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Title	Testing the predictive ability of technical analysis using a new stepwise test without data snooping bias
Author(s)	Hsu, PH; Hsu, YC; Kuan, CM
Citation	Journal Of Empirical Finance, 2010, v. 17 n. 3, p. 471-484
Issued Date	2010
URL	http://hdl.handle.net/10722/141768
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Testing the Predictive Ability of Technical Analysis Using A New Stepwise Test without Data Snooping Bias

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This version: July 20, 2009

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†† We thank the editor, Christian C. P. Wolff, and two anonymous referees for their useful comments and suggestions. We are grateful for the comments on early versions of this paper by Zongwu Cai, Stephen Donald, Peter R. Hansen, Joel Hasbrouck, Shantaram Hegde, Yingyao Hu, Robert Lieli, Chris Neely, Pedro Saffi, Michael Wolf, Yangru Wu, and Jialin Yu. We also thank the participants of the Inaugural Conference of the Society for Financial Econometrics, Financial Management Association Meeting, North American Summer Meeting of the Econometric Society, NTU IEFA Conference, and the seminars in Columbia University, National Central University, National Taiwan University, National University of Singapore, University of North Carolina at Charlotte, and University of Rochester. P.-H. Hsu is especially indebted to Andrew Ang, Charles M. Jones, and John Donaldson for their guidance during his doctoral study at the Graduate School of Business of Columbia University. C.-M. Kuan thanks the NSC of Taiwan for research support (97–2410–H–002–217–MY3). All errors remain ours.

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Abstract

In the finance literature, statistical inferences for large-scale testing problems usually suffer from data snooping bias. In this paper we extend the "superior predictive ability" (SPA) test of Hansen (2005, *JBES*) to a stepwise SPA test that can identify predictive models without potential data snooping bias. It is shown analytically and by simulations that the stepwise SPA test is more powerful than the stepwise Reality Check test of Romano and Wolf (2005, *Econometrica*). We then apply the proposed test to examine the predictive ability of technical trading rules based on the data of growth and emerging market indices and their exchange traded funds (ETFs). It is found that technical trading rules have significant predictive power for these markets, yet such evidence weakens after the ETFs are introduced.

JEL classification: C12, C32, C52, G11

Keywords: data snooping, exchange traded funds, reality check, SPA test, stepwise test, technical trading rules

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1 Introduction

Technical analysis has been widely applied in stock markets since W. P. Hamilton wrote a series of articles in *The Wall Street Journal* in 1902. Its predictive power (or profitability), however, remains a long-debated issue in both industry and academia. A recent article in *The Wall Street Journal* observes: "Some brokerage firms have eliminated their technical research departments altogether. Still, when the markets begin to sag, investors rediscover technical analysis" (Browning, July 30, 2007). Indeed, the same article reports that some technical analysts did foresee and warn their clients right before the stock market plunge on July 23, 2007. There are also numerous empirical results in the literature that support technical analysis, such as Sweeney (1988), Blume, Easley, and O'Hara (1994), Brown, Goetzmann, and Kumar (1998), Gencay (1998), Lo, Mamaysky, and Wang (2000), and Savin, Weller, and Zvingelis (2007). Such evidences, however, may be criticized for their data snooping bias; see, e.g., Lo and MacKinlay (1990) and Brock, Lakonishok, and LeBaron (1992).

Data snooping is common in the finance and economics literature. In practice, only a few financial data sets are available for empirical examination. Data snooping arises when researchers rely on the same data set to test the significance of different models (technical trading rules) individually. As these individual statistics are generated from the same data set and hence related to each other, it is difficult to construct a proper joint test, especially when the number of models (rules) being tested is large. White (2000) proposes a large-scale joint testing method for data snooping, also known as Reality Check (RC), which takes into account the dependence of individual statistics. Sullivan, Timmermann, and White (1999) apply the RC test and find that technical trading rules lose their predictive power for major U.S. stock indices after the mid 80's.

White's RC test suffers from two drawbacks. First, Hansen (2005) points out that the RC test is conservative because its null distribution is obtained under the least favorable configuration, i.e., the configuration that is least favorable to the alternative. In fact, the RC test may lose power dramatically when many poor models are included in the same test. To improve on the power property of the RC test, Hansen (2005) proposes the "superior predictive ability" (SPA) test that avoids the least favorable configuration. Empirical studies, such as Hansen and Lunde (2005) and Hsu and Kuan (2005), also show

that the SPA test is more powerful than the RC test. Second, the RC test checks whether there is any significant model but does not identify all such models. Note that Hansen's SPA test shares the same limitation. Romano and Wolf (2005) introduce a RC-based stepwise test, henceforth Step-RC test, that is capable of identifying as many significant models as possible. Nonetheless, Romano and Wolf's Step-RC test is conservative because its stepwise critical values are still determined by the least favorable configuration, as in the original RC test.

In this paper, the SPA test is further extended to a stepwise SPA (Step-SPA) test that can identify predictive models in large-scale, multiple testing problems without data snooping bias. This is analogous to the extension of White's RC test to Romano and Wolf's Step-RC test. It is shown that the Step-SPA test is consistent, in the sense that it can identify the violated null hypotheses (models or rules) with probability approaching one, and its familywise error (FWE) rate can be asymptotically controlled at any pre-specified level, where FWE rate is defined as the probability of rejecting at least one correct null hypothesis. This paper makes additional contribution by showing analytically and by simulations that the Step-SPA test is more powerful than the Step-RC test, under any power criterion defined in Romano and Wolf (2005).

