Real-Time Profitability of Published Anomalies: An Out-of-Sample Test*

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First draft: September 7, 2007

This draft: March 12, 2014

Abstract

Empirical evidence on the out-of-sample performance of asset-pricing anomalies is mixed so far and arguably is often subject to data-snooping bias. This paper proposes a method that can significantly reduce this bias. Specifically, we consider a long-only strategy that involves only published anomalies and non-forward-looking filters and that each year recursively picks the best past-performer among such anomalies over a given training period. We find that this strategy can outperform the equity market even after transaction costs. Overall, our results suggest that published anomalies persist even after controlling for data-snooping bias.

JEL Classification: G11, G14, D83

Key Words: Data-snooping Bias; Asset-pricing Anomalies; Out-of-sample Test; Published Anomalies

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*We thank Bart Danielson, Xi Dong (MFA discussant), Laura Field, David Haushalter, Jean Helwege, Jeremy Ko, Hong Li, Runze Li, Pankaj Maskara (FMA discussant), Tim Simin, Jun Tu (CICF discussant), Neng Wang, and seminar participants at North Carolina State University, Northern Illinois University, Pennsylvania State University, University of Wisconsin-Milwaukee, the 2007 Financial Management Association meeting (Orlando), the 2008 CRSP Forum (Chicago), the 2008 Wheeler Award for Quantitative and Behavioral Research in Finance (Boston), the 2009 Midwest Finance Association meeting (Chicago), and the 2013 China International Conference in Finance (Shanghai) for their comments. Z. Huang acknowledges a Smeal Dissertation Research Award from the Pennsylvania State University for partial support.

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Electronic copy available at: http://ssrn.com/abstract=1571706

1 Introduction

In the study of asset-pricing anomalies, data snooping may occur when researchers draw inferences from the single realization of a historical time series (e.g., Lo and MacKinlay 1990). One way to guard against data snooping bias is to conduct out-of-sample tests.[1] However, given the large freedom in the research design of such tests, a researcher may unintentionally report time-series predictability that is sensitive to exogenously selected simulation parameters, as noted in Cooper and Gulen (2006). This sensitivity to research design is also reflected in the mixed empirical findings in the real-time out-of-sample test literature. For instance, Breen, Glosten, and Jagannathan (1989), Brennan and Xia (2001), Cooper (1999), Cremers (2002), Solnik (1993), and Xia (2001) find that dynamic asset allocation in real time can beat the market. On the other hand, Cooper, Gutierrez, and Marcum (2005), Goyal and Welch (2003, 2008), Handa and Tiwari (2006), and Pesaran and Timmermann (1995) among others find inconclusive evidence. While the research designs of these papers differ from each other in many aspects, one important factor affecting the out-of-sample performance is the choice of predictive variables used. Data snooping could arise when a researcher chooses which variables to study based on foresight or knowledge of the entire time series.

In this paper we propose a novel method to remove this potential data-snooping bias in out-of-sample tests that is based on published anomalies. Specifically, we simulate a real-time trader/investor who searches for the best trading strategies using *only* published anomalies *prior to* an evaluation time. That is, real-time decisions such as which predictive variables to consider (i.e., to backtest) and to trade with are entirely based on the past information relative to the evaluation point. The investor takes an agnostic view on which predictors to use and progressively picks the best performing anomaly over time out of an expanding universe of anomalies as they become published. And the anomaly universe is determined in

[1] Other methods to assess the likelihood of a data snooping bias include applying statistical model selection criteria (Bossaerts and Hillion 1999), considering the whole universe from which a model is drawn (White 2000), and recalibrating testing statistics when many attempts have been made to explain the same dataset (Harvey, Liu, and Zhu 2013).

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real time by publications in academic journals and some other non-forward-looking filters. We then examine the investor's out-of-sample performance and compare the real-time profit (net of transaction costs) of his strategy to various benchmarks.

To this end, we limit the investor's search (for strategies) to anomalies that 1) are published in five finance journals that are perceived as top ranked in real time, 2) are anomalies about cross-sectional predictability, and 3) can be re-evaluated annually.[2] As such, only those anomalies that have already been published enter into the investor's scope. For example, the strategy of "investing in small firms" is not considered until 1981 when Banz's (1981) study of the size anomaly is first published. This process guarantees that any excess returns earned in the past did not benefit from foresight. Additionally, we focus on long-only strategies in this analysis, rather than long-short ones as typically done in the anomaly literature. This is based on two considerations: First, many investors are subject to short-sale constraints in practice. Second, shorting a decile portfolio including many small stocks may not be feasible in practice due to illiquidity of such stocks and their being more short-sale constrained.

Our main empirical finding is that a real-time long-only investor who trades with the best published anomalies based on backtesting can earn both statistically and economically significant excess returns relative to the equity market. Specifically, if the investor does model selection annually, his average excess return over the CRSP value-weighted index ranges from 4.29% to 10.57% per annum, depending on the training period length used. Furthermore, the outperformance of this active long-only strategy remains under risk-adjusted performance measures such as the CAPM alpha, the Fama-French (1993) three- and the Carhart (1997) four-factor alphas, and the information ratio, even after taking into account transaction costs.

We also provide new evidence on the persistence of anomalies. We find that although an anomaly becomes weaker after it is first published (as documented in the literature), the decay itself tends to be stable during the post-publication period. Also, "old" anomalies

[2] See Section 2.3 for more details about these filters and why they allow us to select anomalies differently from other studies.

published several years ago can perform just as well as "new" anomalies that have just been published, in terms of both absolute and relative measures. As such, old anomalies are as likely as new ones to be included in the active strategy considered in our analysis. Furthermore, the active strategy is found to outperform the average strategy that equally invests in all published anomalies.

This paper contributes to the real-time investment literature in that it is among the first to impose the restriction that a real-time investor could notice an anomaly *if and only if* it has already been published as a way to reduce data snooping bias in out-of-sample tests.[3] This allows us to endogenize the choice of models to only published anomalies among those based on non-forward-looking filters. Existing methods used for selecting predictors tend to focus on either very few candidates or a very large set of candidates.[4] However, using few predictors may be subject to data snooping, as a researcher can choose or not choose a particular anomaly based on foresight (knowledge of which anomalies persist or not); results for the real-time profitability can be rather sensitive to the particular predictor used (Brennan and Xia 2001; Goyal and Welch 2003). On the other hand, considering all possible combinations of predictors may not be feasible for a large number of predictors; even if when it is doable, the resultant model may not be easy to interpret. Additionally, these selections are based on in sample. The procedure proposed here for selecting predictors can significantly reduce the dimension of search space of model specifications and provides an intuitive and easy-to-implement alternative.

Our study also fits in the literature on the persistence of published anomalies. Schwert (2003) finds that while some have persisted after initial publication, majority have attenuated or disappeared either as a result of data snooping or the widespread awareness of these

[3]Granted, some anomalies were known to the public long before their academic publication. We use this restriction simply as a backward-looking filter to reduce the data-snooping bias in selecting anomalies. Intuitively, for an already known anomaly, academic publication serves as an endorsement for reaching a certain level of publicity after a careful scrutiny by the academic research procedure.

[4]For example, Cooper, Gutierrez, and Marcum (2005) and Pesaran and Timmermann (1995) assess 170 and 511 different model specifications, respectively; Cremers (2002) has a combinatorial model space with $2^{14} = 16,384$ models.

anomalies, based on either the CAPM or the Fama-French alpha. Chordia, Subrahmanyam and Tong (2013) document that nine out of 12 anomalies examined show a decrease in their profitability over time due to increases in trading activity, relative to the Fama-French model. McLean and Pontiff (2013) focus on the average of 82 different characteristics and also find that post-publication predictability of this average strategy decreases but not completely goes away, consistent with our findings to certain extent. One difference between these studies and ours is that while we all use academic publication as the start point of the out-of-sample test, we impose an issue-by-issue literature review procedure to make sure that anomaly selection in real-time is not based on knowledge in hindsight. In other words, our purpose is to rule out not only the data-snooping bias, but also the "model-snooping" bias associated with the anomaly selection process in research design. Additionally, the investor in our setting is not required to either stay with a given single anomaly all the time (like in Chordia, Subrahmanyam and Tong, 2013) or evenly spread over all published anomalies to earn the average return (in McLean and Pontiff, 2013)—namely, the investor can learn from past performances of each anomalies and chase the most promising ones in real time.[5] The improved performance of our proposed investment strategy in turn reflects the importance of this learning mechanism. Lastly, in contrast to other papers, we consider a long-only investment strategy and provide evidence of such a strategy's abnormal performance, even though it is known that profits from many anomalies mostly come from the short leg as overpricing is more prevalent than underpricing (e.g. Stambaugh, Yu, and Yuan 2012). Overall, relative the other studies on the publication effect of anomalies, our proposed strategy further reduces possible data-snooping biases, imposes fewer assumptions on anomaly selection, and is more realistic to be implemented by a real-time investor.

