistence since 1987 of abnormally high January returns and January return premiums for equal-weighted portfolios, in which small-cap stocks play a larger role than in value-weighted portfolios, is consistent with the notion that professional investors seek to eliminate embarrassing small-cap losers from their portfolios prior to the end of important reporting periods. The persistence of the January effect after 1986 and the fact that the January effect remains largely a small-cap phenomenon also lend support to Ritter's (1988) conjecture concerning anomalous buying and selling behavior of individual investors at the turn of the year.

Fama–French Factors

The data presented in Tables 1–4 suggest that the January effect is largely a small-cap phenomenon. This notion can be tested directly by using the Fama–French benchmark factors for 1927–2004.

Factor Returns. The famous Fama–French benchmark factors summarize the performance of small-cap stocks relative to large-cap stocks (Small minus Big, or SMB) and the performance of value stocks (companies with *high* book-to-market ratios) relative to the performance of growth stocks (companies with *low* book-to-market ratios) for a High minus Low (HML) factor.

The Fama–French benchmark portfolios are rebalanced quarterly by using independent sorts on size (market value of equity) and the ratio of book value of equity to market value of equity. The size breakpoint used to determine the buy range for the Fama–French small and big portfolios is median NYSE market equity. Value stocks are considered to be those with high book-to-market ratios (B/Ms); growth stocks are those with low B/Ms. The Fama–French benchmark factors are constructed from six size-and-B/M portfolios without considering transaction costs. B/M breakpoints used to determine the buy range for the growth, neutral, and value portfolios are the 30th and 70th NYSE percentiles.[5]

SMB is the average return on three small-cap portfolios minus the average return on three large-cap portfolios:

$$\text{SMB} = 1/3(\text{Small Value} + \text{Small Neutral} + \text{Small Growth}) - 1/3(\text{Big Value} + \text{Big Neutral} + \text{Big Growth}).$$

HML is the average return on two value-stock portfolios minus the average return on two growth-stock portfolios:

$$\text{HML} = 1/2(\text{Small Value} + \text{Big Value}) - 1/2(\text{Small Growth} + \text{Big Growth}).$$

Fama–French also investigated six value-weighted portfolios formed on the basis of size and momentum—measured as the prior 2-month to 12-month returns. The momentum factor, UMD (Up minus Down) is the average return on the two high-prior-return portfolios minus the average return on the two low-prior-return portfolios:

$$\text{UMD} = 1/2(\text{Small High} + \text{Big High}) - 1/2(\text{Small Low} + \text{Big Low}).$$

These portfolios were formed monthly and reflect the intersection of two portfolios formed on the basis of size (market equity) and three portfolios formed on the basis of momentum. The monthly size breakpoint is the median NYSE market equity. The monthly momentum breakpoints are the 30th and 70th NYSE percentiles.

The six portfolios used to construct UMD each month include NYSE, Amex, and NASDAQ stocks with prior-return data. To be included in a portfolio for month t (formed at the end of month $t-1$), a stock had to have a price for the end of month $t-13$ and a return for month $t-2$. Each stock included also had to have a market value of equity for the end of month $t-1$.

Table 4. Equal-Weighted Portfolio Returns for January vs. the Other 11 Months of the Year, 1927–2004 and Subperiods