In our empirical study, the proposed Step-SPA test is applied to evaluate the predictive power of 9,120 moving average rules and 7,260 filter rules in several growth and emerging markets. Unlike many existing studies on technical analysis, we examine not only market indices but also their corresponding Exchange Traded Funds (ETFs). Considering ETFs is practically relevant because ETFs have been important investment vehicles since their inception in late 90's. Moreover, due to the tradability and low transaction costs, ETFs help to increase market liquidity and hence may improve market efficiency (e.g. Hegde and McDermott, 2004). Our empirical study thus enables us to assess whether the predictive power of technical rules, if any, is affected after ETFs are introduced.

Our empirical results provide strong evidence that technical rules have significant predictive ability in pre-ETF periods, yet such evidence weakens in post-ETF periods. In particular, we find many technical rules with significant predictive power prior to the inception of ETFs in U.S. growth markets but *none* when the ETFs that track these

market indices become available. For emerging markets, we find technical rules have predictive ability for 4 (out of 6) index returns but for only 2 ETF returns. For these two predictable ETFs, far fewer rules with significant predictive power can be identified by the proposed stepwise test. The high break-even transaction costs associated with the top rules in those predictable ETFs further suggest that some technical rules may be exploited to make profit in certain emerging markets. Our findings therefore indicate a negative impact of the inception of ETFs on the predictive ability of technical trading rules. This is compatible with the intuition that ETFs allow arbitrageurs to trade away most potential profits in young markets.

To summarize, this paper makes the following contributions to the literature. First, we develop a new test for empirical testing problems in finance that require correction of data snooping bias. Second, we provide new evidence of technical predictability (and potential profitability) of growth and emerging stock markets based on recently available data of ETFs. Last, but not least, this study supports the adaptive market efficiency hypothesis of Lo (2004). Using technical predictability as a barometer of market efficiency, our results suggest that the existence of ETFs effectively improves market efficiency.[1]

This paper proceeds as follows. We summarize the existing tests and introduce the Step-SPA test in Section 2. The simulation results for the Step-SPA test are reported in Section 3. The data and performance measures are discussed in Section 4. The empirical results are presented in Section 5. Section 6 concludes the paper. The proofs and some details of the technical rules considered in the paper are deferred to Appendices.

2 Tests without Data Snooping Bias

Given m models for some variable, let $d_{k,t}$ ($k = 1, 2, ..., m$ and $t = 1, 2, ..., n$) denote their performance measures (relative to a benchmark model) over time. Suppose that for each k, $\mathbb{E}(d_{k,t}) = \mu_k$ for all t, and for each t, $d_{k,t}$ may be dependent across k. We wish to determine whether these models can outperform the benchmark and would like to test the following inequality constraints:

$$H_0^k : \mu_k \leq 0, \quad k = 1, ..., m. \tag{1}$$

[1] Neely, Weller, and Ulrich (2007) also suggest that the weakening technical predictability in foreign exchange markets can be explained by the adaptive market efficiency hypothesis.

For example, we may test if there is any technical trading rule that can generate positive return for an asset. Let r_t be the return of this asset at time t and $\delta_{k,t-1}$ be the trading signal generated by the k-th trading rule at time $t-1$, which takes the values of 1, 0, or -1, corresponding to a long position, no position, and a short position, respectively. Then, $d_{k,t} = \delta_{k,t-1} r_t$ is the realized return of the k-th trading rule, and (1) is the hypothesis that *no* trading rule can generate positive mean return. Note that $d_{k,t}$ depend on each other because they are based on the same return r_t.

Following Hansen (2005), we impose the following condition on $\boldsymbol{d}_t = (d_{1,t}, ..., d_{m,t})'$ which allows $\boldsymbol{d}_t$ to exhibit weak dependence over time.

Assumption 2.1 $\{\boldsymbol{d}_t\}$ is strictly stationary and α-mixing of size $-(2+\eta)(r+\eta)/(r-2)$, for some $r > 2$ and $\eta > 0$, where $E|\boldsymbol{d}_t|^{(r+\eta)} < \infty$ with $|\cdot|$ the Euclidean norm, and $\mathrm{var}(d_{k,t}) > 0$ for all k.

Under this condition, the data obey a central limit theorem:

$$\sqrt{n}(\bar{\boldsymbol{d}} - \boldsymbol{\mu}) \xrightarrow{D} \mathcal{N}(\mathbf{0}, \boldsymbol{\Omega}), \tag{2}$$

where $\bar{\boldsymbol{d}} = n^{-1} \sum_{t=1}^{n} \boldsymbol{d}_t$, $\boldsymbol{\mu} = \mathbb{E}(\boldsymbol{d}_t)$, $\boldsymbol{\Omega} \equiv \lim_{n\to\infty} \mathrm{var}(\sqrt{n}(\bar{\boldsymbol{d}} - \boldsymbol{\mu}))$, and $\xrightarrow{D}$ stands for convergence in distribution. Moreover, Assumption 2.1 ensures the validity of the stationary bootstrapping procedure and the consistency of the covariance matrix estimator of Politis and Romano (1994).

2.1 Existing Tests

Data snooping arises when the inference for (1) is drawn from the test of an individual hypothesis H_0^k. One may circumvent the problem by controlling the significance level of each individual test based on the Bonferroni inequality. This approach is, however, not practically useful when the number of hypotheses, m, is large. In many applications, m is typically very large; for example, Sullivan et al. (1999) evaluate 7,846 technical trading rules, and Hsu and Kuan (2005) study a total of 39,832 simple technical rules and complex trading strategies.

