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## On the Predictability of Stock Returns in Real Time\*

In the last 30 years, financial economists have documented ample evidence that both the time series and the cross section of stock returns are predictable.<sup>1</sup> Recently, economists even extended this evidence of predictability to pre-Compustat and foreign stock returns.<sup>2</sup> All this

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1. Basu (1977); Banz (1981); Chen, Roll, and Ross (1986); Keim and Stambaugh (1986); Campbell (1987); Fama and French (1988, 1992, 1993); Poterba and Summers (1988); Lakonishok, Shleifer, and Vishny (1994); Jegadeesh (1990); Jegadeesh and Titman (1993, 1995); Daniel and Titman (1997); and Conrad and Kaul (1998) are just a few.

2. This evidence seems to mitigate, but does not eliminate, the concern that these effects are spurious. See Davis (1994); Davis, Fama, and French (2000); Chan, Hamao, and Lakonishok (1991); Harvey (1991); Bekaert and Hodrick (1992); Rouwenhorst (1998); and Fama and French (1998).

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Researchers have documented an abundance of evidence that stock returns are predictable ex post facto. In this study, we address the ex ante predictability of the cross section of stock returns by investigating whether a real-time investor could have used book-to-market equity, firm size, and one-year lagged returns to generate portfolio profits during the 1974–97 period. We develop variations on common recursive out-of-sample methods and demonstrate a marked difference between ex post and ex ante predictability, suggesting that the current notion of predictability in the literature is exaggerated.

evidence, however, is of ex post predictability. In other words, the patterns in stock returns were discovered with hindsight (full-period information). What we do not know from this evidence is if stock returns are predictable ex ante.

In this paper, we examine whether cross-sectional patterns in stock returns were evident in real time without the benefit of hindsight. We develop a recursive out-of-sample method to assess the ex ante predictability of stock returns using three premier forecasters: book-to-market equity, size, and momentum.<sup>3</sup> We do not contest that an investor *could* have implemented the book-to-market, size, and momentum strategies historically. We address whether an investor *would* have chosen to implement them. Would an investor in real-time have found these market-beating strategies amid the plethora of alternatives, or is the evidence that the market is beatable due only to the clarity of hindsight?

A secondary motivation for our study is to provide a potential resolution to a paradox observed in the literature. The current notion that the stock market is predictable stands in contrast to the well-documented inability of mutual funds to beat the market in real time (Carhart 1997; Wermers 2000). Some argue this inability is an agency cost, whereby managers choose not to fully exploit apparent mispricings (Lakonishok, Shleifer, and Vishny 1994; Del Guercio 1996; Shleifer and Vishny 1997; Chan, Chen, and Lakonishok 2000). We suggest another explanation. Perhaps stock returns are not predictable ex ante. The performance of these funds then would be a result of managers simply being unable to see the book-to-market, size, and momentum effects coming. It is interesting to note that nearly all studies of real-time investment performances also fail to show that the market is clearly beatable. Barber and Odean (2000) find this for individual investors; Christopherson, Ferson, and Glassman (1998) find this for pension funds; Pirinsky (2001) finds this for banks, investment advisors, and insurance companies; Desai and Jain (1995) find this for “superstar” money managers; Metrick (1999) finds this for newsletter recommendations; Barber et. al. (2001) find this for analysts’ consensus recommendations. How easily the book-to-market, size, and momentum patterns could be detected and exploited ex ante is the focus of this study.

We examine whether a real-time investor, with no prior belief in the efficacy of any specific strategy, would have discovered book-to-market, size, and momentum to be useful predictors of stock returns over the July 1974–December 1997 period. We follow Pesaran and Timmermann (1995) and Bossaerts and Hillion (1999) who note that allowing for alternative, competing variables is the crucial element of

3. Fama and French (1992, 1996) show that book-to-market equity and size subsume other predictors of stock returns but not yearly momentum.

proper ex ante out-of-sample testing (model uncertainty). Investors in real time do not know ex ante which variables will and will not be useful in capturing future profits. For our analysis, the investor may employ book-to-market equity, size, lagged annual return (momentum), beta, or a combination of these variables. We include beta in the analysis as an additional competing variable to increase the realism of the research design. Obviously, including just four variables understates the set of potential forecasters confronting the investor. Our conservative depiction of the variable set serves to bias our tests in favor of finding out-of-sample predictability, since three of the four variables are the premier ex post cross-sectional predictor variables. Another distinguishing feature of our procedure is that we force the investor to decide ex ante how to employ the variables to select stocks (parameter uncertainty). For example, should he or she invest in a momentum or a contrarian strategy, or neither? We characterize the potential trading rules as cross-sectional sorts of all stocks based on each of the four variables.<sup>4</sup>

We test for predictability by analyzing whether a simulated real-time portfolio outperforms a passive index. The real-time portfolio is constructed each year by selecting the trading rules that perform best during the prior in-sample period. We examine three classes of real-time simulations, each characterized by the criterion employed to determine which rules are the best. The results indicate that, if an investor used either mean monthly returns or terminal wealth as the criterion, the real-time portfolios were unable to beat the market. If an investor chose the Sharpe ratio as the criterion, the portfolio generated profits. Despite being handed three of the premier ex post variables, a real-time investor would not have easily beaten the market. The results stand in stark contrast to the in-sample results, which identify profits for each of the three simulations. These results are robust to many variations in the out-of-sample method.

More important, even the best-performing real-time portfolio generates profits that are only a fraction of what the literature has discovered ex post. Specifically, the best portfolio we form beats the market by 19 (10) basis points per month on average (after trading costs). Alternatively, a “hindsight” portfolio of stocks with high book-to-market

4. Pastor and Stambaugh (2000) examine the portfolio-allocation decision of a mean-variance optimizing investor in January 1998, who updates personal beliefs with return data in a Bayesian fashion. They find that pricing uncertainty and short-sale constraints leave the investment portfolio nearly identical if the investor employs the Fama-French three-factor model of expected returns or the Daniel-Titman characteristic model. Employing the Capital Asset Pricing Model (CAPM) results in different allocations. Employing no model and relying solely on the prior data (i.e., an agnostic investor) results in a portfolio similar to that chosen under the first two models. Our study simulates the annual portfolio decisions of an agnostic investor from 1974 to 1997.

ratios, low market capitalization, and high lagged annual returns outperforms the market by 52 basis points per month on average.<sup>5</sup>

Our results complement recent examinations of the out-of-sample predictability of the time series of stock returns. Bossaerts and Hillion (1999); Sullivan, Timmermann, and White (1999); and Goyal and Welch (2003) document substantial in-sample predictability in the time series of stock index returns but find no evidence of out-of-sample forecastability. Lo and MacKinlay (1997) find some evidence of market-timing profits from 1967 to 1993. However, Pesaran and Timmermann (1995) conclude that a real-time investor could have profited from timing the stock market only during the 1970s, not in the 1960s or the 1980s. Overall, these studies and ours highlight a marked difference between *ex ante* and *ex post* predictability.

The next section details the data and methodology; section II reports the in-sample results of our method; section III presents the out-of-sample results; section IV summarizes the robustness checks; section V concludes with several potential explanations for the differences in in-sample and out-of-sample results.

## I. Data and Methodology

### A. Independent and Dependent Variables

We use all NYSE, AMEX, and NASDAQ nonfinancial firms listed on the Center for Research in Security Prices (CRSP) monthly stock return files and the Compustat annual industrial files from 1963 through 1997. To mitigate backfilling biases, a firm must be listed on Compustat for 2 years before it is included in the data set (Fama and French 1993). We exclude stocks priced below \$5 to alleviate the microstructure concerns associated with these securities (proportionally high transactions costs and illiquidity; Ball, Kothari, and Shanken, 1995).

In accordance with Fama and French (1992), we form the book-to-market ratio of equity (B/M) by dividing the book value of a firm's equity at fiscal year end  $t - 1$  from Compustat by the market value of equity from CRSP at the end of December of year  $t - 1$ .<sup>6</sup> SIZE is defined as the market value of the firm's equity from CRSP at the end of June of year  $t$ .

Our proxy for a stock's momentum is its 1-year lagged return, which is consistent with the stock return models of Asness (1995) and Carhart

5. This estimate of the profits from hindsight is conservative, since we employ quintile sorts and a \$5 price screen. Depending on how you choose to define value, size, and momentum strategies, hindsight profits can easily exceed the market by 100 basis points per month.

6. The book value of equity is defined as total shareholder's equity plus balance-sheet deferred taxes and investment tax credits minus the book value of preferred stock (valued at the redemption, liquidation, or par value, in that order as available). Firms with negative book values are eliminated.