Period and Measure	January	Other 11 Months	Paired Difference
1927–2004			
n	78	858	78
Mean	6.05%	0.91%	5.14%
Standard deviation	7.18%	7.37%	7.56%
Median	4.47%	1.22%	3.78%
Percent positive	82.1%	59.4%	79.5%
Sign test (*z*-stat.)	5.66	5.53	5.21
Sign test (prob.)	0.0000	0.0000	0.0000
1927–1952 (May)			
n	26	286	25
Mean	5.99%	1.08%	5.07%
Standard deviation	7.88%	10.62%	9.41%
Median	4.15%	1.39%	1.73
Percent positive	80.8%	57.3%	72.0%
Sign test (*z*-stat.)	3.14	2.48	2.20
Sign test (prob.)	0.0009	0.0065	0.0139
1952 (June)–2004			
n	52	572	52
Mean	6.07%	0.82%	5.25%
Standard deviation	6.89%	5.02%	6.65%
Median	5.64%	1.21%	4.60%
Percent positive	82.7%	60.5%	82.7%
Sign test (*z*-stat.)	4.71	5.02	4.71
Sign test (prob.)	0.0000	0.0000	0.0000
1952 (June)–1986			
n	34	374	34
Mean	5.95%	0.83%	5.12%
Standard deviation	7.43%	4.91%	6.99%
Median	4.28%	1.18%	3.36%
Percent positive	76.5%	60.7%	79.4%
Sign test (*z*-stat.)	3.09	4.14	3.43
Sign test (prob.)	0.0010	0.0000	0.0003
1987–2004			
n	18	198	18
Mean	6.30%	0.80%	5.50%
Standard deviation	5.91%	5.24%	6.16%
Median	6.30%	1.43%	5.20%
Percent positive	94.4%	60.1%	88.9%
Sign test (*z*-stat.)	3.77	2.84	3.30
Sign test (prob.)	0.0001	0.0022	0.0005

We found the strong January returns to be consistently positively related to the SMB and HML factors and negatively related to momentum (UMD). Over the entire 1927–2004 period, the average January SMB factor return was 2.56 percent more than for the other 11 months of the year, with a standard deviation of 3.23 percent. The median January SMB factor return premium was 2.25 percent; January SMB factor return premiums were positive 76.9 percent of the time ($z = 4.76$). January SMB factor return premiums were of the same sign and order of magnitude as January HML factor return premiums. Over this period, the average January HML factor return premium was 2.54 percent, with a standard deviation of 4.01 percent. The median January HML factor return premium was 1.98 percent; January HML factor return premiums were positive 75.6 percent of the time ($z = 4.53$). These findings strongly suggest that abnormally high January returns can be attributed to both company size and B/M effects. **Table 5** depicts Fama–French SMB and HML factor return premiums for January relative to

Table 5. January Return Premiums for Portfolios Based on SMB, HML, and UMD Factors by Decade, 1927–2004

Decade	Return Premium for SMB Portfolios	Return Premium for HML Portfolios	Return Premium for UMD Portfolios
1927–1929	1.67%	0.49%	–1.45%
1930–1939	3.33	5.02	–4.11
1940–1949	3.06	2.03	–2.32
1950–1959	2.32	2.18	–2.58
1960–1969	2.36	2.69	–2.97
1970–1979	5.05	3.76	–2.91
1980–1989	0.60	1.30	–2.17
1990–1999	0.99	0.94	–1.63
2000–2004	2.69	0.85	–3.83

the 11 other months of the year by decade. Notice that the positive premium for January SMB and HML factor returns is quite variable over time. The positive premium for January HML factor returns also appears to have diminished in recent years.

The positive relationship evident between January returns and Fama–French SMB and HML factors is in stark contrast with the consistently negative influence of momentum. Over the 1927–2004 period, the average January UMD factor return was –1.72 percent lower than in the other 11 months of the year, with a standard deviation of 4.82 percent. The median January UMD factor return discount was –1.26 percent; January UMD factor return premiums were positive only 41.0 percent of the time ($z = –1.59$). In Table 5, the negative influence of momentum (UMD) on January returns appears quite consistent over the decades. These findings confirm that superior January returns remain highest for small companies with negative annual returns in the prior period and that the January effect is not observed for small-cap "winners," as discovered by Reinganum (1983).

Like the findings reported by D'Mello et al. (2003), our results are consistent with tax-loss selling by small investors at the turn of the year. These findings also support Ritter's (1988) parking-the-proceeds explanation as to why the January effect is largely confined to small-cap stocks, especially small-cap stocks that have performed poorly during the prior year.

Portfolio Return Comparison. Additional perspective on the relative importance of Fama–French risk factors for January returns can be gained by considering typical differences between January returns and returns for the 11 other months of the year for various size-and-B/M portfolios. **Table 6** shows the average rate of return for January minus the average return earned over the 11 other months of the year for six Fama–French portfolios. For the entire 1927–2004 period, the average January return premium grows monotonically for small-cap and large-cap stocks as we move from Growth to Neutral to Value. The small-cap stocks, however, always maintain an advantage over the large-cap stocks. Thus, both size and book-to-market effects appear to be at work, but size effects predominate. The January effect remains largely a small-cap phenomenon.

