

Technical Trading Rules in Australian Financial Markets

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Received: July 17, 2014

Accepted: July 31, 2014

Online Published: September 25, 2014

doi:10.5539/ijef.v6n10p67

URL: <http://dx.doi.org/10.5539/ijef.v6n10p67>

Abstract

In this paper, we apply the 7,846 technical trading rules considered by Sullivan et al. (1999) to a stock index, some individual stocks, some currencies and some interest rate futures contracts traded in the Australian financial markets, and test for profitability relative to a buy-and-hold strategy. Size distortions due to data-snooping are avoided by using the Reality Check test of White (2000) and the Superior Predictive Ability test of Hansen (2005). We find no evidence that technical trading rules provide trading profits in excess of those available from a simple buy-and-hold strategy.

Keywords: asset pricing, financial forecasting and simulation, investment decisions, hypothesis testing, financial econometrics

1. Introduction

Despite the scepticism of some in the academic community, technical analysis and technical trading rules continue to be widely used in the finance industry. In a recent survey of 682 fund managers in five different countries, Menkhoff (2010) found that 87% of respondents place at least some importance on technical analysis, and in a survey of foreign exchange dealers in Germany and Austria, Gehrig and Menkhoff (2006) found that over 95% made some use of technical analysis (Note 1).

In this paper we consider the profitability of a large number of alternative parameterisations of 5 classes of technical trading rule using data from the Australian financial markets. In total, we consider 7,846 different rules (Note 2). These are tested against a benchmark buy-and-hold strategy. To avoid spurious results due to data-snooping, for each asset we test the null hypothesis that the most profitable rule is no more profitable than the benchmark strategy using both the Reality Check test due to White (2000) and the Superior Predictive Ability test of Hansen (2005). While previous studies have considered the profitability of technical trading rules in the Australian markets, they have typically focused on a small number of trading rules. To our knowledge, this is the first study of Australian financial markets to consider such a wide range of trading strategies using established statistical testing methodologies that are robust to data-snooping.

The remainder of this paper is organized as follows: Section 2 discusses the existing literature on technical analysis with particular emphasis on studies of the Australian markets. Section 3 provides an outline of the methodology of the research. Section 4 presents the empirical results of the study. Finally, conclusions are presented in Section 5.

2. Previous Studies

The academic research literature has a long history of investigating the profitability of technical trading rules, stretching back at least as far as Cowles (1933). Park and Irwin (2007) provide a comprehensive review of much of this literature which we recommend to the interested reader (Note 3). Of the 95 studies that they considered, Park and Irwin (2007) found that 56 yielded positive results, 20 studies found negative results, and 19 found mixed results. Accordingly, on face value, the balance of evidence might be taken to favour the proposition that technical trading rules have predictive power. However, it should be noted that many existing studies are open to criticism. In particular, given the wide range of rules that may be tested for any particular financial asset, the charge that much of the apparent evidence in favour of technical trading rule profitability is in fact the result of data-snooping must be taken seriously.

In recent years new approaches to multiple hypothesis testing that control the family-wise error rate (Note 4)

have been developed. In particular, White (2000) developed the Reality Check test and Hansen (2005) developed the Superior Predictive Ability test. These tests work by considering a large number of test statistics simultaneously, and computing the distribution of the largest statistic. Consequently, they avoid the spurious positive results that occur when standard pairwise tests of equal predictive ability are used over multiple pairs of rules, with evidence of profitability claimed if any individual null hypothesis is rejected. A number of studies of technical trading rule profitability have utilised these tests. Some (e.g., Hsu & Kuan, 2005; Metghalchi et al., 2008; Metghalchi et al., 2012) still find evidence of profitability when data snooping is accounted for. Others (e.g., Marshall et al., 2008) find no evidence of profitability. A common finding for US markets (e.g., Shynkevich, 2012; Qi & Wu, 2006; Sullivan et al., 1999) is that evidence exists of profitability in the first half of the sample, but the evidence is much weaker, or non-existent, in the latter half of the sample.