Finally, Haugan and Baker (1996) and Lewellen (2013) also conduct out-of-sample tests of a comprehensive list of anomalies but do not take into account the effect of publication of the anomalies.

[5]Han and Zhou (2013) construct a new risk factor that can capture trend chasing behavior on the individual stock level.

The rest of the paper starts with a description of data used in our analysis and anomalies considered in our proposed trading strategy in Section 2. This section also includes details of the strategy and empirical research design. Section 3 reports empirical results from the analysis and conducts robustness checks. Section 4 concludes.

2 Data and Empirical Methodology

2.1 Data

We use data from the Center for Research in Security Prices (CRSP) and Compustat databases, and Kenneth French's data library to construct returns of 14 anomalies, our proposed trading strategy, and benchmarks (for performance analysis).[6]

As mentioned before, our proposed recursive trading strategy is based on a set of 14 cross-sectional stock return anomalies. We construct returns of eight decile portfolios based on respectively debt/equity ratio, growth in sales, return on equity, trading volume, capital investments, accrual, stock issuance, and growth in book asset, using data from the CRSP/Compustat database. Data on monthly returns on decile portfolios of the following six predictive variables: size, book-to-market ratio, momentum, earnings/price ratio, cashflow/price ratio, and dividend yield are from French's data library.

We obtain the Fama-French annual and monthly factors, and the momentum factor also from the above data library. We use annual value-weighted returns of the NYSE/AMEX/NASDAQ index from CRSP as the market benchmark return. We use the one-month T-Bill rates from CRSP as the risk-free rates.

[6]The URL of Kenneth French's data library is `http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data\_library.html`.

2.2 Anomalies Considered

In this subsection we first review stock return anomalies considered in our analysis and then implement each of them using a long-only strategy. Results from the latter exercise can serve as a benchmark when we implement the recursive trading strategy in Section 2.3.

We consider 14 cross-sectional stock return anomalies, each of which is based on one of the following 14 predictive variables: earnings/price ratio (E/P), dividend yield (Div/P), size, debt/equity ratio (D/E), momentum, cashflow/price ratio (CF/P), book-to-market ratio (B/M), growth in sales (GS), return on equity (ROE), trading volume (measured by average turnover ratios), capital investments (CI), accrual, stock issuance (SI), and growth in book asset (GA).[7]

Table 1 summarizes these anomalies and reports their initial publications (column 2) and the earliest year of available data for the implementation of each anomaly (column 3), among other things. Note that the publication shown in column 2 may not be the earliest publication of the corresponding anomaly, due to the filter (used in the anomaly selection procedure) that includes only anomalies published in five top finance journals—an anomaly could have already appeared in another journal at an earlier time. Column 4 describes the long-only trading strategy implemented for each basic anomaly. Column 5 reports the formulas used to calculate each predictive variable when it is necessary; here we strictly follow the procedures described in the original publications to do such calculations as a real-time investor would do.[8]

We now describe how to implement each of these 14 anomalies using a long-only trading strategy. Let x_i denote the predictor for anomaly i and t_{i0} denote the earliest time when

[7] These 14 anomalies are slightly different from those used in the early version of this paper as part of the dissertation work of Huang (2008). We removed calendar anomalies and added more recent cross-sectional anomalies. These changes do not affect major findings of this study.

[8] The accrual anomaly is first documented by Sloan (1996) in a study published in *The Accounting Review*, a top accounting journal but its first appearance in a top finance journal is Chan, Chan, Jegadeesh, and Lakonishok (2006) in the *Journal of Business*. Here we assume by reading the latter study, the real-time investor would be aware of the initial publication in the accounting journal and calculate the accrual following Sloan (1996).

data required for the implementation of anomaly i become available. First, we calculate the value of x_i (e.g. size in the case of the size anomaly) for all single stocks at time $t_i \geq t_{i0}$ as described in the initial publication, using data available up to t_i. Next, we sort stocks in each year into decile portfolios based on the value of x_i in the previous year. During this process, we adopt the following two common practices in the cross-sectional return literature: (a) The value of an accounting variable will not be known until six months after the end of a fiscal year—namely, the accounting variables for fiscal year $t-1$ are used to form portfolios from the July of year t to the June of year $t+1$; (b) The break points for deciles are calculated using only NYSE stocks. Lastly, we obtain monthly value-weighted returns of each decile portfolio. The annualized return for the specific decile (the top or bottom one), net of transaction costs, is considered to be the return for that year for the strategy based on anomaly i.[9]

Table 2 reports the performance of these anomalies before and after their initial publication, based on monthly returns. The performance measures used include the annualized average excess return over the equity market, the Sharpe ratio, and the terminal wealth. For comparison, the strategy of averaging over all anomalies (or only published ones) is also included in the table.

Note first that the four anomalies, Div/P, ROE, Volume, and Accrual, do not outperform the market even before their initial publication (column 3). This is due to two reasons: (i) These anomalies may be implemented differently from what has been done in the original paper as we consider a long-only strategy and implement all anomaly strategies in a single framework. For example, Sloan (1996) finds that stocks with low accrual tend to outperform those of similar size. To convert this finding into a tradable one-factor trading strategy, the investor would drop the size weighting in backtesting and only find out that the outperformance of accrual alone is no longer statistically significant. For the volume strategy (Lee

[9] As a robustness check, we also select anomalies based on the long-short strategy or relative return where the return from such a strategy is the difference between two top and bottom deciles without transaction cost.

and Swaminathan 2000), the result in the original paper is based on a five-year portfolio formation and holding period to match that of the long-term reversal strategy. In addition, the result that stocks with low turnover ratio outperform those with high turnover ratio is for top and bottom momentum deciles only. Nonetheless, we assume that the publication of the volume strategy catches a real-time investor's attention so that the strategy would be included in his back-testing framework. (ii) Unlike many studies of anomaly, we use all the data available in those pre-publication years. For example, Haugen and Baker (1996) find that return on equity (ROE) positively predicts future stock returns from 1979 to 1993 but the result is less significant in the second half of their sample than in the first half. We use a much longer sample period 1966–1996 and find no significant outperformance. In fact, this anomaly has not been studied again in the recent literature, but a real-time investor did not know that and would still backtest the anomaly over time.

Next it is worth noting that excess returns of an anomaly do not necessarily disappear after its initial publication, although most anomalies deliver reduced or less significant excess returns after the publication.[10] Similar results obtain from the average strategies. For instance, the average of published anomalies still outperforms the market by 3.3% per annum, suggesting that published anomalies do not completely go away. With non-published anomalies included, the abnormal return of the average strategy jumps to 4.45%. Taken together, these results suggest a performance decay in these anomalies caused by academic publications. These in-sample results are generally consistent with the existing evidence (see., e.g., the survey article by Schwert 2003). Nonetheless, whether or not a real-time investor can outperform the market using trading strategies based on only published anomalies is still an open question.

[10]For those anomalies published in recent years, there are not big enough samples to reach any statistical significance.

2.3 The Empirical Methodology

In our simulation, we follow the real-time investment literature to implement a recursive model- or anomaly-selection and trading scheme. Importantly, we use the initial publication of an anomaly as the time-point when the anomaly first becomes publicly available to control for data snooping bias; specifically, we construct a benchmark set of (fourteen) anomalies with minimal data-snooping bias through a unique *issue-by-issue literature review process*.

The simulation starts when the first anomaly is published, that is, an investor starts to trade when at least one anomaly is available to him through academic publication. If there is more than one anomaly available, the investor picks the strategy that performs the best over a past *training period* according to a particular *selection criterion*. Otherwise, the investor picks the only one available. Then the investor implements the chosen anomaly and holds his portfolio over the next *holding period*. At the end of each holding period, the investor liquidates his portfolio and repeats the above procedure, albeit based on an enlarging information set that may include more published anomalies. The simulation ends at year 2010, which is the latest year with available data.

The performance of this recursive trading strategy is then compared to that of a market benchmark return (a buy-and-hold strategy) using different *performance measures*. We take into account transaction costs in the analysis. Specifically,*transaction costs* are applied to every trading strategy based on a chosen anomaly, but not the buy-and-hold strategy.

Below we describe each main component of this recursive trading strategy in more details.

2.3.1 The Issue-by-Issue Literature Review Process

As the traditional way of picking which anomalies to study—primarily based on their importance in the recent finance literature—may cause data snooping bias (Cooper and Gulen 2006), we require that a real-time investor select an anomaly based on academic publications before the evaluation time of the anomaly. Specifically, in each year, the investor searches through a predefined subset of finance journals for implementable trading strategies. All asset-pricing anomalies published before a historical time point draw equal attention to the investor, who determines which anomaly to use for the next calendar year simply based on his own backtesting results of published anomalies.

To reduce the number of candidate anomalies to a feasible range without incurring forward-looking bias, we apply three filters to the anomaly selection process: the anomaly type, the publishing journal, and the rebalancing frequency. These filters are not forward-looking in the sense that they do not rely on future performance or future publicity in the process of selecting anomalies. We now describe each of the three filters in detail.