Alternatively, one may conduct a joint test of (1) with a properly controlled significance level. A leading example is the RC test of White (2000) with the statistic:

$$\mathrm{RC}_n = \max_{k=1,...,m} \sqrt{n}\bar{d}_k,$$

where $\bar{d}_k$ is the k-th element of $\bar{\boldsymbol{d}}$. Given that (1) is a collection of composite hypothesis, White (2000) chooses the least favorable configuration (LFC), i.e., $\boldsymbol{\mu} = \mathbf{0}$, to obtain the null distribution. It follows from (2) that $\sqrt{n}\bar{\boldsymbol{d}} \xrightarrow{D} \mathcal{N}(\mathbf{0}, \boldsymbol{\Omega})$ under the LFC. The limiting distribution of RC_n is thus $\max\{\mathcal{N}(\mathbf{0}, \boldsymbol{\Omega})\}$ which may be approximated via a (stationary) bootstrap procedure. The null hypothesis (1) would be rejected when the bootstrapped p-value is smaller than a pre-specified significance level or, equivalently, when the test statistic RC_n is greater than the bootstrapped critical value.

The LFC of the RC test is convenient but also renders this test relatively conservative. Hansen (2005) shows that under the null, when there are some $\mu_i < 0$ and at least one $\mu_i = 0$, $\mathrm{RC}_n \xrightarrow{D} \max\{\mathcal{N}(\mathbf{0}, \boldsymbol{\Omega}_0)\}$, where $\boldsymbol{\Omega}_0$ is a sub-matrix of $\boldsymbol{\Omega}$ with the j-th row and j-th column of $\boldsymbol{\Omega}$ deleted when $\mu_j < 0$. That is, the limiting distribution depends only on the models with a zero mean but not on those poor models (i.e., the models with a negative mean). It is also conceivable that including very poor models may artificially increase the empirical p-value of the RC test. This motivates the SPA test of Hansen (2005) with the statistic:

$$\mathrm{SPA}_n = \max\Big(\max_{k=1,\ldots,m} \sqrt{n}\bar{d}_k,\ 0 \Big),$$

which is virtually the same as the RC test.

A novel feature of the SPA test is that it avoids the LFC by re-centering the null distribution, as described below. Let $\widehat{\boldsymbol{\Omega}}$ denote a consistent estimator for $\boldsymbol{\Omega}$ with the (i,j)-th element $\hat{\omega}_{ij}$. Also let $\hat{\sigma}_k^2 \equiv \hat{\omega}_{kk}$ and $A_{n,k} = -\hat{\sigma}_k \sqrt{2 \log\log n}$. We define $\hat{\boldsymbol{\mu}}$ as the vector with the k-th element:

$$\hat{\mu}_k = \bar{d}_k \mathbf{1}\big(\sqrt{n}\bar{d}_k \leq A_{n,k}\big),$$

where $\mathbf{1}(B)$ denotes the indicator function of the event B. It can be seen that $\hat{\mu}_k = 0$ almost surely when $\mu_k = 0$. Moreover, when $\mu_k < 0$, $\sqrt{n}\bar{d}_k \leq A_{n,k}$ with probability approaching one, so that $\hat{\mu}_k$ converges in probability to μ_k. Noting that $\sqrt{n}\bar{\boldsymbol{d}} = \sqrt{n}(\bar{\boldsymbol{d}} - \boldsymbol{\mu}) + \sqrt{n}\boldsymbol{\mu}$, Hansen (2005) suggests to add $\sqrt{n}\hat{\boldsymbol{\mu}}$ to the bootstrapped distribution of $\sqrt{n}(\bar{\boldsymbol{d}} - \boldsymbol{\mu})$. Re-centering the bootstrapped distribution thus yields a better approximation to the null distribution of SPA_n: $\max\{\mathcal{N}(\mathbf{0}, \boldsymbol{\Omega}_0), 0\}$. The SPA test is a more powerful test than the RC test because the bootstrapped SPA p-value is smaller than the corresponding RC p-value.

Another drawback of the RC test is that it does not identify all models that significantly deviate from the null hypothesis. Rejecting the null hypothesis by the RC test only suggests that there exists at least one model with $\mu_k > 0$. Basing on the RC test, Romano and Wolf (2005) propose a stepwise procedure that can identify as many models with $\mu_k > 0$ as possible, while asymptotically controlling the FWE rate, the probability of rejecting at least one of the correct hypotheses. This test, also known as the Step-RC test, is practically more useful than the RC test. For example, a fund-of-fund manager ought to be more interested in finding out the funds that can beat the benchmark, rather than just knowing the best performed fund.

To implement the stepwise procedure, we re-arrange $\bar{d}_k$ in a descending order. A top model k would be rejected if $\sqrt{n}\bar{d}_k$ is greater than the bootstrapped critical value, where bootstrapping is computed as in the RC test. If none of the null hypotheses is rejected, the process stops; otherwise, we remove $\bar{d}_k$ of the rejected models from the data and bootstrap the critical value again using the remaining data. In the new sample, a top model i would be rejected if $\sqrt{n}\bar{d}_i$ are greater than the newly bootstrapped critical value. The procedure continues until no more model can be rejected. Note that Hansen (2005) and Romano and Wolf (2005) also suggest that using studentized statistics, $\sqrt{n}\bar{d}_k/\hat{\sigma}_k$, would render the test more powerful. To ease the expression, our discussion below is still based on non-studentized statistics.

2.2 The Stepwise SPA Test

Analogous to the extension from the RC test to the Step-RC test, it is natural to extend the SPA test to the Step-SPA test. The stepwise procedure enables us to identify significant models, as in the Step-RC test, yet it ought to be more powerful because its null distribution does not depend on the LFC. The proposed Step-SPA test is based on the following statistics: $\sqrt{n}\bar{d}_1, \ldots, \sqrt{n}\bar{d}_m$, and a stepwise procedure analogous to that of the Step-RC test.