(1997). Additionally, Jegadeesh and Titman (1993) and Conrad and Kaul (1998) find that momentum strategies are profitable when the conditioning set of lagged returns is defined as any horizon from 3 months to 1 year prior. Furthermore, a 1-year horizon is consistent with our definitions of B/M and SIZE, which are also computed as annual variables. The 1-year-lagged holding-period returns (LAGRET) are calculated from the beginning of July of year  $t - 1$  to the end of May of year  $t$ . June returns in year  $t$  are excluded to mitigate the return biases due to the bid-ask bounce (Fama and French 1996).<sup>7</sup>

Note that the investor in our analysis has some benefit of hindsight because we provide three variables that have been shown to be correlated (ex post) with returns over the sample period (Fama and French 1992, Jegadeesh and Titman 1993). In reality, unless an investor had strong prior beliefs, he or she would not ex ante employ only these specific variables to identify profitable trading rules. A real-time investor faces a much larger set of potential forecasting variables. In a conservative effort to alleviate this structural advantage, we increase the variable set by including beta, which is very likely to have been considered as a forecast variable over the sample period.<sup>8</sup> By including beta, we examine if there is cross-sectional predictability due explicitly to differences in risk. It is unlikely that beta's poor performance in explaining stock returns would have been realized until the end of the sample period.

A beta is assigned to individual stocks in June of year  $t$  and is estimated using no more than 60 months and no less than 24 months of prior returns, employing the CRSP value-weighted index as the market's proxy. We define BETA as the sum of the coefficients in the regression of stock returns on lagged and contemporaneous market returns (Dimson 1979; Fama and French 1992).

While it is obvious that hindsight biases are not completely eliminated from our study, these biases are mitigated. The remaining bias will likely incline the out-of-sample test statistics toward falsely rejecting the null hypothesis of no predictability. Conrad, Cooper, and Kaul (2003) show that using randomly generated variables that "worked" over the entire sample period biases the recursive out-of-sample performances of these variables toward providing evidence of predictability.<sup>9</sup>

7. Roll (1984) shows that the bid-ask spread induces negative autocorrelation in individual stock returns. "Skip-month" annual returns are used to prevent this spurious negative autocorrelation from reducing a momentum effect in LAGRET.

8. Other variables that could have been considered during this time period are P/E ratios, leverage, dividend yield, multifactor betas from factor analysis or Chen et al.'s (1986) 6-month lagged returns and 3-year lagged returns, to name just a few.

9. Specifically, they find that using the five best-performing randomly generated (spurious) variables generates recursive out-of-sample profits of 14 basis points per month on average.

In the next section, we detail the method used to examine the out-of-sample explanatory power of B/M, SIZE, LAGRET, and BETA for the cross section of monthly stock returns from July of year  $t$  to June of year  $t + 1$ .

### *B. Out-of-Sample Methodology*

To assess the forecasting value of B/M, SIZE, LAGRET, and BETA over the 1974–97 period, we develop a recursive out-of-sample procedure to simulate an investor's real-time decision-making process. We first describe the main specifications of the simulation, which are a culmination of the methods and results of the current predictability literature. Then, after reviewing the results of these simulations, we investigate alternatives for each modeling choice we make. These robustness checks are discussed in section IV.

The rule universe is constructed using all possible one-way and two-way independent sorts of the four variables' quintiles. There are a total of 20 quintiles of the four variables and 150 two-way combinations of the quintiles.<sup>10</sup> Therefore, the investor considers 170 rules in each decision period. We exclude individual rules that identify more than one quintile of a particular variable.

The first in-sample (training) period extends from July 1964 to June 1974. We employ a rolling 10-year in-sample window as a reasonable trade-off between reducing error in the estimation of the relations between stock returns and our choice variables and permitting regime switches in those relations.<sup>11</sup> Stocks are sorted into quintiles based on each variable (B/M, SIZE, LAGRET, and BETA) at the end of June of each year  $t$  of the in-sample window. The monthly equally weighted returns for each of the 170 rules are calculated from July of each year  $t$  to June of each year  $t + 1$ .

We follow Pesaran and Timmermann (1995) and Sullivan, Timmermann, and White (1999) and run three separate simulations, each corresponding to a different criterion for identifying the best rules over the in-sample period. The three criteria we employ are the mean monthly return, the terminal wealth, and the Sharpe ratio. Consistent with the prior studies, we do not assert which criterion our investor would prefer a priori; we wish the real-time analysis to be reasonably broad in terms of investor types (i.e., utility functions).

The rules generating the highest (lowest) 10%—17 rules in this case—of mean monthly returns over the entire 10-year in-sample

10. We also examine a much larger universe of 1295 rules obtained by using all possible one-way, two-way, three-way, and four-way sorts. This specification is discussed in section IV.

11. Pesaran and Timmermann (1995); Bossaerts and Hillion (1999); Sullivan, Timmermann, and White (1999); and Goyal and Welch (2003) document nonstationarity in the time-series relations between stock returns and numerous "predictor" variables. Daniel and Titman (1999) chose a 10-year rolling window as well. We also examine an alternative in-sample window that uses all available past data in section IV.

window form the investor's chosen LONG (SHORT) out-of-sample portfolio for July 1974 to June 1975. Similarly, the LONG (SHORT) portfolios for the terminal-wealth and Sharpe-ratio criteria are formed by selecting the rules that generate the highest (lowest) 10% of terminal wealths and Sharpe ratios, respectively, over the in-sample period. For illustration, we provide 3 of the 17 LONG rules under the Sharpe-ratio criterion for the July 1974 to June 1975 out-of-sample period. The first rule indicates to buy all stocks that are in the lowest quintile of SIZE. The second rule indicates to buy all stocks that are both in the lowest quintile of SIZE and in the highest quintile of B/M. The third rule indicates to buy all stocks that are both in the lowest quintile of B/M and in the lowest quintile of BETA. Each of these three rules is 1 of the 17 (out of 170) rules that generated the highest Sharpe ratios during the prior 10-year in-sample period. Stocks selected for out-of-sample investment by more than one rule do not receive increased weighting. We choose the top (bottom) 10% of the rules to ensure that we have reasonably diversified the noise in the relationships between the sort variables and stock returns. On average, the LONG (SHORT) portfolios for each of the three criteria contain over 500 (600) stocks, which is roughly equivalent to investing in a quintile of the available stocks.<sup>12</sup>

Monthly equally weighted returns are calculated for the LONG and SHORT portfolios over this out-of-sample period.<sup>13</sup> At the end of this first out-of-sample period, June 1975, the in-sample window is rolled forward 1 year, and the process is repeated. This procedure produces a time series of monthly out-of-sample LONG and SHORT returns from July 1974 to December 1997. While the investor's strategy is updated annually, the rules do not change dramatically from year to year. One year of new information is added to the previous 9 years each time he updates his strategy. So small weight is given to the latest year's returns.

Note that we allow the entire cross section of stocks to be the investor's real-time opportunity set. We do not limit the rule universe to just the extreme quintiles of the predictor variables. A real-time investor would not likely have assumed a monotonic relationship between stock returns and each of the respective variables, B/M, SIZE, and LAGRET. With no theoretical guidelines, an investor would most likely have relied on the empirical relationships to form his or her beliefs, as we do here.<sup>14</sup> Our investor invests in the extreme portfolios if prior returns

12. For robustness, we also examine the performances of the top (bottom) rule only and the top (bottom) 5% of the rules. These specifications are discussed in section IV.

13. The returns of the SHORT portfolio are constructed from a positive investment in the appropriate stocks; so profitable SHORT portfolio returns are negative.

14. Nevertheless, as a robustness check, we examine in section IV the performances of the strategies selected only from the first and fifth quintiles of each variable, i.e. imposing the prior belief that the all relationships are monotonic.

to such strategies are best. Note that employing a regression-based analysis instead of the sorting procedure would be inconsistent with our assumption of no prior beliefs since linearity is imposed by the regressions.

To evaluate whether the cross section is predictable in real time, the returns of the active LONG and SHORT portfolios are compared to a passive benchmark. Finding that the LONG (SHORT) portfolio outperforms (underperforms) the benchmark is evidence of predictability. Because financial researchers disagree on how best to evaluate portfolio performance, we employ several measures. First, we compare the LONG (SHORT) portfolio's mean return to that of an equally weighted market index (EW index), using a paired *t*-test. The EW index is composed of all stocks in the data set.<sup>15</sup> If the cross section of stock returns is predictable *ex ante*, the LONG (SHORT) portfolio should generate a higher (lower) mean return. The second and third performance measures are risk adjusted. We estimate a Jensen's alpha and a Sharpe ratio respectively for the active portfolios. If the cross section of stock returns is predictable *ex ante*, the Jensen's alpha of the LONG (SHORT) should be positive (negative) and the Sharpe ratio of the LONG (SHORT) should be greater (less) than that of the passive EW index.

Note that we consider only CAPM-based measures of risk-adjusted performance. This restriction is determined by the fact that the most common alternatives, namely, the three-factor model of Fama and French (1993) or the four-factor model of Carhart (1997), utilize the same variables we investigate here. Financial research labels these variables as "anomalous" because the CAPM is unable to explain the associated in-sample return patterns. For this reason, we employ the same measures to determine if the out-of-sample performance of B/M, SIZE, and LAGRET remains anomalous.

Our final test entails an examination of "zero-cost" COMBINED portfolios. The returns of the COMBINED portfolios are obtained by subtracting the returns of the SHORT portfolio from the returns of the LONG portfolio. We evaluate the COMBINED portfolio's performance by comparing its mean monthly return to zero and estimating a Jensen's alpha (the risk-free rate is not subtracted from the COMBINED returns in the Jensen regression). If the COMBINED portfolio enjoys a mean return that is greater than zero or an alpha that is greater than zero, we view this as evidence of predictability.