In contrast to the wealth of studies that have considered technical trading rules in the context of the large northern hemisphere markets, relatively few past studies have considered the Australian markets. The profitability of technical trading rules for Australian stock market indices has been considered by Ball (1978), Batten and Ellis (1996), Ellis and Parbery (2005) and Loh (2004). None of these studies found evidence in favour of technical rules. Pavlov and Hurn (2012) consider moving average rules for a cross-section of Australian stocks and report evidence of losses, which they interpret as a contrarian profit. Lento (2007) considers three different parameterizations for three different trading rules for an Australian stock index and finds evidence that two of the nine rules considered generate excess profits. Lee et al. (2001), Olson (2004) and Hawtrey and Nguyen (2006) have considered technical trading rules for the Australian dollar. Lee et al. (2001) found no evidence of profitability. Olson (2004) and Hawtrey and Nguyen (2006) found evidence of profitability in the early part of their samples, but no evidence in the later data.

Overall, the literature provides little empirical support for the contemporary use of technical trading rules in Australian markets. However, it should be noted that the Australian studies cited above each consider a narrow range of trading rules. Theory provides relatively little guidance about the types of rules and parameter values that should be profitable. Consequently, the body of evidence on the profitability of technical trading rules is not complete until a wide range of trading rules and parameterizations have been considered.

The present paper contributes to the literature on technical trading rules by providing a far more comprehensive empirical analysis of the profitability of technical trading rules than currently exists for the Australian financial markets. We consider the 7,846 different trading rules that were used by Sullivan et al. (1999) in their analysis of the Dow Jones Industrial Index. These consist of a range of parameterizations of each of 5 well-known technical trading rules. We apply each of these rules to a value-weighted stock index, 6 individual stocks (3 large-cap; 3 small-cap), 3 exchange rates relative to the Australian dollar, and 3 interest rate futures contracts over the time period January 1993 to December 2012 and to 4 sub-periods. In each case we use the Reality Check test of White (2000) and the Superior Predictive Ability test of Hansen (2005) to compute the probability that the most profitable trading rule generates profits no better than a buy and hold strategy.

3. Methodology

In this section, we describe the data that we used in the study, the trading rules that we considered, and the statistical methodology that we applied.

3.1 Data

Our data set spans the period 1st, January 1993 to 31st December 2012. In addition to considering the complete span of data, we also conduct the analysis for 4 sub-periods: 1st January 1993 to 31st December 1997, 1st January 1998 to 31st December 2002, 1st January 2003 to 31st December 2007, and 1st January 2008 to 31st December 2012. A common finding in the literature (e.g., Sullivan et al., 1999; Taylor, 2014) is that the profitability of technical trading rules varies over time in the US market. A consideration of sub-periods allows for this possibility in the Australian markets. Our variables are as follows:

ASX: The ASX200 value-weighted stock index of the largest 200 firms by capitalisation listed on the Australian Securities Exchange.

BHP: BHP Billiton Limited (single stock, large-cap).

CBA: Commonwealth Bank of Australia (single stock, large-cap).

WES: Wesfarmers Limited (single stock, large-cap).

APN: APN News and Media Limited (single stock, small-cap).

BPT: Beach Energy Limited (single stock, small-cap).

PPT: Perpetual Limited (single stock, small-cap).

USD: Australian dollar / US dollar exchange rate.

JPY: Australian dollar / Japanese Yen exchange rate.

GBP: Australian dollar / British Pound exchange rate.

BB90: ASX 90 Day Bank Accepted Bill Futures.

TB3Y: ASX 3 Year Treasury Bond Futures.

TB10Y: ASX 10 Year Treasury Bond Futures.

The large-cap stocks were all in the top 20 stocks on the Australian market by capitalization. The small-cap stocks all lie outside the top 100 stocks by capitalization. All data are taken from the Thomson-Reuters Datastream database (Note 5).

3.2 Trading Rules

The trading rules that we use are those considered by Sullivan et al. (1999). We provide a description of each class of rule below. For more precise details, including the range of parameters used for each class of rule and references, the reader is referred to Sullivan et al. (1999) Section III and Appendix A.

Filter rules: Filter rules require the investor to buy and hold an asset if its daily closing price moves up by more than a predefined threshold (x). The position is held until the daily closing price falls beneath the subsequent highest price by x . At that point, the asset is simultaneously sold and shorted (Note 6). The short position is maintained until the price increases by more than x from its subsequent lowest daily closing price, at which point the short position is reversed and the asset purchased. Three variations on the basic filter rule are also considered:

- 1) Allow for neutral positions to be held if the increase or decrease in the price is more than another predefined threshold (y , where $y < x$).
- 2) Force each position to be held for a predefined minimum number of days (c).
- 3) Redefine high (low) prices to be higher (lower) than the prices for the previous e days, where e is a predefined number.