Table 6. Comparison of January Return Premiums for Six Fama–French Portfolios, 1927–2004

Portfolio	Return Premium
Small	
Growth	2.94%
Neutral	3.21
Value	5.22
Big	
Growth	0.17%
Neutral	1.07
Value	2.62

Post–Tax Reform Act Results

The January effect is a classic example of what Sullivan et al. (2001) referred to as a "data-driven discovery." No *a priori* theory caused researchers to look for anomalous turn-of-the-year trading behavior by institutional or individual investors. Rather, the discovery of the January effect caused researchers to hypothesize that tax-loss selling or other behavior was going on that resulted in anomalous turn-of-the-year stock market returns. The result is an empirical problem because we cannot typically test data-driven hypotheses on information that is independent of the data used to derive them. The practice of using the same dataset to formulate and test hypotheses introduces data-mining biases that, if not accounted for, invalidate the assumptions underlying classical statistical

inference. For example, Sullivan et al. (2001) argued that after accounting for the distortions to statistical inference caused by data mining, the January effect no longer remains significant.

In considering the relevance of the data-mining or data-snooping problem to tests of the January effect, we must keep two points in mind: (1) Sullivan et al. (2001) studied Lakonishok and Smidt's (1988) sample of DJIA (large-cap) returns to evaluate the January effect, but our results confirm that the January effect is largely a small-cap phenomenon. Tests based on equal-weighted portfolios are uniformly more suggestive of a January effect than similar tests based on value-weighted (or DJIA) portfolios. (2) New and interesting data are now available for testing the January effect. Rozeff and Kinney's (1976) seminal evidence of a January effect was based on equal-weighted portfolio evidence for 1904–1974. More recent evidence provides an independent basis for evaluating the statistical importance of a January effect in small-cap returns. In particular, the 1987–2004 period following passage of the Tax Reform Act of 1986 gives the basis for discriminating between competing behavioral explanations of the January effect, if present.

Since passage of the Tax Reform Act of 1986, mutual funds have been required to distribute at least 98 percent of realized capital gain income they generated during the 12-month period ending 31 October. Since 1986, net capital gains distributions to mutual fund shareholders have been determined without regard to capital losses attributable to transactions occurring during the last two months of the calendar year, which are carried over to the next tax year. In the period since passage of the Tax Reform Act, therefore, any seasonal tendencies related to tax-motivated selling by institutional investors should occur well before the end of the calendar year. Because many mutual funds retain a January–December reporting period despite a new November–October tax period, the potential exists to use post-1986 data to distinguish between window dressing for reporting purposes and tax-motivated seasonality. Finding a persistent January effect over the 1987–2004 period that is largely confined to small-cap stocks would also support Ritter's (1988) conjecture that the buying and selling behavior of individual investors causes anomalous returns at the turn of the year.

To establish the statistical significance of a January effect in the period following enactment of the Tax Reform Act of 1986, we tested 1987–2004 data on a year-by-year basis and evaluated the cumulative evidence at any point in time.[6] We considered "decision cones" that permitted us to reject the "no January effect" hypothesis if the January cumulative return for any year, R_{Ci}, was greater than the critical value $R_{Ci} > \theta$, where

$$\theta \cong e^{\Phi\left\{\exp\left[\ln(1-\alpha)/n\right],\, \mu_{n\bar{y}},\, \sigma\sqrt{n}\right\}} - 1,$$

in which

Φ = cumulative normal distribution
α = tolerable amount of global Type 1 error permitted
μ = mean return
σ = standard deviation of return
n = number of returns in the sample

For value-weighted portfolio returns, we calibrated μ and σ on the 184 January returns for 1802–1986. For equal-weighted portfolio returns, we calibrated μ and σ on the 60 January returns for 1927–1986. In both cases, we set the global alpha (i.e., Bonferroni method of multiple tests) to 0.05. When cumulative January returns rose above the decision cones shown in **Figure 1**, we could reject the no-January-effect hypothesis with 95 percent confidence.