Due to the practical limitations on an investor's ability to use the proceeds from short sales to fund long positions as well as margin requirements and "haircuts," caution must be used when interpreting

15. We also examine a value-weighted index and the S&P 500 as alternative passive indices. These alternatives are discussed in section IV.

the returns to the COMBINED portfolios.<sup>16</sup> Furthermore, not many investors would be allowed by their brokers to short an entire portfolio of hundreds of stocks, which the SHORT portfolio requires. For these reasons, we place the greatest emphasis in this study on the ability of the LONG portfolio to outperform the EW index.

It may be useful to note here that we consider a variety of alternatives to this procedure in section IV. Changes in the in-sample window length, in the rule universe, and in the number of rules selected for investment do not alter the general performance of the method described in this section.

## II. In-Sample Results

### A. LONG, SHORT, and COMBINED Portfolios' In-Sample Returns

Figures 1 and 2 display the in-sample performances of the LONG and SHORT portfolios under the mean-return criterion. Both the LONG and SHORT portfolios sustain remarkable in-sample performances. Figure 1 shows the spreads between the mean monthly returns of the LONG portfolio and the mean monthly returns of the EW index for each of the rolling 10-year in-sample periods. The time series of the in-sample spreads is quite smooth with the LONG portfolio outperforming the EW index by an average of 0.55% per month. The LONG alpha is 0.56% per month on average and indicates that the LONG easily outperforms the index on a risk-adjusted basis as well.

Figure 2 plots the in-sample performance of the mean-return-criterion SHORT portfolio relative to the EW index. The performance of the SHORT portfolio mirrors the performance of the LONG portfolio. The average in-sample underperformance of the SHORT portfolio relative to the EW index is 0.51% in raw returns with an average alpha of  $-0.38\%$ . Interestingly, the small time-series variations in the market-adjusted returns of the LONG and of the SHORT are nearly identical.

Using the 10% of rules that generate the highest terminal wealths, the LONG portfolio outperforms the EW index by 54 basis points per month on average. Using the rules generating the worst terminal wealth, the SHORT underperforms the index by 49 basis points per month on average. Furthermore, these portfolios generate average alphas of 0.58% and  $-0.41\%$ , respectively. Although not shown, the times-series plots of average in-sample LONG and SHORT market-adjusted returns using the terminal-wealth criterion are nearly identical to Figures 1 and 2.

Employing the Sharpe ratio to select the investment rules results in a slightly less impressive in-sample performance. The average monthly

16. See Alexander (2000) for a detailed discussion and analysis of the regulatory constraints that preclude "zero-cost" strategies.

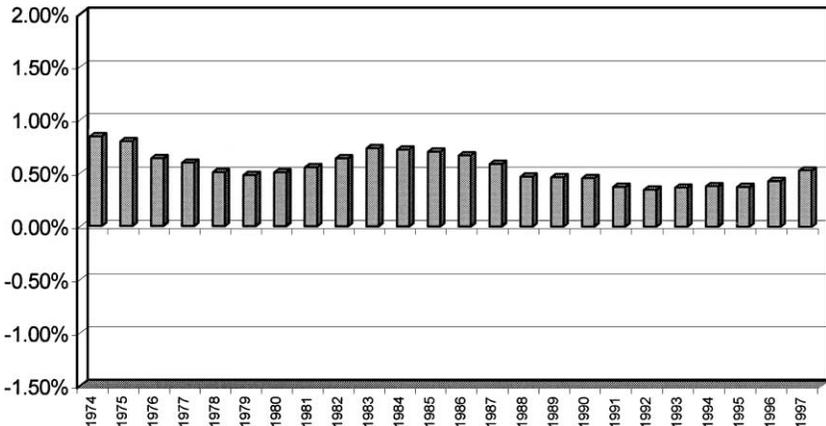


FIG. 1.—Market-adjusted in-sample returns of LONG portfolio under the mean-return criterion. The differences between the mean in-sample monthly returns of the LONG portfolios under the mean-return criterion and the contemporaneous mean monthly return on the EW index are plotted for each rolling 10-year in-sample period. The date indicates the last year of the corresponding in-sample period.

return for the LONG in this case exceeds the average return of the index by 30 basis points, while the return of the SHORT falls below the return on the index by 45 basis points per month on average. Their average alphas are 0.59% and  $-0.41\%$ , respectively. Again, the time-series variations in these in-sample market-adjusted returns is small (figures

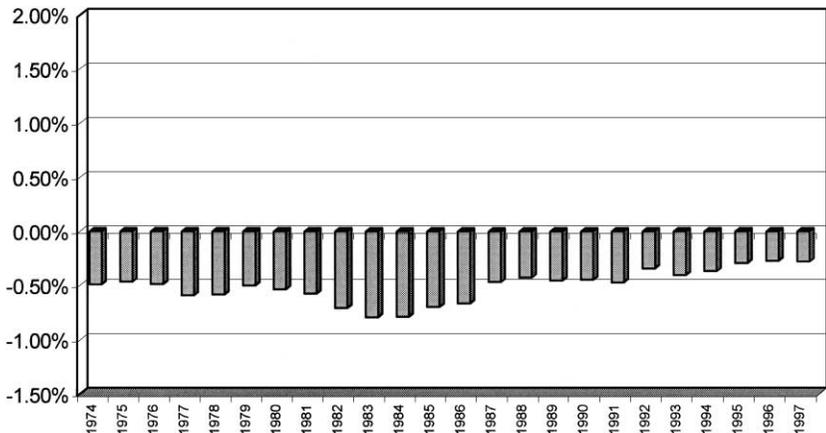


FIG. 2.—Market-adjusted in-sample returns of SHORT portfolio under the mean-return criterion. The differences between the mean in-sample monthly returns of the SHORT portfolios under the mean-return criterion and the contemporaneous mean monthly return on the EW index are plotted for each rolling 10-year in-sample period. The date indicates the last year of the corresponding in-sample period.

not shown), but the pattern of the changes is very similar to that of the previous two simulations, only the magnitude of the spreads is noticeably different.

We draw attention to the level of in-sample returns exhibited by the real-time simulations because they are consistent with the in-sample performances documented for B/M, SIZE, and LAGRET by previous research (Fama and French 1992; Lakonishok, Shleifer, and Vishny 1994; and Jegadeesh and Titman 1993). Second, the persistence of the in-sample returns is quite dramatic. In fact, the minimum market-adjusted in-sample return for the LONG portfolio is 35, 33, and 9 basis points per month for the mean-return, terminal-wealth, and Sharpe-ratio criteria, respectively. Furthermore, these minima all occur in 1992. The only difference apparent so far between the three specifications is the weaker performance of the LONG for the Sharpe-ratio criterion. The examination of the rules selected for the LONG and SHORT portfolios in the next section will reveal why this difference occurs.

### *B. Rule Compositions of LONG and SHORT Portfolios*

For each of the three criteria, two-way rules dominate the investment choices with at least 90% of the selected LONG and SHORT rules being two-way rules. In addition, the LONG and the SHORT portfolios consist of between 558 and 666 stocks on average under each of the three criteria. The EW index has 2646 stocks on average in each year. Most important though is that each of the four variables is employed frequently in the construction of the LONG and SHORT portfolios. Of the total number of investment rules selected (17 rules per year across 24 selection periods) for the portfolios, each of the variables is typically employed in about 40–55% of these rules when using the three in-sample selection criteria.

It is interesting to note how the hypothetical investor uses the four variables. Table 1 provides the mean quintiles of each variable selected for investment in the LONG and SHORT portfolios. For the mean-return and terminal-wealth criteria, the average quintiles selected for B/M and SIZE conform to our hindsight. The LONG (SHORT) portfolio tends to invest in high-B/M (low-B/M) and small-cap (high-cap) stocks. There seems to be no discernable tendency in the LONG or SHORT portfolios under the mean-return and terminal-wealth criteria toward a momentum or BETA strategy, however. For both LAGRET and BETA, the entire spectrum of quintiles is employed as evidenced by the relatively high standard deviations of selected quintiles reported in table 1.