In the tables of results in Section IV, the parameterised filter rules are denoted $FR(x, e, c, y)$. In total, we consider 497 different filter rules made up from all possible combinations of parameters considered by Sullivan et al. (1999) (Note 7).

Moving average rules: A moving average rule is implemented by constructing two moving averages (Note 8)—a short-ordered moving average and a long-ordered moving average—where the long-ordered moving average is necessarily of higher order than the short-ordered moving average. Buy and sell signals are generated when the short-ordered moving average crosses the long-ordered moving average. Thus, when the short-ordered moving average is greater than the long-ordered moving average, the investor should be long, and when the short-ordered moving average is less than the long-ordered moving average, the investor should be short in the asset. Note that the short-ordered moving average could be of order 1, in which case the trading signals are generated when the asset price crosses the long-ordered moving average. Three variations on the basic moving average rule are considered:

- 1) Instead of the trading signal being generated at the time that the two moving averages cross, it is generated when the moving averages have crossed and now differ by more than a fixed amount (b).
- 2) The trading signal is only generated when the moving averages cross and remain crossed for a predefined number of days (d).
- 3) All changes in positions may be held for a minimum of c days irrespective of the trading signals generated during that time.

In the tables of results in Section IV, the parameterised moving average rules are denoted $MA(n, m, b, d, c)$. In total, we consider 2,049 different moving average rules.

Support and resistance rules: Rules based on support and resistance lines involve buying the asset when the closing price exceeds a local maximum and shorting when the closing price is less than a local minimum. The maxima (minima) may be defined as the maximum (minimum) price over the previous n days. Alternatively, the maxima (minima) may be defined as the most recent closing price that is greater (less) than the previous e closing prices. Other variations on the rule are:

- 1) To require that any position is held for a minimum of c days.
- 2) To ignore a signal until it has been maintained for a minimum of d days.
- 3) To require the difference between the price and the maximum or minimum to exceed a predefined percentage (b) before a trading signal is recorded.

In the tables of results in Section IV, the parameterised support and resistance rules are denoted $SAR(n,e,b,d,c)$. In total, we consider 1,220 support and resistance rules.

Channel breakout rules: A channel is defined as a situation in which the highest closing price over the previous n days is within x percent of the lowest closing price over the previous n days. A channel breakout occurs when the current closing price lies outside the channel. A buy signal occurs when the current price exceeds the channel. A sell signal occurs when the current price is less than the channel. All positions are held for a fixed number of days (c). A variation on the basic channel breakout rule is to require that the difference between the current price and the border of the channel is more than b percent before a trading signal is recorded. In the tables of results in Section IV, the parameterised channel breakout rules are denoted $CBO(n,x,b,c)$. We consider a total of 2,040 channel breakout rules.

On-balance volume moving averages: An on-balance volume indicator is constructed by taking the cumulative sum of volumes from days in which the closing price increases and subtracting the cumulative sum of volumes from days in which the closing price decreases. The moving average rules described above are then applied to the on-balance volume indicator to generate trading signals. In the tables of results in Section IV, the parameterised on-balance volume rules are denoted $OBV(n,m,b,d,c)$ where the parameters refer to the construction of the moving averages and are defined above. In total, we consider 2,040 on-balance volume moving averages.

3.3 Statistical Methodology

Each of the above trading rules is applied to every asset over the complete sample and for each sub-sample and the returns are computed. In cases where a trading rule dictates that the position should be neither short nor long, the funds in the portfolio are invested at an interest rate equal to the overnight cash rate that is targeted by the Reserve Bank of Australia. Similarly, when an asset is shorted, it is assumed that the cost of maintaining the short position is equal to the overnight cash rate. The data for the cash rate are taken from Thomson-Reuters Datastream. We assume that all other trading costs are zero. While this assumption is somewhat unrealistic, it simplifies the analysis since it circumvents the fact that trading costs may vary between traders, across assets and over time. Furthermore, the effect of trading costs on profitability is only of interest once it has been established that trading rules are indeed profitable, which has not yet been done conclusively for the Australian markets. The returns are also computed for a benchmark portfolio that consists of buying and holding an asset until the end of the (sub-) sample period.