We introduce the first filter in order to limit the choice of anomalies to those cross-sectional return anomalies, as they are relatively easy to backtest. It is known that the implementation of these anomalies involves only data on stock returns and firm characteristics that were widely available when the anomalies were first published.

Regarding the second filter, the investor is assumed to look for anomalies only in the following five major finance journals: the *Journal of Business* (JB), the *Journal of Finance* (JF), the *Journal of Financial Economics* (JFE), the *Journal of Financial and Quantitative Analysis* (JFQA), and the *Review of Financial Studies* (RFS).[11] We look through every paper published in the above finance journals over the period 1972–2008, searching for anomalies that might have caught the attention of a real-time trader. We choose 2008 as the end year in order to leave at least two years of trading time using data available through 2010. We use 1972 as the start point for two reasons: One is that Black's (1972) seminal paper formulating the foundation of the CAPM was published in the same year, after which the real-time investor could have a clear definition of an anomaly. Another reason is that Black,

[11] These five journals have been considered as top finance journals throughout the years of our simulation so the real-time perception of the quality of these journals is not a concern. While we cannot cover all journals to mimic the true opportunity set of a real-time investor, we extend the analysis by including the references in the papers chosen from the above five journals, regardless of where those references were originally published. In other words, we assume that if a paper in one of the five journals is chosen, the real-time investor reads it carefully enough to include all of its cited papers. This alternative assumption only results a slight difference in the availability of anomalies so that one anomaly (the book-to-market ratio predictor) is known to the real-time investor much earlier. The real-time investor's performance under this alternative assumption is therefore slightly better.

Jensen, and Scholes (1972) publish the first empirical test of the CAPM in the same year.

The third filter, used to reduce the small sample bias, excludes those long-term return anomalies that require a holding period of longer than one year to generate excess returns. Also, to avoid high transaction costs associated with frequent portfolio rebalancing, we exclude short-horizon anomalies with rebalancing frequency less than one year. For example, we do not consider the short-term and the long-term stock price reversals documented in DeBondt and Thaler (1985) that require a holding period of one month and three years, respectively. As a result, the investor is assumed to rebalance only once a year in our analysis.

It is worth emphasizing that our choosing anomalies based on the issue-by-issue search of academic journals differs from the traditional way of selecting anomalies based on literature review. In particular, the former is based on the following three considerations: First, publications outside major finance journals which become famous later are not considered in real time, because the investor does not know which papers later would become famous and which would not. One such example is the earliest reference about the book-to-market ratio return predictability. Stattman (1980) first reports the book-to-market ratio anomaly in *The Chicago MBA: A Journal of Selected Papers*, an obscure journal unknown to most academics. However, unlike academics who benefit in hindsight from the growing literature on asset-pricing anomalies, the simulated investor (in this study with a limited vision of only five top journals) would not know about the anomaly until it was later mentioned by Chan, Hamao, and Lakonishok (1991) in the *Journal of Finance*. Second, top journal publications that later receive less attention in the anomaly literature are given full consideration in real time. One example is the debt/equity ratio predictability by Bhandari (1988). Although leverage ratio is not widely believed to be a predictor of equity returns, the real-time investor would seriously consider this anomaly because it is published in a top finance journal. Third, an anomaly becomes known at the time of its first publication, not the time of its most famous publication, if both papers are published in top journals. For example, the momentum strategy would be known in 1990 by the publication of Jegadeesh (1990), rather than in three years later by Jegadeesh and Titman (1993), although the latter eventually receives more citations.

Overall, the proposed issue-by-issue search process allows us to identify sets of anomalies faced by a real-time investor over time. In another word, this search process ensures that anomalies are chosen because they caught an investor's attention in real time, regardless whether or not they are still famous nowadays.

2.3.2 Training and Holding Periods

We use the training period (or portfolio formation period) with a rolling window of the past one year to ten years. For the holding period, we consider the one-year case only. This leaves about 30 years for simulation given our assumption that the long-only investor begins trading only after those anomalies being published in the 1970s or 1980s. As such, assuming a one-year holding period and annual portfolio rebalancing is an efficient way to fully utilize the available data.

2.3.3 Strategy Selection Criteria

In this study, we use the past excess return as the primary strategy selection criterion, a criterion consistent with the long-only strategy that is implemented by our real-time investor (see Section 2.3.6). An alternative criterion considered is based on the long-short relative return. We do not use any statistical criteria such as those in Bossaerts and Hillion (1999) for model selection.

To be consistent with our assumption that the real-time investor learns about anomalies based on academic publications, the investor is to treat each anomaly as a "black box" without really making judgment based on its structural characteristics. This assumption eliminates the possibility that the investor will evaluate an anomaly based on its internal structure, such as the number of predictive parameters in the model. Instead, the investor relies purely on real-time backtesting results to make investment decisions.

While the real-time investor is assumed to select the best past performer in the backtesting in the baseline case, we also consider the scenario where the worst strategy is selected in the sense of Shleifer and Vishny (1997) when past worst-performer can actually perform better in the future due to the widened mispricing. Untabulated results indicate that the active strategy underperforms the market benchmark under all training lengths considered in the analysis. If anomalies are indeed caused by mispricing, it is safer to bet on a further widening than a narrowing gap.

Another assumption is that the investor believes in active trading so that even if none of the anomaly strategies outperform the market during a particular training period, he will still prefer the one with the least under-performance rather than indexing for the next year. For example, the investor would find that no anomaly beats the market during the past one year at the beginning of 1991. As a robustness check, we redo the analysis by relaxing this assumption and find that the performances under different training lengths are almost the same if the investor ever decides to invest passively in some years.

2.3.4 Transaction Costs

Estimates for various anomalies over time are not available in the literature. We consider first a base-level transaction cost (the same for every anomaly); specifically, we follow Pesaran and Timmermann (1995) and assume a 1% transaction cost on a round-trip transaction between risk-free T-Bills and stock shares. Three other levels of this cost, 0.5%, 1.5%, and 2%, are also considered in the sensitivity analysis.

We then adjust the base level separately for different anomalies using two alternative methods: (i) We customize transaction costs across different trading strategies. Two anomalies considered, the size and the volume anomalies, involve small or illiquid stocks; trading with small stocks requires a higher transaction cost (Stoll and Whaley, 1983; Bhardwaj and Brooks, 1992). We adjust the base-level transaction cost of these two anomalies to 5%. (ii)

We apply the real-world transaction cost data from Stoll (1993) and French (2008). French (2008, Table V) documents that the one-way transaction cost decreases over time from 146 basis points in 1980 to only 11 basis points in 2006. We extrapolate the data linearly for years 1977 to 1979, and 2007 through 2010.

2.3.5 Performance Measures

The set of performance measures that we use includes the average excess return over the equity benchmark return, the CAPM alpha, and the Fama-French (1993) three-factor alpha. We also consider the Carhart (1997) four-factor model for completeness, although it is usually not used as a benchmark in the anomaly literature due to the nature of the momentum factor itself.

In the implementation of the above factor models, we consider both constant and time-varying risk factor loadings (betas). Constant beta is more suitable when investors treat the whole strategy as a black box delegated to professional traders, so the risks of the specific anomalies chosen over time is not important or even visible. In contrast, time-varying betas can help capture the risk factor loadings of the different anomalies used in real time, assuming investors having full knowledge of the details of each anomaly. To estimate alpha in a given year in this case, we use time-varying betas obtained using monthly return data from each anomaly over the past five years.[12]

We also consider the information ratio (Treynor and Black 1973) of the simulated return relative to the market benchmark return. This ratio measures the active return per unit of tracking error of the real-time investor and is given as follows:

$$\text{Info Ratio} = \frac{Mean(R_p - R_m)}{Std(R_p - R_m)} \tag{1}$$

Here vectors R_p and R_m represent strategy returns and market returns, respectively.

[12] We also estimate time-varying betas using past three years or one year data. The results are similar with slightly more decay in the significance of alphas for the three- and four-factor models.

2.3.6 Long-Short Strategies

As mentioned before, we focus on long-only strategies in the analysis. The main reason is that many real world market participants such as traditional mutual funds are restricted from leverage and short selling. Also, shorting a decile portfolio that includes many small stocks may not be feasible in practice due to illiquidity of such stocks.

However, implementing a long-only strategy may raise concern of data snooping because many anomalies, such as the momentum strategy, involve forming a "zero-investment" portfolio by taking both long and short positions. To address this concern, we allow the real-time investor to backtest strategies based on their long-short relative returns in robustness checks. Nonetheless, given our main empirical finding that an investor could beat the market in real time, not allowing any leverage or short selling is in fact a stronger assumption, because short-selling constraints reduce portfolio performances (see, e.g., Alexander 2000).

3 Empirical Results

In this section we implement the proposed recursive long-only strategy in a real time fashion and reports empirical results for the (out-of-sample) performance of this strategy. Specifically, Section 3.1 presents the results for different training lengths. Section 3.2 examines the impact of transaction costs on the performance of the proposed strategy. Section 3.3 considers the case where only a random subset of anomalies is included, in order to examine the sensitivity to the number of anomalies available initially. Section 3.4 conducts additional robustness checks, followed by a discussion on the anomaly profitability after being published in Section 3.5.