For the Step-SPA test, we adopt the stationary bootstrap of Politis and Romano (1994) which is computed as follows. Let $\boldsymbol{d}_t^*(b) \equiv \boldsymbol{d}_{n_{b,t}}^*$, $t = 1, \ldots, n$, be the b-th re-sample of $\boldsymbol{d}_t$, where the indices $n_{b,1}, ..., n_{b,n}$ consist of blocks of $\{1, ..., n\}$ with random lengths determined by the realization of a geometric distribution with the parameter $Q \in [0, 1)$.

First, $n_{b,1}$ is randomly chosen from $\{1, ..., n\}$ with an equal probability assigned to each number. Second, for any $t > 1$, $n_{b,t} = n_{b,t-1} + 1$ with probability Q;[2] otherwise, $n_{b,t}$ is chosen randomly from $\{1, ..., n\}$. A re-sample is done when n observations are drawn; let $\bar{\boldsymbol{d}}^*(b) = \sum_{t=1}^{n} \boldsymbol{d}_t^*(b)/n$ denote the sample average of this re-sample. Repeating this procedure B times yields an empirical distribution of $\bar{\boldsymbol{d}}^*$ with B realizations. Given the pre-specified level α_0, the bootstrapped SPA critical value is determined as

$$\hat{q}^*_{\alpha_0} = \max(\hat{q}_{\alpha_0},\ 0), \tag{3}$$

with $\hat{q}_{\alpha_0} = \inf\{q \mid \mathrm{P}^*[\sqrt{n}\max_{k=1,...,m}(\bar{d}^*_k - \bar{d}_k + \hat{\mu}_k) \leq q] \geq 1 - \alpha_0\}$, the $(1-\alpha_0)$-th quantile of the re-centered empirical distribution, and P^* is the bootstrapped probability measure.

The Step-SPA test with the pre-specified level α_0 then proceeds as follows.

1. Re-arrange $\bar{d}_k$ in a descending order.

2. Reject the top model k if $\sqrt{n}\bar{d}_k$ is greater than $\hat{q}^*_{\alpha_0}$(all), the critical value bootstrapped as in (3) using the complete sample. If no model can be rejected, the procedure stops; otherwise, go to next step.

3. Remove $\bar{d}_k$ of the rejected models from the data. Reject the top model i in the sub-sample of remaining observations if $\sqrt{n}\bar{d}_i$ is greater than $\hat{q}^*_{\alpha_0}$(sub), the critical value bootstrapped as in (3) from the sub-sample. If no model can be rejected, the procedure stops; otherwise, go to next step.

4. Repeat the third step till no model can be rejected.

When the critical values in the procedure above are bootstrapped as in the RC test, we obtain a version of Step-RC test, which is in the spirit of Romano and Wolf (2005) but implemented differently.[3]

[2] If $n_{b,t-1} = n$, we use the wrap-up procedure and set $n_{b,t} = 1$.

[3] The original Step-RC test of Romano and Wolf (2005) differs from our procedure in the following ways. First, they use circular block bootstrap, rather than stationary bootstrap. Second, they rely on a data-dependent algorithm to determine the block size of bootstrap, rather than using an *ex ante* fixed value. Third, they use bootstrapped standard errors, rather than heteroskedasticity and autocorrelation consistent (HAC) estimators based on sample data; see also footnote 5. These differences may affect the finite sample performance of the Step-RC test.

The results below show that the Step-SPA test is consistent while asymptotically controlling the FWE rate at a pre-specified level, analogous to Theorem 4.1 of Romano and Wolf (2005). All proofs of theorems are collected in Appendix A.

Theorem 2.2 *The following results hold under Assumption 2.1 and* $\alpha_0 < 1/2$.

1. *The hypothesis* H_0^k *with* $\mu_k > 0$ *will be rejected by the Step-SPA test with probability approaching 1 when* n *tends to infinity.*

2. *Given the pre-specified level* α_0*, the FWE rate of the Step-SPA test is* α_0 *when* n *tends to infinity if and only if there is at least one* $\mu_k = 0$.

Note that the FWE rate of the Step-RC test is less than or equal to α_0, in contrast with the second result above. This is due to the fact that the RC test relies on the LFC and hence yields a conservative test. If there is no $\mu_k = 0$, it can also be shown that the FWE rate is zero asymptotically,[4] so that no null hypothesis will be incorrectly rejected.

As far as power is concerned, our key result below shows that the Step-SPA test is superior than the Step-RC test.

Theorem 2.3 *Given Assumption 2.1, the Step-SPA test is more powerful than the Step-RC test under the notions of power defined in Romano and Wolf (2005).*

3 Simulations

In this section, we evaluate the finite-sample performance of the Step-SPA and Step-RC tests using Monte Carlo simulations. We are mainly concerned with the FWE rate and the rejection frequency of the models with significant returns. This is similar to the examination of the empirical level and power of a test.