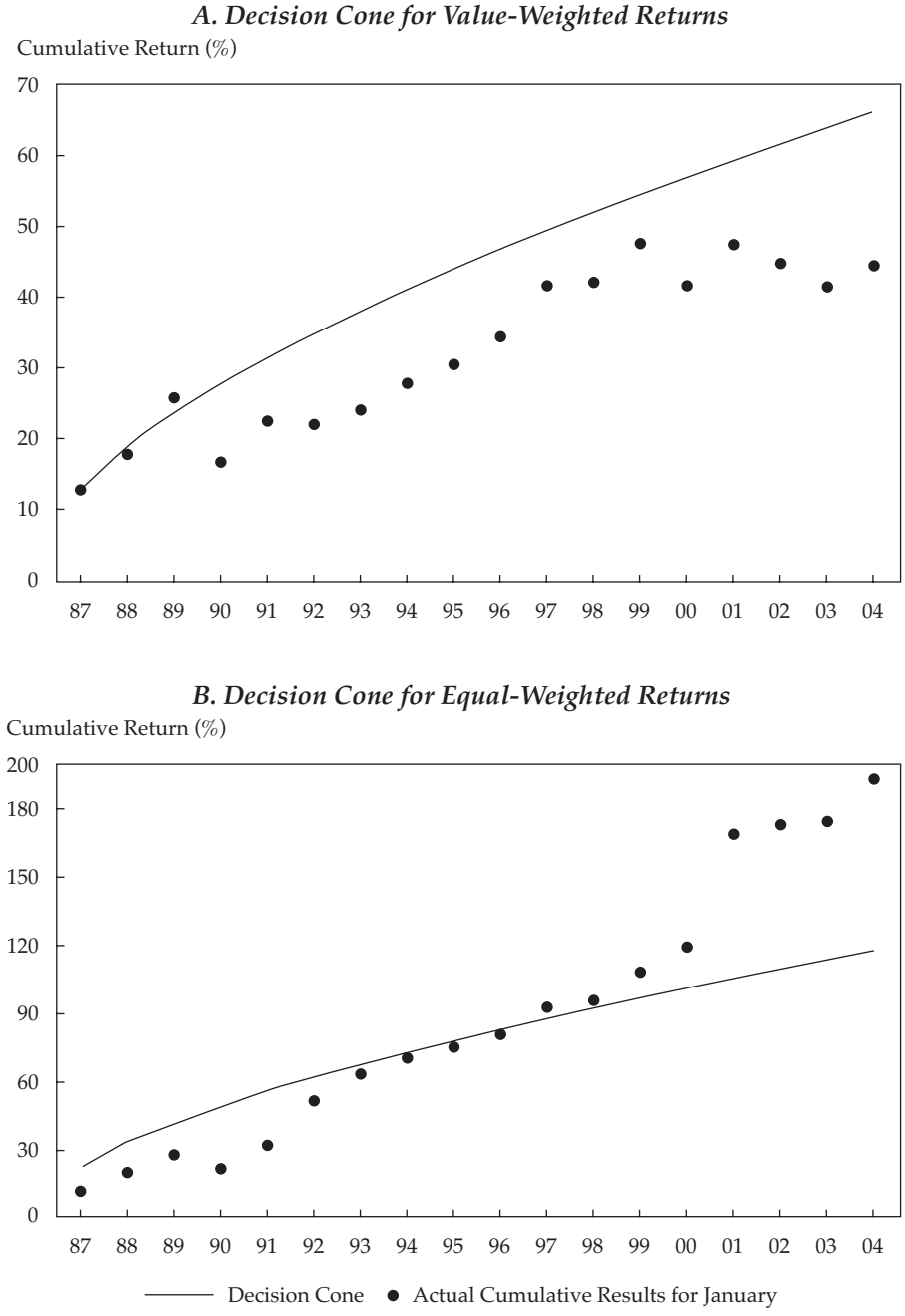
Panel A of Figure 1 shows that the cumulative value-weighted portfolio returns for January tended to fall inside the decision cone. Therefore, independent statistical evidence generated by using value-weighted portfolio returns since the passage of the Tax Reform Act of 1986 suggests that the January effect hypothesis is not tenable. As suggested by Sullivan et al. (2001) and Lakonishok and Smidt (1988), among others, value-weighted portfolio returns provide no robust support for the January effect hypothesis.