Performance measures considered include the excess return over the benchmark return, alpha with constant or time-varying betas, and the information ratio. When either the excess return or alpha is used as a performance measure, we calculate statistical significance based on a standard t test. In the case where the information ratio is used, we calculate their significance numerically by bootstrapping. Unless stated otherwise, transaction costs used in the analysis are based on a 1% round-trip transaction cost as described in Section 2.3.4.

3.1 Performance with Different Training Lengths

Consider the base case where the investor adopts a recursive long-only strategy that, at the end of each holding period, selects the best one among published anomalies based on their past performance over a certain training period.

Table 3 reports results for 10 different training lengths that range from one to ten years. As we can see, in all scenarios in Table 3 the real-time investor who chases the performance of published anomalies beats the market as indicated by the positive values of all measures in all cases. The average annual excess return over the buy-and-hold return of a market benchmark ranges from 4.29% to 10.57% and is statistically significant. If the real-time investor happened to choose two years to six years as the training length, the performance would be even better. The simulation from year 1977 also generates a positive information ratio relative to the market benchmark return in all cases. In contrast, Table 2 indicates that a simple average of all published anomalies only generates a 3.3% average annual excess return over the market, lower than any of the cases in Table 3. Also, the information ratio of this strategy is 0.36, lower than 9 out of 10 cases. As such, results reported in Table 3 show that investors can benefit from backtesting and performance chasing in real time, given that not all anomalies are equal.

Results from alphas with fixed betas are consistent with those from other performance measures. Note that when the strategy betas can vary over time, the CAPM alpha increases, but the statistical significance in the three- and four-factor models drops dramatically. This indicates that a big portion of the performance comes from non-market risks. Nonetheless, all alphas in time-varying beta cases are still positive.

Figure 1 plots the terminal wealth over time for both the active strategy (for four different training lengths) and the average strategy (investing equally in all published anomalies).

Starting from $1 in 1977, an investor could have reached in 2010 from $100 to about $600 by actively backtesting and trading with published anomalies only, net of transaction cost. In contrast, indexing in a market portfolio (with no transaction cost) only generates $35.77, slightly off from its peak in 2007. Also, as shown in the figure, the 2- and 5-year training lengths seem to help identify profitable anomalies in real time better than the other lengths do. Lastly, note that the active strategy generates a terminal wealth higher than that of the simple average strategy ($72.73), regardless of the training period length used.

3.2 Performance under Different Transaction Costs

The finding that a real-time investor can beat the market by considering only published anomalies is based on several assumptions outlined in Section 2.3. One of them is the 1% transaction cost on a round-trip turnover between stocks and T-Bills. In this subsection we test the performance sensitivity to different schemes of transaction cost.

Table 4 presents results for different transaction cost levels as well as for training lengths of 2-, 5-, and 10-years in Panels A, B, and C, respectively. We consider five fixed transaction cost levels ranging from 0% to 2%, and a customized transaction cost level in which a 5% transaction cost is imposed on the size anomaly and the volume effect (because these strategies incur transactions with small or illiquid stocks). Another alternative measure used is the real world annual transaction cost data from French (2008) that are estimated from the aggregated stock brokerage revenue and the market-wide total trading volume. We report only alphas with constant beta. Results with time-varying betas are qualitatively similar.

Note from the table that as expected, the investor's performance is affected negatively by transaction cost. However, the main conclusion of this paper—a positive and significant excess return and alpha—still holds in most of the cases, including the case of real transaction cost data being used. In fact, higher transaction cost barely affects the significance of the results if the investor has a training length longer than one year. Untabulated results show that the break even transaction cost is as high as 7% to 10% for the strategy to under-perform the market benchmark return.

3.3 Performance with a Subset of Anomalies

As mentioned earlier, the way to remove data-snooping bias is to closely mimic the true anomaly universe perceived by an investor in real time. Nonetheless, we still have to add several filters in order to reduce the number of anomalies to be included to an implementable level. Although the three filters introduced and described in Section 2.3.1 are designed not to be forward-looking, the real-time investor may know about only a subsample of the 14 anomalies implemented in this research. For instance, in practice an investor may focus only on a limited number of anomalies based on his expertise, rather than all anomalies published in academic journals. To address this issue, we shrink our set of anomalies to a random subset of the 14 anomalies. Two questions arise: First, can the real-time investor still beat the market if not all anomalies are considered? Second, is considering more anomalies always good?

We repeat the simulation in Section 3.1 for cases when the investor only knows a subset of the original 14-anomaly universe. The training length used is two years.[13] This gives us a total of $2^{14} - 1 = 16,383$ different simulations. Table 5 reports simulation results sorted by the number of anomalies considered in the reduced anomaly universe. Based on different performance measures, the real-time investor outperforms the market even when only a subset of anomalies are ever considered. In addition, the performance increases monotonically as more anomalies are considered. Overall, results shown in Table 5 provide evidence on the benefits of including more anomalies and trading with real-time model selection.[14]

This implication echoes with the recent literature on inattention (Barber and Odean, 2008; Duffie, 2010; Nieuwerburgh and Veldkamp, 2010; Peng and Xiong, 2006; and Peng, 2005), to some extent. One possible reason for the performance persistence in published

[13]Untabulated results using 5-year and 10-year training lengths show exactly the same conclusion.
[14]Similar results are found in Huang (2008) when two calendar anomalies are also included.

anomalies is that investors do not explore all the anomalies continuously, perhaps due to limited attention and/or slow-moving capital (Duffie, 2010). Namely, investors do not always pay attention to all investment opportunities from every published anomaly and, as a result, they focus on a small subset of the anomalies or a few anomalies currently undertaken or under consideration because of the search and deployment costs for new anomalies. In another word, it is unlikely that investors and/or fund managers back test all anomalies all the time as we do in this paper. Our result in Table 5 shows that as an investor devotes more attention as proxied by the number of anomalies considered, his performance increases.

3.4 Additional Robustness Checks

Although the proposed trading strategy allows us to remove the data-snooping bias arising from the choice of anomalies, there is still some freedom left in the simulation design that may potentially cause data snooping. In this subsection we conduct further robustness checks to address this issue.

We first examine the sensitivity of our main findings to data lengths used in the analysis. To proceed, we repeat the whole set of analyses at points of time in 2006, 2001, and 1996, and see if the main results still hold. We use 2006 because it is the cutoff time when the first draft of this paper was written. We then trace five years and ten years back from that point of time. We expect the results to remain qualitatively the same but with less statistical significance in these simulations because of the reduced sample size.

Next, we want to control for the lag time between when a paper becomes forthcoming and when it actually appears in a journal, because a real-time investor could learn about an anomaly from a forthcoming paper. To proceed, we repeat the whole set of analyses with anomalies available two years before each of the publication years. In this case, the simulation should generate better performance because the real-time investor could take advantage of anomalies at an earlier time. In addition, a test postponing the availability of anomalies two years after publication is also conducted here. This case corresponds to the possibility of slow information diffusion after academic publication. Anomalies may not always be exploited immediately after they become known to the public.

We then examine if our main result is purely driven by the best anomaly. In fact, during the 21 years since the momentum strategy is published, it has been selected by the real-time investor in 6 to 12 years under different training lengths. Thus we repeat the analysis excluding the momentum anomaly. We expect the main result to hold qualitatively, because as we can see, excess returns in Table 3 are mostly higher than the post-publication performance generated by the momentum strategy alone in Table 2.

Lastly, we switch the strategy selection criterion from past excess returns to past long-short relative returns, since many anomalies were originally published as abnormal return differences between the top and bottom deciles.

To implement the aforementioned considerations, we redo the exercise done in Section 3.1 for seven additional scenarios: with data ending in 2006, with data ending in 2001, with data ending in 1996, with each publication year minus or plus two, without the momentum strategy, and using long-short strategy selection criterion. Due to space limit, we do not present these tables. As expected, we find that although the 1996 result is still positive and significant under the 2-year to 5-year training lengths, it is worse than the 2001 result. And the 2001 result is worse than that with data ending in 2010. Interestingly, the simulation ending in 2006 generates slightly better results than the one ending in 2010. If we make anomalies available two years earlier, the result is stronger. Also as expected, delaying availability two years after publication makes the result slightly weaker.

The case without the momentum strategy is also interesting. All previously significant results are still significant, and the excess returns are mostly similar or higher than the post-publication performance of the momentum strategy (6.26% annually in Table 2). In other words, the investor can beat the best anomaly using other anomalies. This shows that the abnormal performance of the active anomaly strategy documented in this study is not driven by the best anomaly. Instead, it is caused by the performance persistence of different published anomalies. In the last test of the long-short strategy selection criterion, the performance is slightly worse than that under the long-only criterion but remains qualitatively the same. Interestingly, switching calendar back to 1992 (when only half of the strategies were published) obtains the same result that the long-only selection criterion is slightly better. Therefore, even the real-time investor is open minded about which strategy selection criterion to use, it would not take him long to figure out that the same long-only strategy he is using turns out to be a better strategy selection criterion as well.