The quintiles selected for investment under the Shape-ratio criterion are noticeably different. Table 1 shows that, while the LONG and the SHORT tend to be composed of high-B/M and low-B/M stocks, respectively, the tendency for the LONG (SHORT) to be in low-cap (high-cap) stocks diminishes. Instead, the selected quintiles under the

TABLE 1 Mean Quintiles Selected for Investment

	Mean Quintiles Selected			
	B/M	SIZE	LAGRET	BETA
<b>A. LONG portfolio</b>				
Mean-return criterion	4.3(1.0)	1.3 (.7)	3.2(1.3)	3.5(1.3)
Terminal-wealth criterion	4.4 (.9)	1.3 (.7)	3.2(1.3)	3.0(1.3)
Sharpe-ratio criterion	4.6 (.7)	2.3(1.3)	2.8 (.9)	1.5 (.6)
<b>B. SHORT Portfolio</b>				
Mean-return criterion	1.4(.6)	4.2 (.8)	2.3(1.3)	3.5(1.3)
Terminal-wealth criterion	1.3(.5)	3.9 (.9)	2.3(1.4)	3.9(1.2)
Sharpe-ratio criterion	1.3(.4)	3.7(1.1)	2.3(1.5)	4.3(1.0)

NOTE.—Using the four variables B/M, SIZE, LAGRET, and BETA, investment portfolios are identified in June of each year from 1974 to 1997. Consider the first in-sample period, which extends from July of 1964 to June of 1974. At the beginning of July of each year 1964 to 1973, all NYSE-, AMEX-, and -NASDAQ-listed stocks are sorted into quintiles based on the four variables separately. Equally weighted returns to each of the resulting 170 one-way and two-way rules are calculated for each month of the 10-year, in-sample period. The rules in the highest (lowest) decile of the respective performance criterion (mean return, terminal wealth, and Sharpe ratio) over the in-sample period identify the stocks selected for investment in the LONG (SHORT) portfolio in the first out-of-sample period from July 1974 to June 1975. The 10-year in-sample window is then rolled forward 1 year and the process is repeated. Panel A reports the mean quintiles selected for the LONG portfolio under each criterion, and Panel B for the SHORT portfolio. Standard deviations of selected quintiles are given in parentheses.

Sharpe-ratio criterion demonstrate a pronounced tendency to invest in a particular BETA style. Particularly interesting is that the LONG portfolio tends to comprise low-BETA stocks and the SHORT high-beta stocks. A momentum effect is not employed under this criterion either.

We see now why the in-sample returns of the mean-criterion and terminal-wealth criterion are so similar; in general, they select the same characteristics for B/M, SIZE, LAGRET, and BETA. The in-sample returns of the Sharpe-ratio criterion are noticeably different than the other two criteria because it selects markedly different BETA characteristics and, to a lesser degree, different SIZE characteristics.

Despite the noted deviations from the ex post best rules, the real-time simulations still can detect a high level of in-sample “predictability” consistent with the prior literature. In the next section, we document the out-of-sample performances of these real-time portfolios to see if the large in-sample returns persist into the next period. In other words, we examine the viability of B/M, SIZE, LAGRET, and BETA as predictors of stock returns in real time.

### III. Out-of-Sample Results

#### A. Mean-Return Criterion

Table 2 presents statistics for the 23.5 out-of-sample years extending from July 1974 to December 1997. Without adjusting for trading costs,

**TABLE 2** Out-of-Sample Performances under the Mean-Return Criterion

	Unadjusted			Adjusted for Trading Costs		
	% Mean Monthly Return (std. dev.)	Jensen's Alpha	Sharpe Ratio	% Mean Monthly Return (std. dev.)	Jensen's Alpha	Sharpe Ratio
EW Market	1.41 (5.40)	N.A.	.15	N.A	N.A	N.A
LONG	1.54*(5.76)	.09	.17	1.41(5.82)	-.05	.14
SHORT	1.38 (5.47)	-.02	.15	1.46(5.47)	.05	.16
COMBINED	.16 (1.93)	.12	N.A.	-.05(1.99)	-.10	N.A.

NOTE.—Using the four variables B/M, SIZE, LAGRET, and BETA, a time series of monthly out-of-sample returns is generated from July 1974 through December 1997. Consider the first in-sample period, which extends from July 1964 to June 1974. At the beginning of July of each year 1964 to 1973, all NYSE-, AMEX-, and NASDAQ-listed stocks are sorted into quintiles based on the four variables separately. Equally weighted returns to each of the resulting 170 one-way and two-way rules are calculated for each month of the 10-year, in-sample period. The rules in the highest (lowest) decile of mean monthly returns over the in-sample period identify the stocks selected for the out-of-sample LONG (SHORT) portfolio in the first out-of-sample period from July 1974 to June 1975. Monthly returns are calculated for the LONG (SHORT) portfolio over the out-of-sample period. The 10-year in-sample window is then rolled forward 1 year, and the process is repeated for each of the 23.5 out-of-sample years (July 1974 to December 1997). The out-of-sample COMBINED portfolio is defined as the LONG minus the SHORT.

The mean equally weighted monthly returns (standard deviation of monthly returns) to the LONG, SHORT, and COMBINED portfolios are reported unadjusted and adjusted for trading costs. The method of estimating the dynamic, stock-specific trading costs is described in section I.C. The LONG (SHORT) portfolio's mean monthly return is compared to the return of the equally weighted (EW) market index. The COMBINED portfolio's mean monthly return is compared to zero. The Sharpe ratios of the LONG and SHORT are compared to the Sharpe ratio of the EW index. The *t*-tests for comparing mean returns and Sharpe ratios are robust to heteroscedasticity and autocorrelation (Gallant 1987). The Jensen's alpha and Sharpe ratio are reported for the LONG and SHORT portfolios; only the Jensen's alpha is reported for the COMBINED.

N.A. indicates that the measure is not applicable.

\*\*\*, \*\*, and \* indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

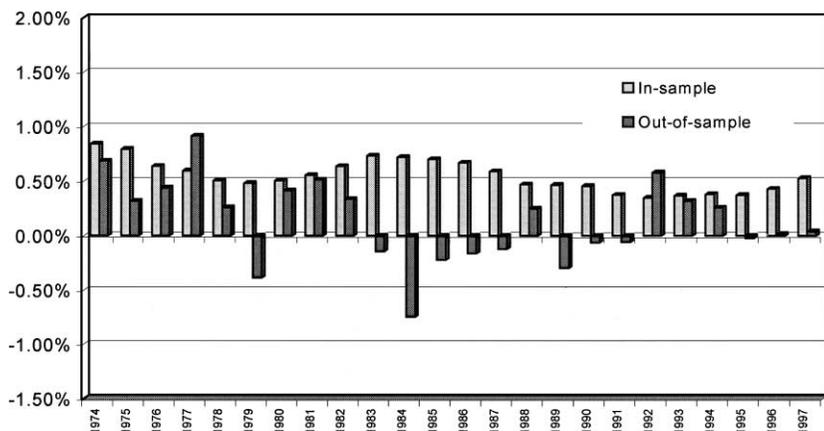


FIG. 3.—Market-adjusted in-sample and out-of-sample returns on the LONG portfolio under the mean-return criterion. The in-sample and corresponding out-of-sample mean monthly market-adjusted returns to the LONG portfolio under the mean-return criterion are plotted for each rolling 10-year in-sample period. The contemporaneous monthly return to the EW index is subtracted from the in-sample and out-of-sample mean monthly returns, respectively. The date indicates the last year of the in-sample period.

the LONG portfolio earns a mean monthly return of 1.54%, exceeding the EW index by 13 basis points, which is a statistically significant difference at the 10% level.<sup>17</sup> However, the Jensen's alpha and the Sharpe ratio of the LONG portfolio fail to detect risk-adjusted abnormal returns. Additionally, the SHORT and COMBINED portfolios do not perform statistically differently than their respective benchmarks on a raw or risk-adjusted return basis.

We adjust the portfolio returns for trading costs using the findings of Keim and Madhavan (1997). These cost estimates are stock specific and a function of the price of the stock, the exchange where the stock trades, and the market value of the stock. The Keim-Madhavan estimates are for 1991–93. We adjust these cost estimates for time-series variations using the results of Stoll (1995). The method for estimating trading costs is detailed in appendix A. Once the returns of these mean-return-criterion portfolios are adjusted for trading costs, there is no evidence in

17. The  $t$ -statistics for the means tests and the Jensen's alpha tests are calculated using the HAC covariance estimator of Gallant (1987). In appendix B, we describe the procedure we employ for selecting the bandwidths, suggested by Andrews (1991). Note that the inferences throughout this study are robust to alternative choices of the bandwidths. These alternatives are also suggested by Andrews (1991) and are described in appendix B as well. The  $t$ -statistics that compare the Sharpe ratio of the LONG or SHORT portfolio to the Sharpe ratio of the EW market portfolio are estimated via the "delta method" (Greene 1997, theorem 4.16, p. 124). For any two portfolios, we estimate the mean and variance of the excess returns as well as the covariance matrix of these four parameters using GMM with the robust HAC covariance estimator. The asymptotic distribution of the difference between the Sharpe ratios of the two series, which is a function of the four parameters, is given in theorem 4.16 of Greene (1997).