We first generate m return series:

$$x_{i,t} = c_i + \gamma x_{i,t-1} + \epsilon_{i,t}, \quad i = 1, \ldots, m, \quad t = 1, \ldots, T$$

[4]When $\mu_k < 0$, $\sqrt{n}\bar{d}_k$ would diverge to negative infinity in probability. Given that the critical value is always non-negative, H_k^0 would be rejected with probability approaching zero, which implies that FWE rate is zero in the limit.

where $\epsilon_{i,t}$ are i.i.d. noises distributed as $\mathcal{N}(0, \sigma^2)$, c_i and γ are parameters such that c_i is a constant a for $i = 1, \ldots, m_1$, $c_i = 0$ for $i = m_1 + 1, \ldots, m_1 + m_2$, and $c_i = -a$ for $i = m_1 + m_2 + 1, \ldots, m$. We first set $a = 0.0008$ (8 basis points), $\gamma = 0.01$, and $\sigma = 0.005$. Thus, each sample contains m_1 "outperforming" returns that have a positive mean $0.0008/0.99 = 0.00081$, m_2 "neutral" returns with a zero mean, and $m - m_1 - m_2$ "poor" returns with a negative mean -0.00081. The numbers of return series are $m = 90$, 900 and 9000, the sample size is $n = 1000$, and the number of simulation replications is $R = 500$. For each m, there are three cases: (1) $m_1 = m_2 = m/3$ so that there are 3 equal groups of returns, (2) $m_1 = m_2 = m/9$ so that there are unequal groups of returns with a much larger group of "poor" returns, and (3) $m_2 = m$ so that there are only neutral returns. In the stationary bootstrap, we set the number of bootstraps $B = 500$ and the parameter of the geometric distribution $Q = 0.9$. To estimate the covariance matrix Ω_0, we use the consistent estimator of Politis and Romano (1994) in the simulations and subsequent empirical study.[5]

The simulation results based on non-studentized and studentized statistics are summarized in Tables 1 and 2, respectively. Here, $\bar{d}_k = \bar{x}_k$, the sample average of the k-th return series. For Table 1, the k-th return would be rejected if $\sqrt{n}\bar{x}_k$ is greater than the 5% bootstrapped critical value. For Table 2, we base the test decisions on studentized statistics $\sqrt{n}\bar{x}_k/\hat{\sigma}_k$, where $\hat{\sigma}_k$ is as discussed in Section 2.1 and computed from the sample data.[6] In each replication, the rejection rate of these tests is the number of correctly rejected returns divided by m_1, the total number of outperforming returns. Averaging these rejection rates over R, the number of replications, yields the average rejection (AR)

[5]Following Hansen (2005), the following estimator due to Politis and Romano (1994) is used:

$$\hat{\Omega} = \hat{\Omega}_0 + \sum_{j=1}^{n-1} \kappa(j, n)[\hat{\Omega}_j + \hat{\Omega}_j'],$$

where $\hat{\Omega}_j = n^{-1} \sum_{t=j+1}^{n} (d_t - \bar{d})(d_{t-j} - \bar{d})'$ and the weight function $\kappa(j, n)$ is defined as

$$\kappa(j, n) \equiv \frac{n - j}{n}(1 - Q)^j + \frac{j}{n}(1 - Q)^{n-j},$$

where Q is the parameter of the geometric distribution. This HAC estimator is similar to that of Newey and West (1987) but with a different weight function.

[6]For the studentized method, the bootstrapped statistics are computed as $\sqrt{n}\bar{x}_k^*/\hat{\sigma}_k$, $k = 1, \ldots, m$. That is, $\hat{\sigma}_k$ from the sample data is also used in bootstrap. One could also compute the bootstrapped statistics as $\sqrt{n}\bar{x}_k^*/\hat{\sigma}_k^*$, with $\hat{\sigma}_k^*$ estimated from bootstrapped samples; see, e.g., Romano and Wolf (2005).

rate, which is also the "average power" defined in Romano and Wolf (2005). The FWE rate is computed as the relative frequency of the replications in which at least one neutral or poor return is incorrectly rejected. In Tables 1 and 2 we report the average rejection rates in the first step, the average rejection rates in all steps, and the FWE rates of the Step-SPA and Step-RC tests. Note that case 3 contains only the FWE rates, because there is no outperforming return ($m_1 = 0$).

Table 1 shows that, for the first two cases, the FWE rates of the Step-SPA test are controlled properly and closer to the nominal level 5% than those of the Stpe-RC test. Note that case 3 is exactly the LFC considered by the RC test. Thus, it is not surprising to see that the Step-SPA and Step-RC tests have the same FWE rates in this case (the last column of Table 1) becasue the Step-SPA test has no advantage here. Moreover, we find that the FWE rate of the Step-SPA test is much smaller than 5% when m is large, but it is closer to 5% when $m = 90$. This shows that the second result of Theorem 2.2 is relevant in finite samples when m is small relative to the sample size n. When m is too large, the test becomes more conservative.

Moreover, we can see that the Step-SPA test is more powerful than the Step-RC test in terms of average rejection rate. For each m, the improvement of the Step-SPA test over the Step-RC test is greater when there are unequal groups of returns (with a larger number of poor models). Such improvement becomes more significant when m is large. The greatest improvement is about 16% (81.4% for the Step-SPA test vs. 65.4% for the Step-RC test) which occurs for $m = 9000$ with unequal groups of returns. All the results support the argument of Hansen (2005) that the RC test is adversely affected by the number of poor models included in the test. It is also clear that the stepwise procedure does identify more significant returns than its one-step counterpart. Yet, the power gain is quite marginal for the Step-RC test. For example, when $m = 900$ with unequal groups of returns, further steps of the Step-SPA test identify 2.3% more significant returns (91.9% vs. 94.2%), whereas the Step-RC test only finds extra 0.6% significant returns (83.8% vs. 84.4%).