3.5 Discussions and Implications

In this subsection we examine two aspects of the anomaly choices made by the long-only investor in the process of real-time backtesting and trading. First, we want to investigate what anomalies are more frequently chosen by the active strategy. As anomalies become available in different publication years, the widely used "inclusion frequency" measure (e.g., see Pesaran and Timmermann (1995) and Cremers (2002)) may not fully capture the relative effectiveness of each anomaly in real time. As such, we introduce "relative popularity" (RP) measures that reflect an anomaly's performance rank relative to other contemporaneous anomalies. Also, we look at the most "popular" anomalies to see if they are the same ones we find in-sample and if those anomalies dominate the overall results.

Second, using the relative popularity measures, we investigate whether or not "old" anomalies published several years ago are less likely to be picked compared to "new" anomalies that have just been published. In other words, we are interested in if there is any time trend in the attractiveness of anomalies after publication, in terms of either absolute profitability measured by excess return or performance relative to other published anomalies. If asset-pricing anomalies decrease in profitability after publication over time, a simulated real-time trader, having successfully exploited published anomalies in the past, may find them unprofitable in the future. Schwert (2003) focuses on the in-sample performance to show that asset-pricing anomalies generally disappear or attenuate after their initial publication.

The question being addressed here is therefore whether this post-publication performance drop happens immediately or gradually over time.

3.5.1 Which Anomalies Are More Frequently Chosen?

Fixing other parameters, a proper measure of the competitiveness of an anomaly in the scope of a real-time investor should satisfy the following three properties: 1) it increases as the number of years being selected increases; 2) it decreases as the anomaly's total number of available years increases; and 3) it increases as the number of other anomalies in the chosen years increases. Namely, a good measure rewards those anomalies that are newly published, often picked, and picked among many other anomalies. We measure an anomaly's relative popularity based on its normalized rank among all published anomalies over $[t_1, t_2]$:

$$\text{Relative Popularity}(t_1, t_2) = \frac{\text{number of anomalies outperformed}}{\text{number of anomalies that could have outperformed}} \tag{2}$$

Specifically, we consider two types of relative popularity: The first one (RP1) only counts first place winners in each year and the second one (RP2) assigns points to all published anomalies based on their ranks even when the anomaly is not chosen. For example, in 1981, only three anomalies had been published. Based on their performances in the previous year (1980), the three anomalies are ranked as follows: the size anomaly (the best), the earning/price ratio anomaly, and the dividend yield anomaly (the worst). A real-time investor using a one-year training length would find the earning/price ratio anomaly ranked second among the three anomalies published in or before 1981. According to RP1, the E/P ratio anomaly gets zero points for 1981 because it is not the best anomaly in that year nor would it be chosen, but it will get some points using RP2 because the earning/price ratio anomaly still outperforms the dividend yield anomaly in rank for 1981.

Given an anomaly i available during the period $[t_1, t_2]$ with a training period of j years, we define the RP1 and RP2 of this anomaly as follows:

$$RP1_{i,j}(t_1,t_2) = \frac{\sum_{\tau=t_1}^{t_2}(N^\tau - 1)\cdot I(r_{i,j}^\tau = 1)\cdot (N^\tau/2)}{\sum_{\tau=t_1}^{t_2}(N^\tau - 1)}; \tag{3}$$

$$RP2_{i,j}(t_1,t_2) = \frac{\sum_{\tau=t_1}^{t_2}(N^\tau - r_{i,j}^\tau)}{\sum_{\tau=t_1}^{t_2}(N^\tau - 1)}, \tag{4}$$

where N^t is the number of anomalies available in year t; $\sum_{\tau=t_1}^{t_2}(N^\tau - 1) > 0$—namely, there is more than one anomaly available between t_1 and t_2; $I(\cdot)$ is the indicator function; and $r_{i,j}^t$ is the real-time perceived rank of anomaly i, in year t, based on a j-year training length, and in which better performing anomalies have lower rankings.

Both RP1 and RP2 are equal to their unconditional mean of 1/2 if only one anomaly ever existed between years t_1 and t_2. Namely, if $\sum_{\tau=t_1}^{t_2}(N^\tau - 1) = 0$, then $RP1_{i,j}(t_1,t_2) = RP2_{i,j}(t_1,t_2) = 1/2$. For the relative popularity in a particular year, we have

$$RP1_{i,j}^t \equiv RP1_{i,j}(t,t) = I(r_{i,j}^t = 1)\cdot(N^t/2); \tag{5}$$

$$RP2_{i,j}^t \equiv RP2_{i,j}(t,t) = \begin{cases} \frac{N^t - r_{i,j}^t}{N^t - 1} & \forall \ N^t > 1 \\ \frac{1}{2} & \text{if } \ N^t = 1 \end{cases}. \tag{6}$$

The normalization factor $N^t/2$ in (6) is such that given a particular year t, both RP1 and RP2 have an average of 1/2 across all published anomalies in year t. Indeed we have (when $N^t > 1$ for any t):

$$\overline{RP1}^{\,t} \equiv \frac{1}{N^t}\sum_{i=1}^{N^t} RP1_{i,j}^t = \frac{1}{N^t}\left[\sum_{i=1}^{N^t} I(r_{i,j}^t = 1)\right]\cdot(N^t/2) = \frac{1}{2}; \tag{7}$$

$$\overline{RP2}^{\,t} \equiv \frac{1}{N^t}\sum_{i=1}^{N^t} RP2_{i,j}^t = \frac{1}{N^t}\sum_{i=1}^{N^t}\frac{N^t - r_{i,j}^t}{N^t - 1} = \frac{1}{N^t}\sum_{i=1}^{N^t}\frac{N^t - i}{N^t - 1} = \frac{1}{2}. \tag{8}$$

Therefore, the expected values of both relative popularity measures are not dependent on time t. As such, if the rankings of published anomalies are random, there should not be any time trend in the relative popularity measures.

Figure 2 illustrates the results on the post-publication relative popularity of anomalies for four different training lengths: one year, two years, five years, and ten years. Mathematically, if we use t'_i to denote the publication year of anomaly i, the values presented in these two figures are $RP1_{i,j}(t'_i, 2010)$ and $RP2_{i,j}(t'_i, 2010)$, where i ranges from 1 to 14 for different anomalies and j may equal 1,2,5, or 10 for different training lengths. We report both the relative popularity measure that counts first place winners only (RP1) and the one that considers all ranking information (RP2). As shown in the figure, when the traning length is short, most anomalies get a good chance to be selected by the real-time trader, with earning/price ratio, momentum, and trading volume strategies standing out. This is because of their good post-publication performance as shown in Table 2. When the training length is long, the anomaly selections converge to the debt to equity ratio and the momentum strategy. While there is randomness in this process, the momentum strategy seems to be the best after its publication. This result is consistent with Schwert's (2003) finding that most anomalies disappear or attenuated after publication except for the momentum anomaly.

3.5.2 Are Old Anomalies Less Profitable?

Using the data on relative popularity for each anomaly in every year, we run fixed effects regressions to see if relative popularity measures, as well as the excess return, decrease as the "age" of an anomaly—the number of years after publication—increases. Specifically, for a given training length j, we run the following three regressions to examine the potential link between the "age" and RP1, RP2, and the excess return, controlling for individual anomaly dummies:

$$RP1^t_{i,j} = \gamma_{1,14} + \beta_1 Age^t_i + \Sigma^{13}_{m=1}\gamma_{1,l} I(m=i) + \varepsilon^t_{1,i,j}; \quad (9)$$

$$RP2^t_{i,j} = \gamma_{2,14} + \beta_2 Age^t_i + \Sigma^{13}_{m=1}\gamma_{2,l} I(m=i) + \varepsilon^t_{2,i,j}; \quad (10)$$

$$ER^t_{i,j} = \gamma_{3,14} + \beta_3 Age^t_i + \Sigma^{13}_{m=1}\gamma_{3,l} I(m=i) + \varepsilon^t_{3,i,j}, \quad (11)$$

where $ER_{i,j}^t$ represents the cumulative excess return over the market benchmark for anomaly i in year t for the past j years; Age_i^t is the number of years after publication for anomaly i as of year t; β_k is the coefficient on Age for the k^{th} regression; $\gamma_{k,i}$ is the loading on the i^{th} dummy variable for anomaly i in regression k; and $\varepsilon_{k,i,j}^t$ is the White standard error which is robust to heteroscedasticity.

Table 6 reports the results for the above three regressions. As we can see, there is no significant attenuation over time for any of the three measures if the real-time investor looks back for the past one year to past ten years. Overall, although anomalies experience decreased in-sample performance as documented in Schwert (2003) as well as in Table 2 of this paper, this reduction is not related to how long the anomaly has been published. An old anomaly is as likely to be selected by the real-time investor as a new one. Any performance reduction due to investor participation, if it occurs, happens immediately after the initial publication of the anomaly.