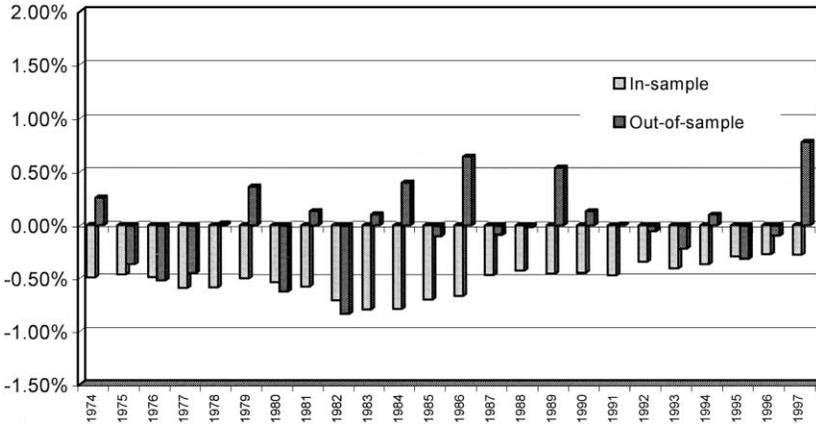


FIG. 4.—Market-adjusted in-sample and out-of-sample returns on the SHORT portfolio under the mean-return criterion. The in-sample and corresponding out-of-sample mean monthly market-adjusted returns to the SHORT portfolios under the mean-return criterion are plotted for each rolling 10-year in-sample period. The contemporaneous monthly return to the EW index is subtracted from the in-sample and out-of-sample mean monthly returns, respectively. The date indicates the last year of the in-sample period.

favor of predictability.<sup>18</sup> An investor employing the mean-return criterion to select investment strategies would not have outperformed the passive index.

Figure 3 plots the out-of-sample versus the in-sample performances of the LONG portfolio under the mean-return criterion. The out-of-sample returns are quite volatile. The LONG performs well from 1974 to 1978 but underperforms the EW index from 1983 to 1991 in all but 1 year. Similarly, figure 4 illustrates the poor and volatile performance of the SHORT portfolio, which never enjoys a sustained period of market underperformance. Nonetheless, figure 5 shows that the COMBINED portfolio generated reasonable returns over the 1974–82 period, based primarily on the strength of the LONG portfolio. During this period, we observe a negative mean monthly return to the COMBINED portfolio only in 1979. After 1983, extreme variability and negative returns mark the out-of-sample performance of the COMBINED portfolio.

*B. Terminal-Wealth Criterion*

Table 3 shows that the terminal-wealth criterion performs better than the mean-return criterion. While the LONG portfolio unadjusted for trading costs beats the EW index by 13 basis points again and at a 10% level of significance, the LONG in this case displays some ability to outperform the index on a risk-adjusted basis. The LONG alpha is 14 basis points

18. On average, the estimated roundtrip cost of the LONG (SHORT) under the mean-return criterion is 265 (180) basis points.

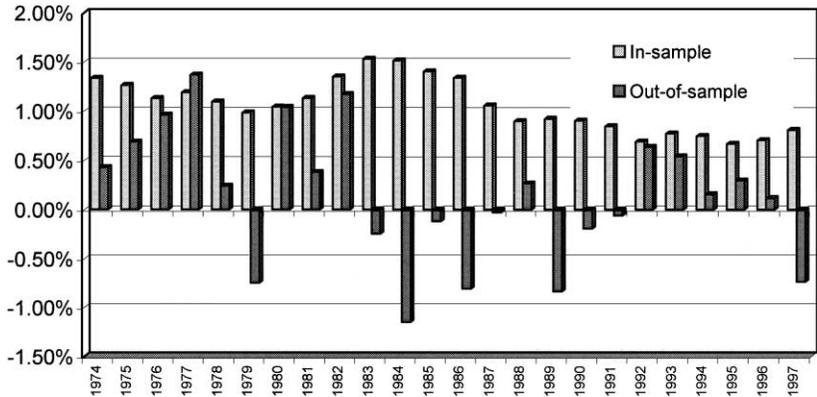


FIG. 5.—In-sample and out-of-sample returns on the COMBINED portfolio under the mean-return criterion. The in-sample and corresponding out-of-sample mean monthly returns to the COMBINED portfolios under the mean-return criterion are plotted for each rolling 10-year in-sample period. The date indicates the last year of the in-sample period.

per month, which is significant at the 10% level, and the Sharpe ratio of the LONG portfolio is 0.18, which is significantly different from the EW index's Sharpe ratio of 0.15. The other difference between this case and the mean-return case is that the COMBINED here has an alpha of 28 basis points that is significant at the 5% level. Even so, all evidence of predictability dissipates when trading costs are considered.<sup>19</sup>

Figure 6 shows that the LONG starts off well but performs erratically after 1979. The SHORT, in figure 7, never enjoys any sustained underperformance. Figure 8 illustrates that the spread between the LONG and the SHORT is positive for the latter half of the 1970s, but it becomes erratic after then.

### C. Sharpe-Ratio Criterion

The performance of the Sharpe-ratio criterion better supports the view that stock returns are predictable. As shown in table 4, the monthly mean return of the LONG without trading costs exceeds the EW index by 19 basis points and generates an alpha of 28 basis points per month on average. The Sharpe ratio of the LONG is 0.21. All of these measures indicate that this specification generates performance superior to the EW index. The mean monthly return of the SHORT without trading costs does not differ from the mean return of the index statistically, but it does underperform the EW index on a risk-adjusted basis according to both the Jensen's alpha and the Sharpe ratio. The Sharpe-ratio criterion also generates a COMBINED portfolio that averages 29 basis points per month with an alpha of 47 basis points, both statistically significant.

19. On average, the estimated roundtrip cost of the LONG (SHORT) under the terminal-wealth criterion is 253 (196) basis points.

**TABLE 3** Out-of-Sample Performances under the Terminal-Wealth Criterion

	Unadjusted			Adjusted for Trading Costs		
	% Mean Monthly Return (std. dev.)	Jensen's Alpha	Sharpe's Ratio	% Mean Monthly Return (std. dev.)	Jensen's Alpha	Sharpe's Ratio
EW market	1.41 (5.40)	N.A.	.15	N.A.	N.A.	N.A.
LONG	1.54*(5.48)	.14*	.18*	1.42(5.33)	.00	.15
SHORT	1.36 (5.77)	-.09	.14	1.44(5.78)	-.01	.15
COMBINED	.18 (1.87)	.23**	N.A.	-.02(1.91)	.02	N.A.

NOTE.—Using the four variables B/M, SIZE, LAGRET, and BETA, a time series of monthly out-of-sample returns is generated from July 1974 through December 1997 as described in table 2, with one exception; the LONG (SHORT) comprises the rules in the top (bottom) decile of terminal wealth in-sample.

N.A. indicates that the measure is not applicable.

\*\*\*, \*\*, and \* indicate significance at the 1%, 5%, and 10% levels, respectively.

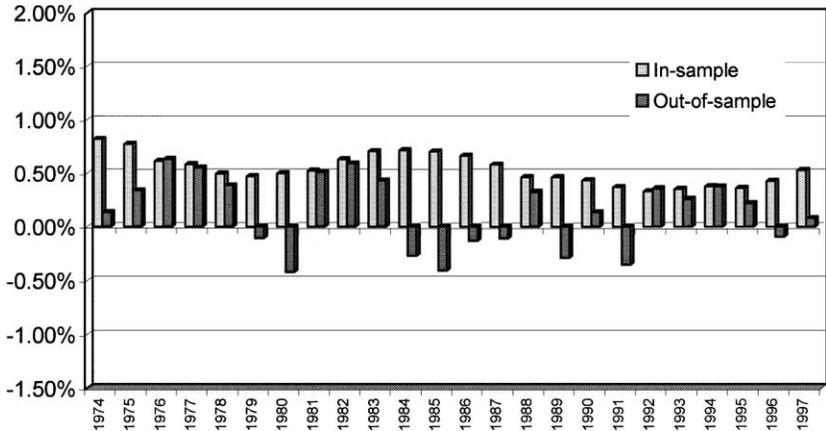


FIG. 6.—Market-Adjusted in-sample and out-of-sample returns on the LONG portfolio under the terminal-wealth criterion. The in-sample and corresponding out-of-sample mean monthly market-adjusted returns to the LONG portfolio under the terminal-wealth criterion are plotted for each rolling 10-year in-sample period. The contemporaneous monthly return to the EW index is subtracted from the in-sample and out-of-sample mean monthly returns, respectively. The date indicates the last year of the in-sample period.

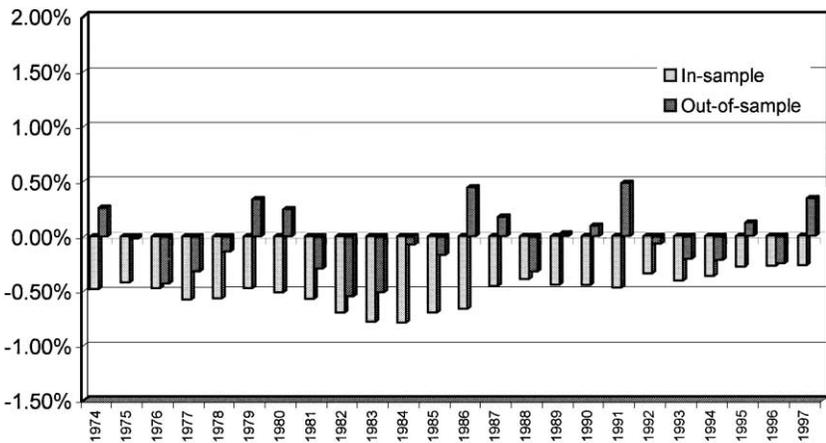


FIG. 7.—Market-adjusted in-sample and out-of-sample returns on the SHORT portfolio under the terminal-wealth criterion. The in-sample and corresponding out-of-sample mean monthly market-adjusted returns to the SHORT portfolios under the terminal-wealth criterion are plotted for each rolling 10-year in-sample period. The contemporaneous monthly return to the EW index is subtracted from the in-sample and out-of-sample mean monthly returns, respectively. The date indicates the last year of the in-sample period.