From Table 2 it is readily seen that the Step-SPA and Step-RC tests are marginally improved when studentized statistics are used and that all the conclusions based on Table 1 carry over. We also note that these simulation results are quite robust to different

a values for c_i. In Figure 1, we plot the average rejection rates and FWE rates for $a = 0.0005, 0.00055, \ldots, 0.001$ and $m = 90$, 900, and 9000 with unequal groups of returns. We can see that the rejection frequencies and FWE rates increase with a; that is, these tests reject the null more easily when the return has a larger mean. More importantly, the Step-SPA test uniformly dominates the Step-RC test across a values in all 3 panels of Figure 1. Similar findings are obtained in unreported simulations with various settings, such as correlated ϵ_i and different γ. All the results support the theoretical properties established in Section 2.2 and unambiguously indicate that the Step-SPA test ought to be preferred to the Step-RC test in practice.

4 Empirical Data and Performance Measures

In our empirical study, we evaluate the predictive ability of technical trading rules based on the data of market indices and corresponding ETFs. When an index is found to be predictable, one may question whether it can be easily traded by (U.S.) investors. This concern is practically relevant, especially for the indices of emerging markets, but it can be mitigated to a large extent when ETFs are available. Indeed, ETFs have been powerful investment tools for arbitrageurs and hedge funds because they track market indices closely and can be conveniently traded at low transaction costs. Thus, it makes practical sense to also examine the predictability of ETFs.

4.1 Index and ETF Data

We consider three indices of U.S. growth markets: S&P SmallCap 600/Citigroup Growth Index (SP600SG), Russell 2000 Index (RUT2000), and NASDAQ Composite Index (NASDAQ), and the ETFs that track these indices: SmallCap 600 Growth Index Fund (IJT), Russell 2000 Index Fund (IWM), and NASDAQ Composite Index Tracking Fund (ONEQ). We also consider the indices of six emerging markets, including MSCI Emerging Markets Index, MSCI Brazil Index, MSCI South Korea Index, MSCI Malaysia Index, MSCI Mexico Index, and MSCI Taiwan Index.[7] The corresponding ETFs are: MSCI Emerging

[7]Note that the MSCI indices are evaluated in U.S. dollars and reflect the holding returns on these markets for U.S. investors. These MSCI indices are important references to institutional investors, and they mitigate the liquidity and tradability issues in emerging markets because they include only investable larger stocks (Chang, Lima, and Tabak, 2004).

Markets Index Fund (EEM), MSCI Brazil Index Fund (EWZ), MSCI South Korea Index Fund (EWY), MSCI Malaysia Index Fund (EWM), MSCI Mexico Index Fund (EWW), and MSCI Taiwan Index Fund (EWT). All ETFs are issued by iShares, except that NASDAQ Composite Index Tracking Fund is issued by Fidelity.

The SP600SG and all MSCI indices are taken from Global Insight, while the other two U.S. indices and all ETFs are taken from Yahoo Finance with dividend adjustment. These data are partitioned into pre- and post-ETF periods, i.e., the periods before and after the inception of the corresponding ETF. Table 3 summarizes the pre-ETF periods for all indices (upper panel), the post-ETF periods for all ETFs up to the end of the year 2005 (lower panel), and the inception dates of the ETFs. All pre-ETF periods have more than 2,000 observations, yet the numbers of observations in post-ETF periods are quite different. For example, MSCI Malaysia and Mexico Index Funds have more than 2,000 observations, but NASDAQ Composite Index Tracking Fund and MSCI Emerging Markets Index Fund have only 508 and 566 observations, respectively.

Table 4 contains the descriptive statistics of daily holding returns on the indices and ETFs considered in the paper. We can see that, for U.S. markets, NASDAQ Composite Index yields the highest daily return (7.4 basis points) and the largest standard deviation in the pre-ETF period, but its ETF has the smallest mean and standard deviation in the post-ETF period. For emerging markets, MSCI Mexico Index enjoys the largest mean return of 12.5 basis points in the pre-ETF period, and the MSCI South Korea Index Fund has the largest mean return of 9.3 basis points in the post-ETF period. The Ljung-Box Q statistics indicate that, at 5% level, all index returns have significant first-order autocorrelations, and all ETFs but MSCI Taiwan Index Fund have insignificant autocorrelations. Moreover, all index and ETF returns are leptokurtic; in particular, the index and ETF returns for Malaysia and Mexico have very large kurtosis coefficients.

4.2 Technical Trading Rules and Performance Measures

We study two leading classes of technical trading rules: moving averages (MA) rules and filter rules (FR). There is a total of 16,380 rules, among them 9,120 are MA rules and 7,260 are filter rules.[8] The details of all trading rules are summarized in Appendix B. The

[8]These rules encompass 2,049 MA rules and 497 filter rules used in Brock et al. (1992) and Sullivan et al. (1999).

trading signals are generated from the technical rules operated on market indices. The performance of technical rules are evaluated using three performance measures: mean return, Sharpe ratio (Sharpe, 1966 and 1994), and x-statistic (Sweeney, 1986 and 1988).

Specifically, let $\delta_{k,t}$ denote the trading signal generated by the k-th trading rule at the end of time t, where $\delta_{k,t} = 1, 0$, or -1 corresponds to the signal of taking a long, neutral, or short position at time $t+1$. Also let r_t denote the return of an index and r_t^f be a risk free rate.[9] The first performance measure of the k-th trading rule is based on the following mean return:

$$\bar{d}_k^{(1)} = \frac{1}{T}\sum_{t=1}^{T} d_{k,t} = \frac{1}{T}\sum_{t=1}^{T} \ln\big(1 + \delta_{k,t-1}(r_t - r_t^f) - \mathrm{TC}\big), \quad k = 1, \ldots, m,$$

where TC is one unit of transaction cost when there is a buy or sell and TC is zero when no action is taken. The performance measure based on Sharpe ratio is:

$$\bar{d}_k^{(2)} = \frac{1}{T}\sum_{t=1}^{T} \frac{\ln\big(1 + \delta_{k,t-1} r_t - r_t^f - \mathrm{TC}\big)}{\hat{\sigma}_k}, \quad k = 1, \ldots, m,$$

where $\hat{\sigma}_k$ is the estimated standard deviation of the summand in the numerator, $\ln\big(1 + \delta_{k,t-1} r_t - r_t^f - \mathrm{TC}\big)$, based on the examined sample. The measure based on the x-statistic is

$$\bar{d}_k^{(3)} = \bar{d}_k^{(1)} - \left(\frac{\sum_{t=1}^{T} \mathbf{1}(\delta_{k,t-1} = 1)}{T} - \frac{\sum_{t=1}^{T} \mathbf{1}(\delta_{k,t-1} = -1)}{T} \right) \frac{\sum_{t=1}^{T} r_t - r_t^f}{T}, \quad k = 1, \ldots, m,$$

where $\mathbf{1}(A)$ denotes the indicator function of the event A. Note that the third measure can be understood as the measure based on mean return adjusted for a proportion of market risk premium: $\sum_{t=1}^{T}(r_t - r_t^f)/T$. We also consider studentized mean return as a performance measure:

$$\bar{d}_k^{(4)} = \frac{1}{T}\sum_{t=1}^{T} d_{k,t} = \frac{1}{T}\sum_{t=1}^{T} \frac{\ln\big(1 + \delta_{k,t-1}(r_t - r_t^f) - \mathrm{TC}\big)}{\hat{\sigma}_k}, \quad k = 1, \ldots, m,$$

where $\hat{\sigma}_k$ is the estimated standard deviation of the summand of the numerator from the examined sample.

[9] The risk free rate in this study is the effective federal funds rate from Federal Reserve Economic Data (http://research.stlouisfed.org/fred2/). The daily risk free rate $r^f(d)$ is converted from the annual rate $r^f(a)$ as $r^f(d) = \ln(1 + r^f(a))/250$.

What we examine here is the absolute performance of technical rules, because zero is the benchmark in these measures. We could, of course, examine the relative performance by taking the buy-and-hold return as the benchmark. In the empirical study, we impose a one-way transaction cost of 0.05% on each trade of all market indices and ETFs. This choice is based on the literature and personal correspondence with other researchers and industry practitioners.[10] For the U.S. stock markets, the earliest estimate of minimum transaction costs could be Fama and Blume (1966). They point out that the floor traders' costs are roughly 0.05% of asset values one-way. Such costs could be even lower (e.g. Sweeney, 1988). The costs of trading index ETFs in large volume are also known to be very low. Note, however, that the trading cost for the indices of emerging markets could be much larger (e.g. Ratner and Leal, 1999; Chang, Lima, and Tabak, 2004). We still impose 0.05% transaction cost for those trades so as to make all results directly comparable. In addition, we compute the break-even transaction costs, i.e., the transaction cost that eliminates all positive returns or performance (Bessembinder and Chan, 1995). Such costs in effect suggest potential "margins" for profitability in ETF transactions.

5 Predictive Ability of Technical Rules

We apply the Step-SPA test to evaluate the predictive power of technical rules in U.S. growth markets and emerging markets. The rules with significant predictive power will be referred to as significant rules or outperforming rules in what follows. Of particular interest to us is, for each market, whether the predictive power of technical rules may be affected after the ETF is introduced. In the application of this test, we set the number of stationary bootstrap $B = 500$ and the parameter of the geometric distribution $Q = 0.9$, as in Sullivan et al. (1999) and Hsu and Kuan (2005). We also consider $Q = 0.5$ as in Qi and Wu (2006) and obtain similar results; these results are not reported to save space.

[10] We thank Huifeng Chang, Shantaram Hegde, Charles Jones, and Pedro Saffi for useful discussions on this issue.

5.1 U.S. Market Indices and ETFs

The numbers of significant rules in the U.S. growth markets identified by the Step-SPA test are summarized in Table 5.[11] It can be seen that technical rules are quite powerful in predicting U.S. indices in pre-ETF periods, especially for S&P SmallCap 600/Citigroup Growth Index. There are as many as 269 significant rules in terms of mean return, 136 rules in terms of Sharpe ratio, 220 rules in terms of x-statistic, and 230 rules in terms of studentized mean return. Yet, the evidence for the predictability of NASDAQ Composite Index is relatively weaker; there are 33 and 7 significant rules in terms of mean return and studentized mean return, respectively, and there is only one significant rule in terms of Sharpe ratio. For Russell 2000 Index, there are more than 100 significant rules under all four measures. The existence of a "thick" set of outperforming rules under these measures constitutes a strong evidence of the predictability of index returns (Timmermann and Granger, 2004). Note that Hsu and Kuan (2005) also find technical rules may be exploited to predict the indices of relatively young markets (Russell 2000 Index and NASDAQ Composite Index) during the period of 1990–2000.

On the other hand, it is interesting to observe from Table 5 that the predictability of market indices found in pre-ETF periods does not carry over to corresponding ETFs in post-ETF periods. Indeed, the Step-SPA test identifies zero significant rule for all three U.S. ETFs under any performance measure. While MSCI indices usually contain nonsynchronous prices and hence may not be readily tradeable, ETFs are different and can be easily traded at very low transaction cost. Thus, ETFs help to enhance market liquidity and improve market efficiency (Hegde and McDermott, 2004). The result here can be interpreted as (indirect) evidence that market efficiency affects the predictive power of technical rules. More discussions are given in Section 5.3.