Another implication from Table 6 is that the performance of published asset-pricing anomalies is quite stable over time within the post-publication period. Therefore, any dynamic trading strategies taking advantage of published anomalies, such as the one presented in this paper, are likely to perform similarly in the future. Overall, the empirical results shown in Table 6 indicate that the future performance for the simulated real-time investor would remain similar to what we find using historical data.[15]

4 Conclusion

This paper examines the question whether or not a real-time and long-only investor can beat the market in real time by picking the published asset-pricing anomaly that has the best past performance. Importantly, we use the academic publication process to control for

[15]Of course, here we neglect the impact of the circulation of this paper itself. As we have already seen, academic publication does (negatively) impact the performance of an anomaly. This implies that whether or not the results found here will hold in the future may somewhat depend on the potential future publication of this paper.

the potential data-snooping bias in out-of-sample tests of anomalies. Unlike conventional methods that select anomalies to test through literature reviews on recently published papers and thus cause a potential data-snooping bias, our search is conducted via a rigorous *issue-by-issue* literature review process that is free of data-snooping and that can restore the information set faced by a real-time trader.

We find robust empirical evidence that the active strategy implemented by our real-time and long-only investor can outperform the market and other standard benchmarks.[16] Importantly, this main finding is not due to data snooping because all the evidence is from out-of-sample tests combined with the use of published anomalies. In particular, the implementation of all the strategies is done recursively in real time and only anomalies already published in top finance journals are included in the analysis. In addition, the outperformance of the active strategy is not caused purely by one or two "top" anomalies as it does not disappear even after we exclude the momentum anomaly—the most likely selected one among the set of strategies considered. We also find that although an anomaly becomes weaker after it is first published, the decay itself is relatively stable during the post-publication period.

[16]Granted, like other empirical studies, the analysis done here still relies on past performance data to draw inferences about future profitability.

Table 1: Anomalies Considered

This table summarizes the basic information of the 14 anomalies implemented in our analysis. For each anomaly, we provide its initial publications, the earliest year of available data, a brief description of the trading strategy implemented for each basic anomaly, and how the variable is calculated. JB, JF, JFE, and JFQA stand for the Journal of Business, Journal of Finance, Journal of Financial Economics, and Journal of Financial and Quantitative Analysis, respectively. The 14 trading strategies, ordered by their publication date, are based on the following cross-sectional predictive variables: earnings/price ration (E/P), dividend yield (Div/P), size, debt/equity ratio (D/E), lagged return (short-term momentum and long-term reversal), cash flow/price ratio (CF/P), book-to-market (B/M) ratio, the growth in sales (GS), return on equity (ROE), trading volume (Volume) measured by the average turnover ratio, capital investments (CI), accruals (Accrual), stock issurance (SI), and growth in book asset (GA).

Anomaly Name	Initial Publication	Data Starts	Strategy Description†	Variable Calculation
E/P	Basu (1977, JF)	1952	Buy firms with high E/P ratio	French data library
Div/P	Ball (1978, JFE)	1928	Buy firms with high dividend yield	French data library
Size	Banz (1981, JFE)	1926	Buy small firms	French data library
D/E	Bhandari (1988, JF)	1965	Buy firms with high debt/equity ratio	(book assets - book equity) / market equity
Momentum	Jegadeesh (1990, JF)	1926	Buy winners in last year	French data library
CF/P	Fama (1990, JF)	1952	Buy firms with high cash flow	French data library
B/M	Chan, Hamao, and Lakonishok (1991, JF)	1926	Buy firms with high B/M ratio	French data library
GS	Lakonishok, Shleifer, and Vishny (1994, JF)	1970	Buy firms with low sales growth rate	Weighted sales growth rank in past five years
ROE	Haugen and Baker (1996, JFE)	1966	Buy firms with high return on equity	Earnings per share / book value per share
Volume	Lee and Swaminathan (2000, JF)	1964	Buy firms with low average turnover ratio	Annual average turnover ratio in the past year
CI	Titman, Wei, and Xie (2004, JFQA)	1966	Buy firms with low capital investments	CAPEX / avg. CAPEX in past three years - 1
Accrual	Chan, Chan, Jegadeesh, and Lakonishok (2006, JB)	1966	Buy firms with low accruals	Eq. (1) in Sloan (1996)
SI	Daniel and Titman (2006, JF)	1970	Buy firms with low stock issuance	log(5-year increase in market equity) - log(5-year return)
GA	Cooper, Gulen, and Schill (2008, JF)	1966	Buy firms with low growth in book asset	Growth rate in data item "AT" (total asset)

† In brief descriptions of strategies, "buy" means holding a decile portfolio of a particular risk factor throughout the portfolio holding period.

Table 2: Performance of Individual Anomalies

This table reports the performance of individual anomalies, based on a long-only strategy, against different benchmarks. For each of 14 anomalies (column 1), we report its year of the first publication (column 2), average annual excess return over the market index (R_p–R_m) before (column 3) and after publication (column 3), post-publication terminal wealth (column 5), and Sharpe ratio (column 7). Columns 6 and 8 show the market index's terminal wealth and Sharpe ratio during the same time period, respectively. Statistical significance of R_p–R_m are calculated using standard t-test against zero. Terminal wealth is the dollar amount (after transaction costs) at the end of year 2010 with an initial wealth of $1 in the publication year of a given anomaly. The bottom two rows report respectively the performance for the average of *Published* anomalies and the average of *All* anomalies, published or not. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Anomaly	Publish	R_p–R_m(%)		Terminal Wealth($)		Sharpe Ratio	
	Month	Before	After	Anomaly†	Benchmark	Anomaly	Benchmark
E/P	197706	7.04***	4.53***	106.98	38.28	0.61	0.43
Div/P	197809	2.09	0.26	24.20	31.54	0.42	0.41
Size	198103	9.92**	0.90	15.33	21.01	0.35	0.41
D/E	198806	6.75***	8.00***	35.12	8.28	0.79	0.44
Momentum	199007	7.70***	6.26**	13.37	5.89	0.57	0.41
CF/P	199009	5.92***	4.59**	12.18	6.55	0.63	0.46
B/M	199112	5.46**	5.54*	10.13	5.29	0.57	0.43
GS	199412	4.08**	2.18	5.13	4.01	0.63	0.41
ROE	199607	3.37	7.88***	6.56	2.64	0.64	0.30
Volume	200010	-0.05	11.97**	3.60	1.21	0.66	0.07
CI	200412	4.27**	1.27	1.30	1.33	0.25	0.22
Accrual	200605	0.18	3.14	1.26	1.12	0.31	0.11
SI	200608	4.37**	6.49	1.43	1.15	0.44	0.17
GA	200808	5.48***	11.29*	1.35	1.08	0.59	0.24
Avg of Anomalies							
Published	1977	N/A	3.30**	72.73	35.77	0.59	0.41
All	1977	N/A	4.45***	100.7	35.77	0.65	0.41

† In calculating the terminal wealth, a 1% annual transaction cost is considered for all anomalies.

Table 3: The Out-of-Sample Performance with Different Training Lengths

This table presents the out-of-sample performance of an investor who picks the best performing anomaly among published anomalies recursively over time. The training length varies from one year to ten years. For each training length, we report the average annual *strategy* return and the market *benchmark* return. We also report three performance measures of the real-time return over the benchmark index: the annual excess return (*Excess Return*), the alphas including CAPM, three-factor, and four-factor alphas, and the information ratio of the real-time investor relative to the market index (*Info Ratio*). We report factor alphas under both the constant beta assumption and with time-varying betas updated in real-time using past five years of monthly data. The detailed calculations of alphas and information ratio are described in Section 2.3.5. The standard deviations are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Training	Return(%)		Excess	Alpha w/ const. β (%)			Alpha w/ non-const. β (%)			Info
Length	Strategy	Benchmark	Return(%)	CAPM	3-Factor	4-Factor	CAPM	3-Factor	4-Factor	Ratio
1-Year	16.86	12.58	4.29*	3.28	2.27	2.15	4.59**	1.29	0.46	0.34
	(22.90)	(17.39)	(12.79)	(2.36)	(2.32)	(2.75)	(13.48)	(17.00)	(17.39)	(0.17)
2-Year	23.15	12.58	10.57***	9.57***	8.60**	9.19**	11.10***	6.13*	5.16	0.67**
	(24.30)	(17.39)	(15.74)	(2.93)	(3.22)	(3.80)	(16.48)	(21.14)	(21.79)	(0.18)
3-Year	22.70	12.58	10.12***	8.37***	8.05**	10.55***	10.25***	6.18**	5.38*	0.68**
	(25.06)	(17.39)	(14.78)	(2.66)	(2.95)	(3.38)	(15.23)	(19.38)	(20.63)	(0.18)
4-Year	22.24	12.58	9.67***	7.30***	6.62**	8.90**	9.68***	5.99**	5.31*	0.64**
	(26.36)	(17.39)	(15.11)	(2.63)	(2.94)	(3.38)	(15.39)	(19.52)	(20.87)	(0.18)
5-Year	21.33	12.58	8.75***	6.62**	6.07**	7.71**	8.18***	5.47**	4.77*	0.62**
	(25.46)	(17.39)	(14.09)	(2.46)	(2.75)	(3.20)	(14.19)	(18.37)	(19.80)	(0.18)
6-Year	21.24	12.58	8.66***	6.39**	5.89**	7.52**	8.61***	4.98*	3.87	0.63**
	(25.32)	(17.39)	(13.72)	(2.36)	(2.66)	(3.09)	(14.47)	(17.55)	(19.16)	(0.18)
7-Year	19.59	12.58	7.01***	4.71*	4.79*	6.58**	6.99***	3.04	2.20	0.52*
	(25.27)	(17.39)	(13.59)	(2.33)	(2.53)	(2.92)	(14.68)	(17.41)	(18.95)	(0.18)
8-Year	20.50	12.58	7.92***	5.59**	6.01**	9.57***	8.03***	4.52*	2.76	0.54*
	(25.75)	(17.39)	(14.67)	(2.55)	(2.76)	(3.00)	(15.57)	(18.04)	(19.43)	(0.18)
9-Year	20.15	12.58	7.57***	5.99**	4.90*	3.61	7.58***	4.65*	2.90	0.52*
	(24.68)	(17.39)	(14.55)	(2.64)	(2.85)	(3.34)	(15.45)	(16.24)	(17.62)	(0.18)
10-Year	19.35	12.58	6.77**	4.94*	3.74	5.59	6.68***	3.58	2.26	0.45*
	(25.14)	(17.39)	(15.01)	(2.70)	(3.00)	(3.49)	(15.67)	(16.08)	(18.01)	(0.18)