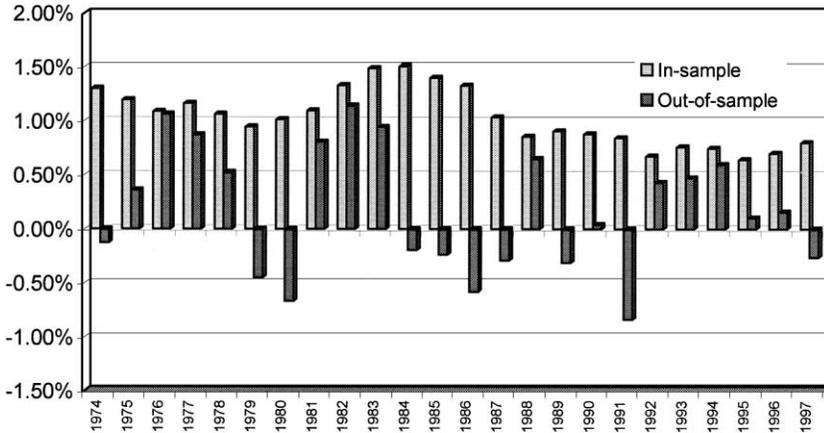


FIG. 8.—In-sample and out-of-sample returns on the COMBINED portfolio under the terminal-wealth criterion. The in-sample and corresponding out-of-sample mean monthly returns to the COMBINED portfolios under the terminal-wealth criterion are plotted for each rolling 10-year in-sample period. The date indicates the last year of the in-sample period.

The most notable point of the Sharpe-ratio case, however, is that the evidence of predictability is not completely eroded by trading costs.<sup>20</sup> In fact, after trading costs are considered, the LONG still statistically outperforms the mean on a risk-adjusted basis with an average monthly alpha of 19 basis points and a significantly higher Sharpe ratio (0.19) than that of the EW index. The COMBINED has a significant alpha of 28 basis points. The raw returns are no longer distinguishable from the benchmarks however. This simulation therefore provides evidence that a Sharpe-ratio investor could have beaten the EW index in real time.

Figure 9 shows that the LONG portfolio constructed using the Sharpe-ratio criterion performs more consistently out-of-sample than the previous two criteria. There is a string of positive market-adjusted returns from 1981 through 1990. However, there is a tremendous below-market return of 89 basis points per month on average in 1991. Figure 10 indicates that the SHORT performs well from 1981 to 1986. The COMBINED, shown in figure 11, performs remarkably well from 1981 through 1990, but it takes extraordinarily large losses in 1980 and 1991.

*D. Subperiod Results*

Although the performances of the first two criteria are poor overall, we do see time-series variation in the performances of their active portfolios, illustrated in figures 4–9. Specifically, this cursory evidence suggests that the mean-return and terminal-wealth simulations perform best in the 1970s. Interestingly, Pesaran and Timmermann (1995) find

20. On average, the estimated roundtrip cost of the LONG (SHORT) under the Sharpe-ratio criterion is 241 (206) basis points.

**TABLE 4** Out-of-Sample Performances under the Sharpe-Ratio Criterion

	Unadjusted			Adjusted for Trading Costs		
	% Mean Monthly Return (std. dev.)	Jensen's Alpha	Sharpe's Ratio	% Mean Monthly Return (std. dev.)	Jensen's Alpha	Sharpe's Ratio
EW market	1.41 (5.40)	N.A.	.15	N.A.	N.A.	N.A.
LONG	1.60**(4.88)	.28***	.21***	1.51(4.91)	.19***	.19***
SHORT	1.31 (6.02)	-.18**	.12**	1.40(6.01)	-.09	.14
COMBINED	.29**(2.19)	.47***	N.A.	0.11(2.21)	.28**	N.A.

NOTE.—Using the four variables B/M, SIZE, LAGRET, and BETA, a time series of monthly out-of-sample returns is generated from July 1974 through December 1997 as described in table 2, with one exception; the LONG (SHORT) comprises the rules in the top (bottom) decile of Sharpe ratios in sample.

N.A. indicates that the measure is not applicable.

\*\*\*, \*\*, and \* indicate significance at the 1%, 5%, and 10% levels, respectively.

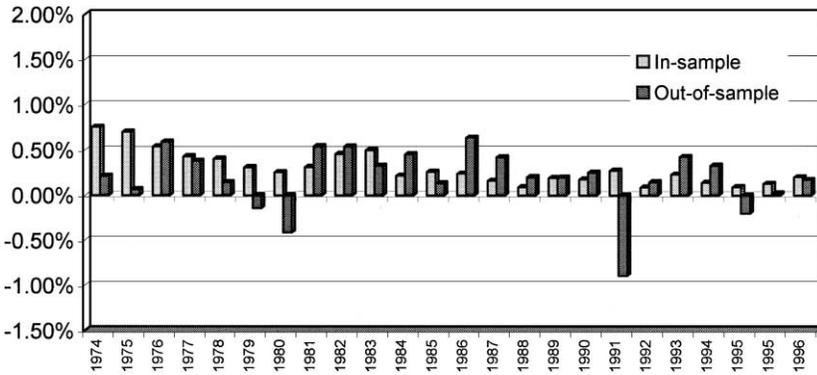


FIG. 9.—Market-adjusted in-sample and out-of-sample returns on the LONG portfolio under the Sharpe-ratio criterion. The in-sample and corresponding out-of-sample mean monthly market-adjusted returns to the LONG portfolio under the Sharpe-ratio criterion are plotted for each rolling 10-year in-sample period. The contemporaneous monthly return to the EW index is subtracted from the in-sample and out-of-sample mean monthly returns, respectively. The date indicates the last year of the in-sample period.

out-of-sample time-series predictability of stock index returns in the 1970s using macroeconomic variables but little evidence of out-of-sample predictability in the 1960s or 1980s. However, we find that, for the mean-return and terminal-wealth criteria, there is no evidence of predictability in the 1970s, 1980s, or 1990s after adjusting for trading costs.

As in the overall period, only the Sharpe-ratio criterion provides trading-cost-adjusted evidence of predictability in the subperiods. An

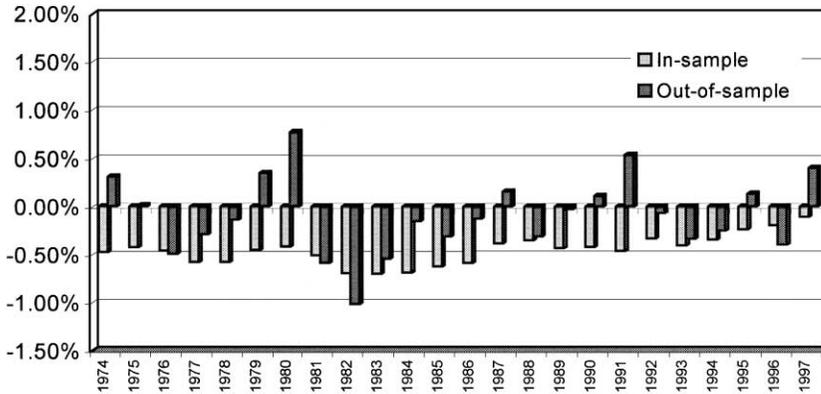


FIG. 10.—Market-adjusted in-sample and out-of-sample returns on the SHORT portfolio under the Sharpe-ratio criterion. The in-sample and corresponding out-of-sample mean monthly market-adjusted returns to the SHORT portfolios under the Sharpe-ratio criterion are plotted for each rolling 10-year in-sample period. The contemporaneous monthly return to the EW index is subtracted from the in-sample and out-of-sample mean monthly returns, respectively. The date indicates the last year of the in-sample period.

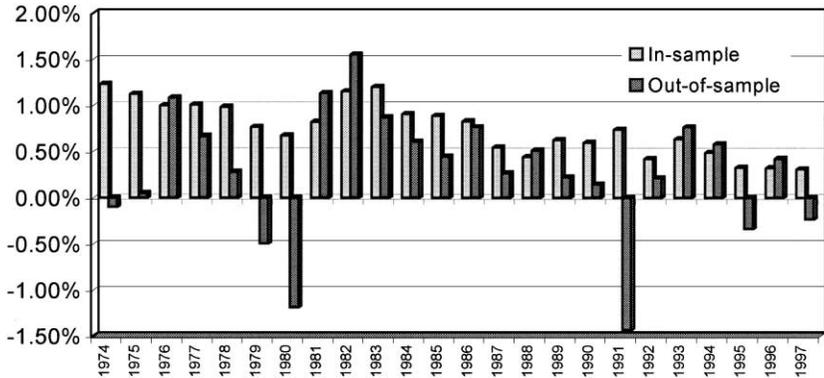


FIG. 11.—Market-adjusted in-sample and out-of-sample returns on the COMBINED portfolio under the Sharpe-ratio criterion. The in-sample and corresponding out-of-sample mean monthly returns to the COMBINED portfolios under the Sharpe-ratio criterion are plotted for each rolling 10-year in-sample period. The date indicates the last year of the in-sample period.

examination of figures 9 through 11 suggests that the Sharpe-ratio case performs well in the 1980s. In fact, even after trading costs are considered, the LONG, SHORT, and COMBINED provide evidence of predictability in the 1980s. In the 1970s and 1990s, however, no evidence is found after adjusting for trading costs. So the profits uncovered in the real-time simulation under the Sharpe-ratio criterion are not persistent; they are concentrated in the 1980s.