For each asset in each sub-sample, and for the complete sample, we compute three statistics. The first statistic is the p-value for the Diebold and Mariano (1995) test for equal predictive ability. The null hypothesis for the Diebold-Mariano test that we conduct is

$$H_0: E(r_{max,t+1} - r_{0,t+1}) = 0$$

where $r_{max,t+1}$ is the return of the most profitable trading rule and $r_{0,t+1}$ is the return from the benchmark buy-and-hold strategy. The test statistic is

$$d = \frac{|r_{max,t+1} - r_{0,t+1}|}{\sqrt{\text{var}(r_{max,t+1} - r_{0,t+1})}}$$

The p-value (p_{DM}) is then computed by integrating the relevant t-distribution beyond $\pm d$. Note that, our application of the Diebold-Mariano test involves choosing the most profitable of the 7,846 trading strategies, and comparing its returns to the benchmark strategy. Consequently, it is likely to be oversized, and it is computed only to determine whether data-snooping bias leads to misleading results in these applications.

The second statistic that we compute is White's Reality Check statistic. The null hypothesis for this test is

$$H_0: \max_{k=1, \dots, M} \mu_k \leq 0 \quad (1)$$

where $\mu_k = E(r_{k,t+1} - r_{0,t+1})$. The test statistic is constructed by first computing for each trading rule the performance measure

$$f_{k,t+1} = r_{k,t+1} - r_{0,t+1} \quad (2)$$

for $k = 1, \dots, M$, where M is the number of trading rules. The test statistic is computed as

$$\bar{V}_n = \max_{k=1, \dots, M} \sqrt{nf_k} \quad (3)$$

where $\bar{f}_k = \sum_{t=1}^n f_{k,t}/n$ and n is the number of observations in the sample.

To find an asymptotic p-value for \bar{V}_n , White (2000) suggested implementing the stationary bootstrap method of Politis and Romano (1994). In the stationary bootstrap, each pseudo-sample is constructed by randomly drawing contiguous blocks of observations from the time series and joining them together to form a series of the same length as the observed time series. Excess observations in the last drawn block are discarded. The starting index for each block is drawn from a uniform distribution, and the block length is independently drawn from a geometric distribution. Following Sullivan et al. (1999) we parameterise the geometric distribution so that the expected block length is 10. Several authors report results that are quite insensitive to the value chosen for the expected block length (e.g., Sullivan et al., 1999; Hsu & Kuan, 2005; Metghalchi et al., 2008; Hsu et al., 2010). Consequently, we do not experiment with this value. For each bootstrap sample, the returns from the benchmark buy-and-hold strategy $r_{0,t+1}^*$ and from each of the technical trading rules $r_{k,t+1}^*, k=1, \dots, M$ are calculated over the relevant sample period and for each trading rule we compute the bootstrapped performance statistic.

$$\bar{f}_{k,t+1}^* = r_{k,t+1}^* - r_{0,t+1}^* \quad (4)$$

Denote $\bar{f}_k^*(b) = \sum_{t=1}^n \bar{f}_{k,t}^*(b)/n$. We estimate the empirical distribution of \bar{V}_n^* with the realisations:

$$\bar{V}_n^*(b) = \max_{k=1, \dots, M} \sqrt{n} (\bar{f}_k^*(b) - \bar{f}_k), \quad b = 1, \dots, B. \quad (5)$$

where B is the number of bootstrap simulations. White's reality check p-value is estimated by

$$P_{RC} = \sum_{b=1}^B \frac{\mathbf{1}(\bar{V}_n^* > \bar{V}_n)}{B} \quad (6)$$

where $\mathbf{1}(\cdot)$ takes a value of 1 when its argument is true, and zero otherwise. The null hypothesis is rejected if the p-value is less than a given significance level.

The third statistic that we compute is the p-value for the Superior Predictive Ability test of Hansen (2005). Hansen observed that the null hypothesis of White's Reality Check statistic is a composite hypothesis and that the null probability density function of the test statistic is computed under the configuration that is least favourable to the alternative hypothesis. This causes the test to perform poorly in cases in which the analysis includes many poorly performing models in addition to some that perform well. Accordingly, Hansen (2005) proposed two modifications of the Reality Check test.