Table 4: Out-of-Sample Performances under Different Transaction Costs

This table presents the out-of-sample performance of an investor who picks the best past performer among the 14 anomalies recursively over time. Panels A through C represent different training lengths of 2, 5, and 10 years, respectively. We report cases with seven different schemes of transaction cost. The first five rows in each panel use a fixed round-trip transaction cost of 0%, 0.5%, 1%, 1.5%, and 2%, respectively. The case *Customized* assumes the same base level transaction cost for all strategies except that it is 5% for the size anomaly and the volume effect. The *real data* case uses the annual estimated transaction costs from French (2008) for years 1980 through 2006 and linearly extrapolate their transaction cost data to other years. For each level of transaction cost, we report the average annual *strategy* return and the market *benchmark* return. We also report three performance measures of the real-time return over the benchmark index: the annual excess return (*Excess Return*), the alphas including CAPM, three-factor, and four-factor alphas, and the information ratio of the real-time investor relative to the market index (*Info Ratio*). The detailed calculations of alphas and information ratio are described in Section 2.3.5.

Transaction	Return (%)		Excess	Alpha w/ const. β (%)			Info
Costs	Strategy	Benchmark	Return(%)	CAPM	3-Factor	4-Factor	Ratio
Panel A: 2-Year Training Period							
0.0%	24.15	12.58	11.57***	10.57***	9.60***	10.19**	0.73***
0.5%	23.65	12.58	11.07***	10.07***	9.10***	9.69**	0.70***
1.0%	23.15	12.58	10.57***	9.57***	8.60**	9.19**	0.67**
1.5%	22.65	12.58	10.07***	9.07***	8.10**	8.69**	0.64**
2.0%	22.15	12.58	9.57***	8.57***	7.60**	8.19**	0.61**
Customized	22.56	12.58	9.98***	8.94***	8.27**	8.82**	0.66**
Real Data	22.84	12.58	10.27***	9.27***	8.27**	8.64**	0.66**
Panel B: 5-Year Training Period							
0.0%	22.33	12.58	9.75***	7.62***	7.07**	8.71**	0.69**
0.5%	21.83	12.58	9.25***	7.12***	6.57**	8.21**	0.66**
1.0%	21.33	12.58	8.75***	6.62**	6.07**	7.71**	0.62**
1.5%	20.83	12.58	8.25***	6.12**	5.57*	7.21**	0.59*
2.0%	20.33	12.58	7.75***	5.62**	5.07*	6.71**	0.55*
Customized	20.51	12.58	7.93***	5.84**	5.67**	7.22**	0.57*
Real Data	21.03	12.58	8.45***	6.31**	5.74**	7.15**	0.61**
Panel C: 10-Year Training Period							
0.0%	20.35	12.58	7.77***	5.94**	4.74	6.59*	0.52*
0.5%	19.85	12.58	7.27***	5.44*	4.24	6.09*	0.48*
1.0%	19.35	12.58	6.77**	4.94*	3.74	5.59	0.45*
1.5%	18.85	12.58	6.27**	4.44	3.24	5.09	0.42
2.0%	18.35	12.58	5.77**	3.94	2.74	4.59	0.38
Customized	18.76	12.58	6.18**	4.32	3.35	5.28	0.40
Real Data	19.04	12.58	6.47**	4.64*	3.41	5.04	0.44

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

Table 5: The Out-of-Sample Performance with a Subset of Anomalies

This table presents the out-of-sample performances of an investor who picks the best performing anomaly out of random subsets of the some anomalies recursively over time. The training period for this table is two years. Each row summarizes simulations of having only a subset of the total 14 available anomalies. The number of selected anomalies, *No. of Anommalies*, ranges from one to fourteen. *No. of Comb'n* is the total number of different combinations of anomalies to form a subset of the group. We report the average annual *strategy* return and the market *benchmark* return. We also report three performance measures averaging across all combinations. *Excess Ret* is the average annual excess return. *Alpha* includes the averages for CAPM, three-factor, and four-factor alphas. *Info Ratio* is the average information ratio of the real-time trader's strategy relative to the market. The detailed calculations of alphas and information ratio are described in Section 2.3.5. All values reported to the right of the second column are the average values over all cases represented by the number of combinations (*No. of Comb'n*).

No. of		Average Return(%)		Excess	Alpha w/ const. β (%)			Info
Anomalies	Comb'n	Strategy	Benchmark	Ret (%)	CAPM	3-Fac	4-Fac	Ratio
1	14	13.76	9.49	4.27	4.47	1.09	1.25	0.30
2	91	16.62	11.11	5.52	5.78	3.23	3.92	0.41
3	364	18.17	11.79	6.38	6.57	4.22	5.03	0.47
4	1001	19.14	12.14	7.00	7.05	4.87	5.74	0.51
5	2002	19.81	12.34	7.48	7.38	5.38	6.34	0.53
6	3003	20.34	12.46	7.88	7.66	5.82	6.86	0.55
7	3432	20.79	12.54	8.24	7.91	6.22	7.32	0.56
8	3003	21.19	12.60	8.59	8.16	6.59	7.74	0.58
9	2002	21.58	12.63	8.94	8.41	6.94	8.12	0.60
10	1001	21.93	12.65	9.29	8.67	7.29	8.44	0.61
11	364	22.27	12.65	9.62	8.91	7.62	8.72	0.63
12	91	22.59	12.63	9.95	9.15	7.95	8.94	0.64
13	14	22.88	12.61	10.27	9.37	8.28	9.10	0.66
14	1	23.15	12.58	10.57	9.57	8.60	9.19	0.67

Table 6: Relative Popularity, Excess Returns, and the Age of Anomaly

This table presents the results from the following fixed effects regressions of the relative popularity measures RP1 and RP2, and the annual excess return (ExRt):

$$\begin{aligned} RP1_{i,j}^{t} &= \gamma_{1,14} + \beta_1 Age_i^t + \Sigma_{m=1}^{13}\gamma_{1,l} I(m=i) + \varepsilon_{1,i,j}^{t}; \\ RP2_{i,j}^{t} &= \gamma_{2,14} + \beta_2 Age_i^t + \Sigma_{m=1}^{13}\gamma_{2,l} I(m=i) + \varepsilon_{2,i,j}^{t}; \\ ExRt_{i,j}^{t} &= \gamma_{3,14} + \beta_3 Age_i^t + \Sigma_{m=1}^{13}\gamma_{3,l} I(m=i) + \varepsilon_{3,i,j}^{t} \end{aligned}$$

Training length j ranges from one year to ten years. β_1, β_2, and β_3 are the coefficients for the RP1 measure counting first place winners only, the RP2 measure counting all rank information, and the annual excess return over the market benchmark, respectively. $\gamma_{k,i}$, $k = 1, 2, 3$, is the loading on the i-th dummy variable for anomaly $i = 1, \ldots, 14$. The R-squared and t statistics based on the White standard error are reported for each coefficient with statistical significance marked.