### *E. Overview of the Out-Of-Sample Performances*

The most important observation to be made about the out-of-sample performances of these simulations is the dramatic difference between the in-sample and out-of-sample returns. The monthly average return spread between the LONG and the EW index declines from in-sample to out-of-sample by 37% (11 basis points) for the Sharpe-ratio criterion and by 76% for each of the other two criteria (42 basis points for the mean-return criterion and 41 basis points for the terminal-wealth criterion). The erosion in alphas is just as startling. The LONG alpha declines out of sample by 53% (31 basis points) for the Sharpe-ratio criterion, by 70% (39 basis points) for the mean-return criterion and by 76% (44 basis points) for the terminal-wealth criterion. These results suggest that the predictability of stock returns has been vastly overstated in the current literature.

Additionally, the ability of an investor to outperform the index in real time is dubious when using B/M, SIZE, and LAGRET. Of the three criteria applied for selecting the best in-sample rules, only one resulted in abnormal profits after trading costs are considered. Recalling that we

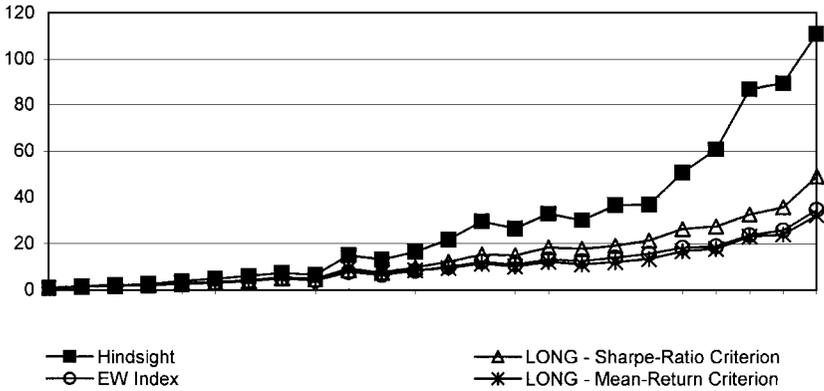


FIG. 12.—Terminal wealth of \$1 invested in each portfolio on July 1, 1974. The terminal wealth values of \$1 invested in each portfolio on July 1, 1974, are plotted through December 1997. The hindsight portfolio comprises all stocks that simultaneously are in the highest quintile of B/M, the lowest quintile of SIZE, and the highest quintile of LAGRET. The LONG portfolios under the mean-return criterion and the Sharpe-ratio criterion are described in tables 2 and 4, respectively, and are adjusted for trading costs. The equally weighted (EW) index comprises all available stocks in our data set.

give the investor three (ex post) premier cross-sectional variables to use makes these results even more striking. We conclude that beating the market in real time is difficult to do.

Perhaps even more interesting is the finding that the best simulation is able to generate profits that are only a fraction of the revenues commonly believed to be attainable with B/M, SIZE, and LAGRET. In other words, all our simulations indicate that the evidence on the predictability of the cross section of stock returns is exaggerated. Figure 12 makes this point emphatically. We plot the terminal wealth generated by a “hindsight” portfolio against the terminal wealth generated by the mean-return criterion and the Sharpe-ratio criterion. The hindsight portfolio comprises all stocks simultaneously in the highest quintile of B/M, the lowest quintile of SIZE, and the highest quintile of LAGRET. As the evidence in section II.B indicates, this portfolio is discovered only through the hindsight of financial researchers.

Figure 12 shows that the hindsight portfolio easily dominates the real-time simulations and the EW index. Like prior literature, we ignore the trading costs of the hindsight portfolio and the EW index. One dollar invested in the hindsight portfolio on July 1, 1974, generates a staggering \$111 by the end of December 1997. One dollar invested in the EW index generates \$35 over the same period. One dollar invested in the mean-return LONG portfolio, incurring trading costs along the way, generates only \$32; \$1 invested in the Sharpe-ratio LONG portfolio, also incurring trading costs along the way,

**TABLE 5**      **Robustness Checks**


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A. In-sample window length
Expanding, all prior years
B. Rule universe
First and fifth quintiles only (32 rules)
Three-way and four-way rules also included (1295 rules)
C. Number of rules selected
Best 5% of the rules
Best rule
D. Trading costs
0.50% one-way
1.00% one-way
E. Passive benchmarks
Value-weighted CRSP index
S&P 500 index

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NOTE.—Listed in this table are the specifications, identified by categories, examined as alternatives to the base-case out-of-sample method employed earlier. Each of these alternatives is employed separately to the base case under each of the three selection criteria (mean return, terminal wealth, and Sharpe ratio).

generates only \$49. It is easy to see that the prior evidence of predictability is exaggerated.

#### IV. Robustness of the Results

To determine if the real-time out-of-sample results are sensitive to the design of the method, we examine several variations in the procedure. Table 5 identifies the alternative specifications we consider. Each of the stated changes is employed separately to the base case described in section I. These alternatives are examined under each of the three selection criteria: mean return, terminal wealth, and Sharpe ratio. We essentially find that our results are robust across a wide variety of alterations in the test design. Specifically, the mean-return and the terminal-wealth criteria perform poorly after including trading costs while the Sharpe-ratio criterion provides evidence of predictability even after trading costs are considered. The best-performing specifications still generate dramatically lower profits than the hindsight portfolio of figure 12. One particular result that should be mentioned is that adjusting the out-of-sample profits for a 1.00% one-way trading cost eliminates all evidence of the LONG portfolio outperforming the market under any criterion.

#### V. Conclusion

Researchers documented an abundance of evidence that stock returns are predictable ex post facto. In this study, we address whether stock returns are predictable ex ante. We ask if a real-time investor could have used book-to-market equity, firm size, and 1-year-lagged returns to forecast returns over the 1974–97 period. Using a recursive

out-of-sample method, we find that the market was difficult to beat despite being given three of four variables that are the ex post premier cross-sectional variables. Moreover, the specification with the highest level of real-time profits falls far short of the in-sample evidence of predictability. The results are robust to variations in the procedure.

The poor out-of-sample performance of book to market, size, and momentum suggests that the notion of predictability currently in the literature is exaggerated. This has several implications for financial economics. First, the strong debate over whether predictability is due to mispricing or risk seems potentially misplaced. At a minimum, the findings soften the debate given that the level of predictability is markedly lower than previously believed. Second, the well-documented performances of real-time investors seem consistent with our findings. This suggests that agency costs in the money management industry might not be as high as some economists recently suggested (Lakonishok, Shleifer, and Vishny 1994; Del Guercio 1996; Shleifer and Vishny 1997; Chan, Chen, and Lakonishok 1999). Last, the cross-sectional out-of-sample results of our study and the times-series out-of-sample results of others (Pesaran and Timmermann 1995; Bossaerts and Hillion 1999; Sullivan, Timmermann, and White 1999; and Goyal and Welch 1999) suggest that financial economists should perhaps be (re)focusing on understanding why the level of predictability in stock returns is so low.

On this last issue, we mention several (non-mutually exclusive) ideas for why the out-of-sample evidence of stock-return predictability is so strikingly different from the in-sample evidence but give no particular credence to any. First, the poor out-of-sample performance we document is consistent with the notion that the book-to-market, size, and momentum effects are spurious. In truth, this particular issue might never be settled, except perhaps after observing the next 50 years of stock returns. This is because a second possibility is that one or more of the variables we examine here may be in fact truly correlated with expected returns. Their explanatory power for the cross section of stock returns, however, may simply be quite limited. To identify these variables and their relationships to stock returns “cleanly” in real time may require many future decades of return data.

These are by no means the only potential interpretations of our results. One alternative explanation is offered by Sullivan, Timmermann, and White (1999). They suggest that opportunities may disappear out of sample because of increased efficiency in the markets over time, such as lower transactions costs and increased liquidity. This does not seem to be the predominant case, however, since we find little evidence of predictability in the 1970s, but we find the strongest evidence in the 1980s (under the Sharpe-ratio criterion). Alternatively, the lack of out-of-sample predictability may be a consequence of learning in the marketplace. That is, the best in-sample investment strategies may not

persist into subsequent periods because the market adjusts to the new information.