Firstly Hansen (2005) proposed that a studentized test statistic be used.

$$\tilde{V}_{SPA} = \max \left(\max_{k=1, \dots, M} \frac{\sqrt{n} \bar{f}_k}{\hat{\omega}_k}, 0 \right) \quad (7)$$

where $\hat{\omega}_k^2$ is a consistent estimator of $\text{var}(\sqrt{n} \bar{f}_k)$, computed from the stationary bootstrap. Secondly, he proposed a sample-dependent computation of the null distribution that results in the following bootstrap statistics.

$$\begin{aligned} \tilde{V}_{SPA}^*(b) &= \max \left(\max_{k=1, \dots, M} \frac{\sqrt{n} (\bar{f}_k^* - \bar{f}_k) I(\bar{f}_k^* - \bar{f}_k > \omega_k \sqrt{2 \log \log n})}{\hat{\omega}_k}, 0 \right) \\ & \quad b = 1, \dots, B. \end{aligned} \quad (8)$$

By counting $\tilde{V}_{SPA}^* > \tilde{V}_{SPA}$, the p-value can be calculate as

$$P_{SPA} = \sum_{b=1}^B \frac{\mathbf{1}(\tilde{V}_{SPA}^* > \tilde{V}_{SPA})}{B} \quad (9)$$

4. Empirical Results

The results for the full sample period with zero transactions costs are presented in Table 1. The column 'Return' provides the return earned by the most profitable of the 7,846 trading rules over the sample period. The column 'Best rule' indicates which rule generated the highest return. The notation used for the rules is explained in Section 3.2. The column 'Bench' provides the return earned by the benchmark buy-and-hold strategy. For the 13 assets considered, a technical trading rule was more profitable than the benchmark strategy over the sample period for 8 assets. For 3 of the 5 assets for which the benchmark strategy is superior, a filter rule was the most profitable of the technical trading rules. In the (more interesting) cases in which a technical trading rule was most profitable a filter rule was superior for 3 assets, an on-balance volume rule was superior for 2 assets, channel

breakout rules were the most profitable for 2 assets and a support and resistance rule was superior for the remaining asset. It should be noted however that for only 2 assets (CBA and APN) was the pairwise difference in the returns of the best technical rule and the benchmark strategy statistically significantly different from zero at the 5% significance level according to the Diebold-Mariano test (the column p_{DM} contains the p-values for this test). Furthermore, since the p-values for the Reality Check (p_{RC}) and the Superior Predictive Ability (p_{SPA}) test are all quite large, it is clear that once data snooping is considered in the construction of the test, there is no

Table 1. Full period: Jan. 1993 to Dec. 2012

	Best Rule	Bench	Return	p_{DM}	p_{RC}	p_{SPA}
ASX	FR (0.005,3,0,0)	6.44	7.24	0.3500	0.9800	0.9800
BHP	FR(0.2,5,0,0)	4.25	4.77	0.4396	0.9959	0.9663
CBA	FR(0.3,20,0,0)	10.08	9.96	0.4941	0.9975	0.9537
WES	FR(0.4,0,10,0)	7.95	9.59	0.3317	0.9987	0.9467
APN	SAR(250,0,0.02,0,25)	-10.36	7.85	0.0330	0.4419	0.3302
BPT	FR(0.4,1,0,0)	10.15	19.93	0.1511	0.9785	0.8863
PPT	FR(0.4,15,0,0)	9.92	8.68	0.5827	0.9990	0.9880
GBP	SAR(10,0,0.005,0,25)	3.21	4.28	0.3910	0.9950	0.9800
JPY	FR(0.16,5,0,0)	3.10	4.76	0.3370	0.9760	0.9770
USD	FR(0.035,10,0,0)	1.48	7.50	0.1410	0.8130	0.8310
BB90	FR(0.005,4,0,0)	25.12	1.77	0.9630	0.3370	0.3290
TB3Y	CBO(150,0.03,25,0.005)	42.83	0.55	0.7220	0.7620	0.6110
TB10Y	OBV(1,75,0,0,25)	46.71	0.44	0.7900	0.8130	0.8970

evidence that any of the trading rules outperforms the benchmark. This constitutes the main finding of this paper –that while it is possible to find technical trading rules that have been profitable relative to a benchmark buy-and-hold strategy for some assets over the sample period, once the effects of data snooping are properly accounted for, there is no evidence that any of the 7,846 technical trading rules that we consider outperform the benchmark buy-and-hold strategy.