Training Length	β_1	t	R^2	β_2	t	R^2	β_3	t	R^2
1 Year	-0.0013	-0.10	0.08	-0.0013	-0.42	0.09	0.0014	1.08	0.05
2 Years	-0.0042	-0.33	0.07	-0.0012	-0.40	0.16	0.0025	0.84	0.11
3 Years	-0.0034	-0.29	0.12	-0.0005	-0.18	0.21	0.0038	0.95	0.13
4 Years	-0.0016	-0.14	0.17	0.0002	0.07	0.26	0.0058	1.12	0.15
5 Years	-0.0005	-0.04	0.18	-0.0001	-0.05	0.31	0.0062	0.93	0.17
6 Years	0.0031	0.26	0.18	0.0002	0.08	0.31	0.0044	0.48	0.18
7 Years	-0.0057	-0.55	0.24	-0.0003	-0.10	0.32	0.0025	0.23	0.21
8 Years	-0.0061	-0.61	0.27	-0.0011	-0.41	0.35	0.0007	0.06	0.22
9 Years	-0.0038	-0.37	0.26	-0.0007	-0.27	0.42	0.0016	0.13	0.26
10 Years	-0.0025	-0.25	0.26	-0.0007	-0.30	0.43	0.0014	0.11	0.28

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

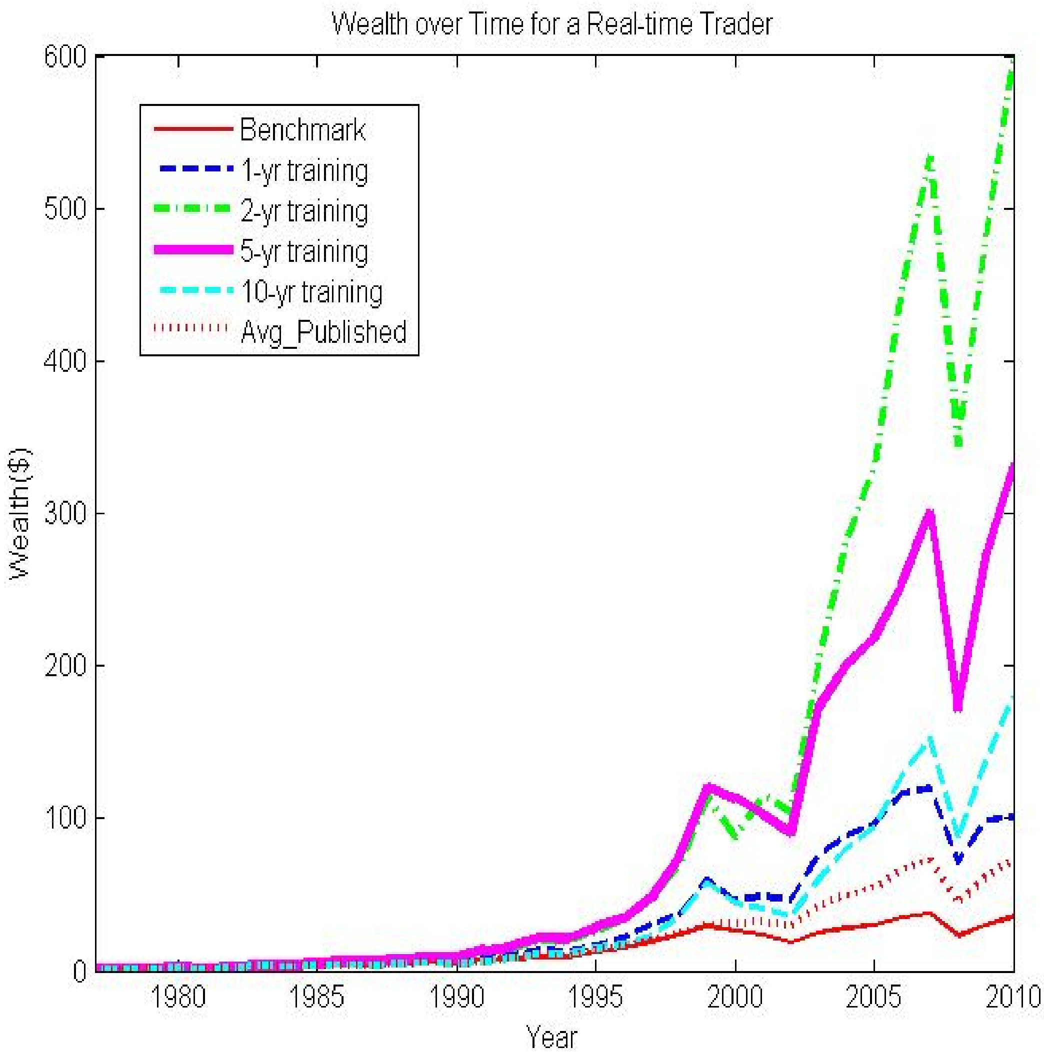

Figure 1: Terminal Wealth of Various Strategies Starting with $1

This figure plots the terminal wealth of strategies that include the market benchmark, the active strategy that picks the best performing published anomaly over time, and the simple average of all published anomalies. The initial wealth is $1. The training lengths for the active strategy are 1 year, 2 years, 5 years, and 10 years. The simulation starts from 1977 when the first anomaly is published, and ends in 2010. A one percent round-trip transaction cost is applied to the active strategy and the simple average strategy.

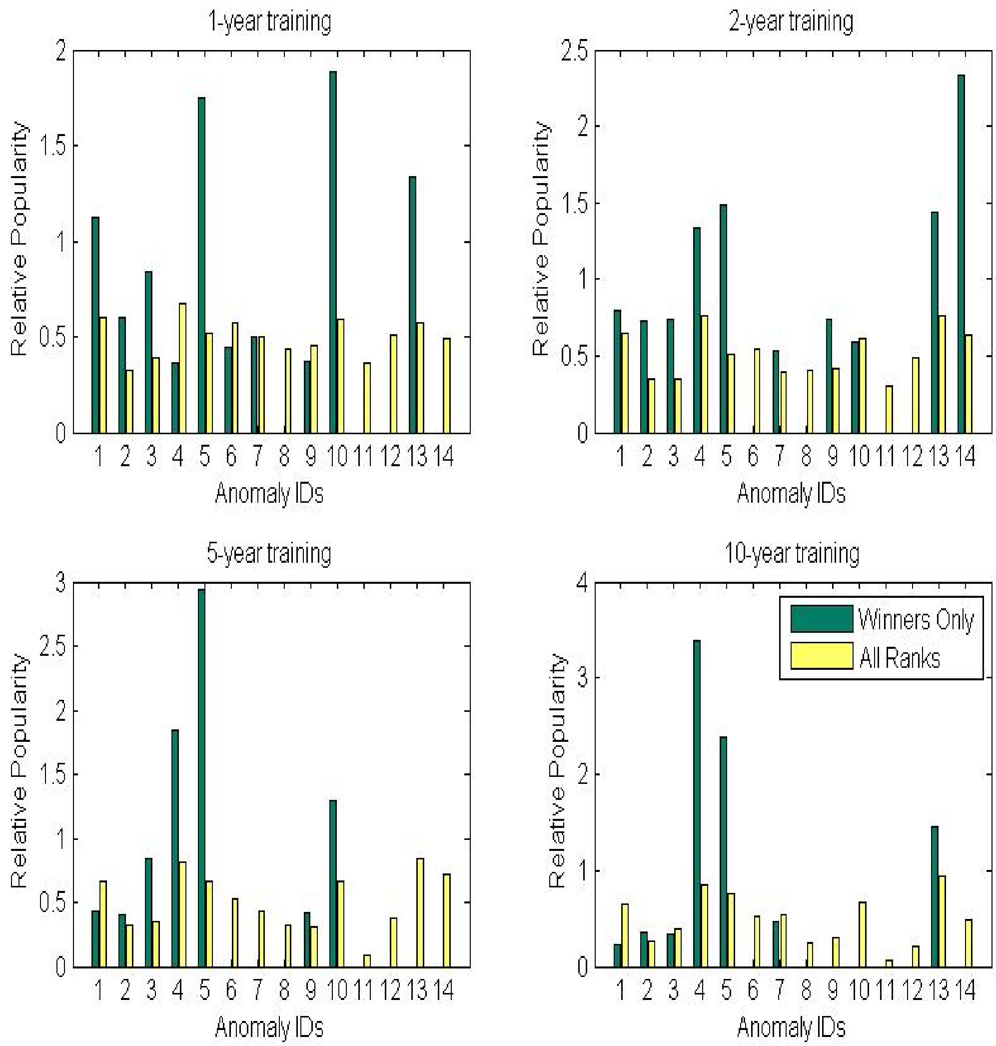

Figure 2: Relative Popularity of Anomalies

This figure plots the post-publication relative popularity of each anomaly over the whole sample period when an investor picks from all published anomalies in real time. The Y-axis is the value of the two relative popularity measures as defined in section 3.5.1. The X-axis represents different anomalies indexed by a sequence number: 1 for earnings/price ratio, 2 for dividend yield, 3 for size, 4 for debt/equity ratio, 5 for momentum, 6 for cashflow/price ratio, 7 for book-to-market ratio, 8 for growth in sales, 9 for return on equity, 10 for trading volume measured by average turnover ratios, 11 for capital investments, 12 for accrual, 13 for stock issuance, and 14 for growth in book asset.

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