Starkly different robust support for the January effect is evident when equal-weighted portfolio returns are considered. As Panel B of Figure 1 shows, the cumulative equal-weighted portfolio returns for January rise far above the decision cone calibrated on equal-weighted monthly returns for 1927–1986. This independent evidence from data since the Tax Reform Act of 1986 allows rejection of the no-January-effect hypothesis with 95 percent confidence. A January effect in equal-weighted returns remains today that is both statistically significant and economically meaningful.

Conclusion

In this study, we updated evidence on the January effect in value-weighted returns for 1802–2004 and in equal-weighted returns for 1927–2004. We found a persistent January effect for small-cap stocks in equal-weighted returns, even during the period following passage of the Tax Reform Act of 1986.

Figure 1. Decision Cones That Permitted Rejection of the No-January-Effect Hypothesis, 1987–2004



We also documented the anomalous pattern of monthly returns for portfolios based on the Fama–French (1993) size and B/M factors and showed that both factors contribute to a continuing January effect. A persistently negative January effect for momentum stocks was also suggested. We conclude that the January effect remains a small-cap phenomenon and has been unaffected by the Tax Reform Act of 1986. The January effect is a real and continuing anomaly in stock market returns and one that defies easy explanation.

Although tax factors have long been offered as a plausible explanation for a January effect in the United States, the continuing presence of a January effect since 1987 appears to weaken that argument. Since passage of the Tax Reform Act of 1986, any seasonal tendencies related to tax-motivated selling by institutional investors should not occur at

calendar year-end. Because many institutions have retained a January–December reporting period despite the new November–October tax period, however, window dressing may have contributed to a small-cap January effect during the 1987–2004 period. Tax-motivated selling by individual investors and the anomalous buying and selling behavior of individual investors at the turn of the year also remain plausible contributing explanations. In any event, more than 30 years after its discovery, the January effect remains a compelling riddle.

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This article qualifies for 1 PD credit.

Notes

1. We offer a necessarily brief review of papers on the January effect. For an extensive review and update on calendar anomalies in general, see Keim and Ziemba (2000).
2. Although DJIA return data do not include dividends, Lakonishok and Smidt (1988) and Schwert (1990) found no evidence of seasonality in DJIA dividend income that would affect inferences drawn about any monthly seasonality in index returns. Keep in mind that DJIA data are from large-cap stocks only. Thus, studies based on DJIA data can offer no insight into the question of whether the January effect might stem from trading by investors in small-cap stocks.
3. Trading costs are often cited as impediments to the full exploitation and elimination of calendar anomalies. With even modest transaction costs, investors may not be able to take full advantage of certain anomalies; relatively large bid–ask spreads and illiquidity compound the problem for small-cap stocks. Nevertheless, if calendar-related anomalies do exist, trades can be scheduled to take advantage of them. Long-term investors would be wise to plan ahead to take advantage of true calendar anomalies in making buy and sell decisions. Even modest calendar effects could be relevant to traders (see Chen and Singal 2003).
4. Schwert's value-weighted returns begin with January 1802, so there is no January return for 1802.
5. Market equity (size) is price times shares outstanding. Price is from CRSP; shares outstanding are from Compustat (if available) or CRSP. Book equity is constructed from Compustat data or collected from the Moody's Investors Service industrial, financial, and utilities manuals. Book equity is the book value of stockholders' equity, plus balance sheet deferred taxes and investment tax credits (if available), minus the book value of preferred stock. Depending on availability, Fama–French use the redemption, liquidation, or par value (in that order) to estimate the book value of preferred stock. Stockholders' equity is the value reported by Moody's or Compustat if it is available. If not, Fama and French measure stockholders' equity as the book value of common equity plus the par value of preferred stock or the book value of assets minus total liabilities (in that order). See Davis, Fama, and French (2000) and Fama and French (1993) for complete details.
6. Szakmary and Kiefer (2004) examined relative returns on cash indices and futures tracking smaller stocks around the turn of the year for a more limited time frame than the 1987–2004 period we consider. They found evidence that the traditional turn-of-the-year effect in both cash and futures is confined to the pre-1993 period.

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