Finally, on quite a different note, Lewellen and Shanken (2002) and Bossaerts and Hillion (2001) argue that the Bayesian learning of economic agents can generate ex post predictable patterns that are ex ante rational and therefore not real-time tradable opportunities. In this case, predictability is just an ex post illusion. For example, suppose you know that the time series of stock returns is mean reverting. In real time, you still do not know if stock prices will be higher or lower next period because you do not know the true mean of the distribution. Nonetheless, a pattern of mean reversion is easily detected ex post relative to the sample mean. Disentangling the potential explanations for poor out-of-sample predictability may provide fruitful avenues for future research.

## Appendix A

### Trading Cost Estimates

Since we are interested in quantifying the extent to which stock returns are predictable in real time, we must account for the trading costs that investors incur. This is difficult to estimate since each investor faces different costs for each transaction. Keim and Madhavan (1997) estimate the trading costs for 21 institutions from January 1991 through March 1993. Using 62,333 trades, they find that the average roundtrip total cost of equity trading is 146 basis points. This amount includes commissions paid as well as an estimate of the price impact of the trade. More important, the authors show the tremendous variation that exists in trading costs across institutions, investment styles, trade difficulty, and exchanges.<sup>21</sup>

Keim and Madhavan regress total trading costs on several characteristics of the trade and the traded stock. Like Wermers (2000), we employ the regression results of Keim and Madhavan to estimate trading costs for each stock transaction in our simulation. Since we wish to be conservative in our cost estimates, we assume that our hypothetical investor is a trader of the type that Keim and Madhavan classify as “value” (long-term traders). They find that this group incurs lower trading costs than the “technical” and “index” traders, possibly due to the value traders’ lower demand for immediacy in the execution of their trade orders. We also are conservative in that we set the trade size equal to zero. Not surprising, they find that, as the size of the trade increases, the trading costs increase.

Using the results in table 5 of Keim and Madhavan and setting their technical-trader and index-trader dummies to zero as well as trade size to zero, we obtain our estimates of buyer and seller trading costs

$$\hat{C}_i^{\text{Buy}} = 0.767 + 0.336D^{\text{NASDAQ}} - 0.084\text{Logmcap} + 13.807\left(\frac{1}{P_i}\right) \quad (\text{A.1})$$

21. For example, they find that “value” (long-term) traders average 47 basis points roundtrip while “index” traders and “technical” traders average 108 and 205 basis points, respectively. The average trading costs for the stocks in the smallest (largest) quintile of market capitalization are 383 (56) basis points for NYSE/AMEX and 578 (40) basis points for NASDAQ.

$$\hat{C}_i^{\text{Sell}} = 0.505 + 0.058D^{\text{NASDAQ}} - 0.059\text{Logmcap} + 6.537\left(\frac{1}{P_i}\right) \quad (\text{A.2})$$

where  $\hat{C}_i^{\text{Buy}}$  and  $\hat{C}_i^{\text{Sell}}$  are the estimated total trading costs for stock  $i$  in percent for either a buyer-initiated or seller-initiated order, respectively,  $D^{\text{NASDAQ}}$  is equal to 1 if stock  $i$  is a NASDAQ-traded stock and zero if stock  $i$  is traded on NYSE or AMEX,  $\text{Logmcap}$  is the logarithm of the market value of outstanding stock  $i$  measured in thousands of dollars, and  $P_i$  is the price per share of stock  $i$ . All these variables are obtained from CRSP. For the LONG portfolio, we use  $\hat{C}_i^{\text{Buy}}$  to open the positions in the component stocks and  $\hat{C}_i^{\text{Sell}}$  to close the positions, vice versa for the SHORT portfolio.

Equations (A.1) and (A.2), however, do not reflect the substantial decline in trading costs over the 1974 through 1997 period. Using the revenues from broker and dealer firms to estimate trading costs, Stoll (1995) finds that costs have declined over 40% from 1980 to 1992. Like Wermers (2000), we use the year-by-year results of Stoll to adjust eqq. (A.1) and (A.2) for changes in costs over time.<sup>22</sup> The adjusted costs are

$$A\hat{C}_i^{\text{Buy}} = Y_t^e \hat{C}_i^{\text{Buy}} \quad (\text{A.3})$$

$$A\hat{C}_i^{\text{Sell}} = Y_t^e \hat{C}_i^{\text{Sell}} \quad (\text{A.4})$$

where  $A\hat{C}_i^{\text{Buy}}$  and  $A\hat{C}_i^{\text{Sell}}$  are the adjusted estimates of the respective buyer and seller trading costs for stock  $i$ ,  $Y_t^e$  is the yearly scale factor for year  $t$  and exchange  $e$  (either NYSE/AMEX or NASDAQ), and  $\hat{C}_i^{\text{Buy}}$  and  $\hat{C}_i^{\text{Sell}}$  are from eqq. (A.1) and (A.2), respectively. The yearly scale factor,  $Y_t^e$ , is calculated by dividing the year  $t$  cost estimate of Stoll by the 1992 estimate (Stoll 1995, table 7–4). Stoll separates the yearly costs into exchanges and NASDAQ. The NYSE/AMEX stocks that our simulations select are adjusted using the exchange scale factors, and the NASDAQ stocks using the NASDAQ scale factors. These yearly scale factors are provided in table 6.

Stoll provides cost estimates only for 1980 through 1992. Therefore, we adjust trading costs for each year 1974 through 1979 by employing the appropriate exchange’s 1980 scale factor and adjust each year from 1993 through 1997 by employing the appropriate 1992 scale factor. This likely results in our underestimating the costs in the early years and overestimating the costs in the later years.<sup>23</sup>

It is important to note that the estimates we employ for equity trading costs are those of institutional investors. Recent studies have addressed the trading costs of individual investors. Odean (1999) finds that the average cost for 10,000 individuals at a discount brokerage firm from 1987 through 1993 is 5.93% roundtrip (of which 4.99% is commissions). Barber and Odean (2000) find that the average trading cost of 66,465 individuals at a discount brokerage house from 1991 through 1996 is

22. Wermers does not use our eqq. (A.1) and (A.2) exactly. First, in his cost estimates, he adjusts the intercept to account for the higher costs incurred by index and technical traders; second, he considers trade size, which he observes in his mutual fund data.

23. We contacted the SEC to acquire the data necessary to extend the estimates of Stoll (1995) through 1997, but the data are unavailable.

**TABLE 6**      **Yearly Scale Factors for Trading Cost Estimates**

	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992
Exchanges	2.426	2.225	2.176	1.827	1.673	1.525	1.335	1.106	1.211	1.250	0.986	1.194	1.000
NASDAQ	2.196	2.076	1.856	1.802	1.850	1.685	1.428	1.232	1.386	1.188	1.097	1.029	1.000

4.03% roundtrip (of which 3.03% is from commissions). It is sufficed to say that these cost levels preclude all of our real-time simulations from outperforming the market.

## Appendix B

### Automatic Bandwidth Selection for HAC Estimation of the Covariance Matrix

We calculate standard errors using the covariance estimator suggested by Gallant (1987), which is robust to heteroscedasticity and autocorrelation. To select the bandwidth, we follow the suggestions of Andrews (1991). Let  $y_t = \mathbf{X}_t\theta + \varepsilon_t$ , where  $y_t$  and  $\varepsilon_t$  are scalars,  $\mathbf{X}_t$  and  $\theta$  are  $(k \times 1)$  vectors. We parameterize  $(\varepsilon_t | \mathbf{X}_t)$  as an AR(1) model with no drift term. Let  $(\hat{\phi}_a, \hat{\sigma}_a^2)$  be the least squares estimates of the autoregressive and innovation variance parameters for the AR(1) model of series  $a$ , where  $a$  denotes  $\varepsilon_t$  and each elemental series of  $\mathbf{X}_t$ .

We plug these parameters into the following equation to obtain an estimate of  $\alpha$  for the kernel suggested by Gallant (1987) (eq. [6.4] of Andrews 1991):

$$\hat{\alpha} = \frac{\sum_a w_a \frac{4\hat{\phi}_a^2 \hat{\sigma}_a^2}{(1 - \hat{\phi}_a)^8}}{\sum_a w_a \frac{\hat{\sigma}_a^4}{(1 - \hat{\phi}_a)^8}} \tag{A.5}$$

The value of  $\hat{\alpha}$  from eq. (A.5) is then used to calculate the optimal bandwidth,  $S_T$ , for the kernel suggested by Gallant (1987) (eq. [6.2] of Andrews 1991).

$$\hat{S}_T = 2.6614(\hat{\alpha}T)^{1/5} \tag{A.6}$$

Andrews (1991) shows that  $S_T$  is optimal under a mean-squared-error loss function. However, he suggests estimating the covariance matrix with several alternative bandwidths centered about the optimal bandwidth. To examine the robustness of our results with respect to the bandwidth selection, we follow his recommendation and calculate these alternative bandwidths by replacing  $\hat{\phi}_a$  with its estimated value  $\pm 1$  and  $\pm 2$  standard deviations of its value  $(1/\sqrt{T})$ .

Note that this procedure uses real values for the bandwidths; Gallant (1987) uses integers. Also, in this notation,  $S_T$  equals 1 plus the bandwidth parameter defined by Gallant (1987).

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