Table 2 provides the results for each of the subsamples assuming zero transactions costs. As was the case for the full sample, in the subsamples the best trading rule often generated a superior profit to the buy-and-hold benchmark strategy, but the pairwise Diebold-Mariano test rejects the null hypothesis that the superior technical trading rule is no better than the benchmark strategy in only a few cases. Note that there is little consistency across the subsamples and the full sample with respect to the best trading rules for each asset and whether the best trading rule is superior to the benchmark. Furthermore, there is only a single asset in a single subsample for which the Reality Check and Superior Predictive Ability tests reject the null hypothesis that the best technical trading rule beats the benchmark at the 5% significance level (a channel breakout rule for APN in the last subsample). Since for both tests the p-values are greater than 0.01 and, since we have conducted multiple tests

Table 2. P-values by subsample periods

Subsample period 1: Jan. 1993 to Dec. 1997						
	Best rule	Bench	Return	p_{DM}	p_{RC}	p_{SPA}
ASX	MA(75,250,0,5,0)	10.42	16.09	0.131	0.838	0.825
BHP	OBV(250,200,0.05,0,0)	-3.61	7.92	0.122	0.841	0.703
CBA	FR(0.14,4,0,0)	16.42	17.56	0.408	0.998	0.986
WES	SMA(200,15,0.01,0,0)	11.46	12.09	0.464	1.000	0.994
APN	FL(0.14,3,0,0)	6.28	7.87	0.441	0.999	0.993
BPT	OBV(250,75,0,0,25)	-21.59	38.04	0.044	0.793	0.777
PPT	FL(0.25,50,0,0,)	17.96	17.87	0.541	1.000	0.998
GBP	SAR(4,0.015,0,0,25)	-1.31	7.98	0.129	0.713	0.677
JPY	SAR(250,0.01,0,0,5)	-0.53	7.17	0.113	0.691	0.675
USD	MA(25,40,1,0.01)	0.77	5.33	0.054	0.736	0.721
BB90	FR(0.005,2,0,0)	-9.49	1.77	0.966	0.338	0.611
TB3Y	CBO(100,0.03,10,0,0.01)	25.48	0.55	0.557	0.361	0.263

TB10Y	CBO(100,0.15,5,0.01)	39.02	0.33	0.635	0.552	0.519
Subsample period 2: Jan. 1998 to Dec. 2002						
	Best rule	Bench	Return	P _D M	P _R C	P _S P _A
ASX	MA(5,25,25,0)	3.92	13.38	0.144	0.803	0.771
BHP	OBV(15,1,0,0,25)	-3.95	23.59	0.244	0.671	0.911
CBA	CBO(5,0.02,0.01,10)	4.54	6.91	0.434	0.986	0.967
WES	OBV(20,2,0,0.01,0)	17.87	17.78	0.497	1.000	0.992
APN	OBV(150,30,25,0,0)	7.23	26.29	0.151	0.951	0.931
BPT	OBV(15,2,0,0,10)	57.61	85.29	0.219	0.808	0.784
PPT	CBO(10,0.15,0.005,50)	15.46	19.96	0.399	0.998	0.992
GBP	CBO(250,0.075,5,0.005)	-0.14	12.26	0.046	0.719	0.663
JPY	SAR(25,3,0,0,50)	-1.26	9.17	0.129	0.711	0.689
USD	FR(0.02,20,0,0)	-3.47	12.34	0.089	0.626	0.853
BB90	FR(0.005,2,0,0)	-6.71	0.3	0.243	0.314	0.235
TB3Y	CBO(100,0.03,10,0.01)	-24.91	0.37	0.417	0.25	0.138
TB10Y	CBO(100,0.15,5,0.01)	73.05	0.38	0.731	0.114	0.129
Subsample period 3: Jan. 2003 to Dec. 2007						
	Best rule	Bench	Return	P _D M	P _R C	P _S P _A
ASX	CBO(5,0.02,5,0.01)	15.11	16.72	0.621	1.000	1.000
BHP	FR(0.2,5,0,0,)	4.25	4.77	0.461	0.995	0.973
CBA	SMA(250,125,0,0,50)	17.36	16.73	0.541	1.000	0.998
WES	OBV(100,10,0,0,0,)	10.98	10.36	0.535	1.000	0.997
APN	CBO(150,0.15,0.005,25)	6.98	5.46	0.616	1.000	0.990
BPT	OBV(75,1,0,0,50)	36.42	44.89	0.371	1.000	1.000
PPT	SMA(250,100,0,3,0)	10.93	15.66	0.234	0.995	0.982
GBP	SAR(20,0,0.015,0,5)	5.64	4.86	0.315	0.993	0.981
JPY	SAR(100,0.02,0,10)	9.37	8.65	0.278	0.988	0.978
USD	OBV(20,150,0.05,0,1)	8.16	10.58	0.267	0.973	0.969
BB90	FR(0.005,2,0,0)	-114.53	0.11	0.028	0.067	0.057
TB3Y	CBO(100,0.03,10,0.01)	-75.68	0.27	0.194	0.210	0.166
TB10Y	CBO(100,0.15,5,0.01)	-40.86	0.38	0.335	0.307	0.392
Subsample period 4: Jan. 2008 to Dec. 2012						
	Best rule	Bench	Return	P _D M	P _R C	P _S P _A
ASX	FR(0.01,10,0,0)	-3.68	19.92	0.122	0.873	0.868
BHP	FR(0.01,15,0,0)	6.25	18.57	0.223	0.517	0.503
CBA	FR(0.4,0,0,0)	20.55	19.63	0.593	0.999	0.999
WES	FR(0.2,10,0,0)	17.86	18.17	0.488	0.999	0.99
APN	SMA(10,2,0,0,5)	-56.61	62.60	0.002	0.027	0.032
BPT	FR(0.14,2,0,0,)	11.46	24.72	0.224	0.946	0.928
PPT	OBV(100,30,0,4,0)	0.39	22.97	0.195	0.927	0.918
GBP	CBO(100,0.075,5,0.001)	8.65	10.61	0.461	1.000	0.999
JPY	SAR(250,0,0.05,0,10)	4.82	11.63	0.498	1.000	1.000
USD	FR(0.015,15,0,0)	0.46	22.22	0.132	0.891	0.853
BB90	SAR(20,0,0.03,0,25)	17.25	0.58	0.571	0.517	0.458
TB3Y	CBO(100,0.03,10,0.01)	52.00	0.5	0.649	0.543	0.461
TB10Y	CBO(100,0.1,5,0.02)	73.05	0.38	0.731	0.689	0.643

for multiple assets, we do not interpret the result as evidence in favour of technical trading rule profitability relative to the benchmark strategy.

5. Conclusions

By considering 7,846 different technical trading rules applied to 13 different Australian financial assets, the research reported in this paper provides a far more comprehensive consideration of the profitability of technical trading rules in the Australian markets than is available in the prior literature. Nonetheless, our results are consistent with prior studies, both for Australian markets and for those of other countries, that have either found no evidence of profitability (e.g., Ball, 1978; Batten & Ellis, 1996; Ellis & Parbery, 2005; Loh, 2004; Marshall et

al., 2008) or have found some evidence, but not in recent time periods (e.g., Olson, 2004; Hawtrey & Nguyen, 2006; Shynkevich, 2012; Qi & Wu, 2006; Sullivan et al., 1999).

For each asset that we considered, we were able to find a technical trading rule that provided a superior profit to the buy-and-hold strategy in at least one sub-sample or the whole sample. This may be the reason that technical trading rules continue to be used widely in the Australian finance industry. Nonetheless, as the results presented above show, for the cases that we have considered, once the range of models that one must search to find profitable rules has been properly accounted for in the construction of statistical tests, there is no statistically significant evidence that technical trading rules generate superior returns to a simple buy-and-hold strategy at conventional levels of significance in the Australian markets.

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Notes

Note 1. See also Section 3 of Menkhoff (2007) for a review of similar surveys.

Note 2. The set of rules that we consider is that used by Sullivan et al. (1999) in their analysis of the Dow Jones Industrial Index.

Note 3. See also Section 4 of Menkhoff (2007).

Note 4. The family-wise error rate is defined as the probability of rejecting at least one true null hypothesis in a set of multiple hypothesis tests.

Note 5. <https://forms.thomsonreuters.com/datastream/>.

Note 6. Note that the small-cap stocks that we consider are not available for short-selling on the ASX. However, at the time of writing, there exist private firms that offer contracts for difference which allow an investment equivalent to a short position on these stocks to be held

Note 7. See their Appendix A for a list of all the parameter combinations considered.

Note 8. The moving average of order n is the arithmetic mean of the closing prices from the previous n days including the current day.

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