5.2 Emerging Market Indices and ETFs

The empirical findings for emerging markets are consistent with those for U.S. growth markets. There are significant rules for 4 out of 6 emerging market indices in pre-ETF periods, namely, MSCI Emerging Markets Index, MSCI Brazil Index, MSCI Malaysia

[11] This table is based on one-way transaction cost of 0.05% on each trade of all market indices and ETFs. We obtained similar results when no transaction cost is imposed.

Index, and MSCI Mexico Index. As far as the number of outperforming rules is concerned, some indices in emerging markets seem to be more predictable than U.S. indices. Taking MSCI Emerging Markets Index in its pre-ETF period as an example, we find as many as 797 significant rules in terms of mean return, 414 rules in terms of Sharpe ratio, and 917 rules in terms of x-statistic. There are also more than 300 significant rules identified for MSCI Mexico Index in its pre-ETF period. These again constitute a "thick" set of trading rules with significant predictive power.

In post-ETF periods, we find significant rules only for 2 out of 6 ETFs: MSCI Malaysia Index Fund and MSCI Mexico Index Fund. Note that MSCI Emerging Markets Index Fund is not predictable, even though there are the most outperforming rules identified for its index before ETF is introduced. Moreover, the numbers of identified significant rules for the two predictable ETFs are far less than those for the corresponding indices. For example, in terms of mean return, we find 559 significant rules for MSCI Mexico Index in its pre-ETF period but only 241 rules for MSCI Mexico Index Fund in the post-ETF period. The number of identified rules also drops from 331 in the pre-ETF period to 285 in the post-ETF period under x-statistic.

Table 6 collects the mean returns, annualized Sharpe ratios, x-statistics, and studentized mean returns of the best rules identified for the market indices and their ETFs. When a best rule is found significant by the Step-SPA test, we compute its break-even transaction cost. We find that the break-even transaction costs for the best identified rules in emerging market ETFs are lower than the costs for corresponding indices, except for Malaysia. This, together with the results in Table 5, again supports the argument that the predictive ability of technical rules may be affected by the introduction of ETFs. It can also be seen that the largest break-even transaction cost may be as large as 66 basis points for mean return, 23 basis points for Sharpe ratios, 67 basis points for x-statistic, and 25 basis points for studentized mean return. These margins are much larger than the transaction cost imposed in this study. Hence, it seems plausible to generate profit from proper technical trading rules.

Another interesting finding is that the predictive power of technical rules need not be a consequence of the serial correlation in the data, in contrast with the viewpoint of Fama and Blume (1966) and Allen and Karjalainen (1999). The Step-SPA test identifies

significant rules for MSCI Malaysia and Mexico Index Funds whose returns are serially uncorrelated, but it does not find any outperforming rules for MSCI Taiwan Index Fund which has significant first-order autocorrelation. Note that the predictable ETFs are two early ETFs (since 1996) that are leptokurtic and with more than 2,000 daily data. As demonstrated in the simulations, the performance of the Step-SPA test is affected by the sample size (relative to the number of models being tested). The fact that the other ETFs have smaller samples may be a reason why the Step-SPA test fails to identify outperforming rules. We also note that the autocorrelation may not be precisely estimated when the data are leptokurtic. This may also explain why some ETFs are predictable even the data do not have significant autocorrelation.

5.3 Discussions

Why can some technical rules predict the stock markets? This is an important albeit tough question for researchers in this field. There are several explanations in the literature. An explanation is due to Fama and Blume (1966) which conjectures that the predictive ability of filter rules is due to serial correlations in the data. Our results on the predictability of some ETFs suggest that it is not necessarily the case. Another explanation is that technical rules in fact capture some information contained in the movements of prices, volumes, and order flows (Treynor and Ferguson, 1985; Brown and Jennings, 1990; Blume, Easley, and O'Hara, 1994; Kavajecz and Odders-White, 2004). The third one argues that market maturity matters (Ready, 2002; Hsu and Kuan, 2005; Qi and Wu, 2006). It is conceivable that there are more arbitrage opportunities in younger markets than in mature ones. When a young market attracts more investors and arbitrageurs, the availability of ETFs allows them to exploit possible profitability using trading rules and eventually trade away all profitability. This is also known as "self-destruction" of profitable trading rules (Timmermann and Granger, 2004) and explains why the predictive power of technical rules weakens when the market becomes more efficient. Our empirical findings support this explanation.[12]

Another practical question follows naturally: Can technical analysts transform the predictive power of technical rules to profit? Although there is no definite answer for this

[12]This explanation can also be related to Lo's (2004) adaptive market efficiency hypothesis and Hong, Torous, and Valkanov's (2007) limited information-processing capacity.

question, we try to discuss the potential profitability of technical rules from different perspectives. The first issue is the availability of the closing prices in our ETF data. There is no guarantee that technical analysts can trade those ETFs at the closing prices; nevertheless, they can always place limit orders to trade in prices close enough to the closing prices. That way, technical analysts also prevent themselves from paying too much for the bid-ask spread by placing market orders. Second, using x-statistic as a performance measure, we have demonstrated that the potential profits from outperforming technical rules exceed associated risk premiums. As a result, with good executions and low transaction costs, the technical analysts in large institutions may be able to make profits in excess of risk premiums. Note, however, that our predictability/profitability findings do not necessarily contradict perfect market efficiency because such predictability and profitability may be attributed to tail risk (e.g. extreme events) and market frictions (e.g. tradability, liquidity, and transaction costs).

6 Concluding Remarks

This paper makes two contributions to the literature. On the methodology side, we propose a new stepwise test (the Step-SPA test) for large-scale multiple testing problems without data snooping bias. This test allows us to identify as many significant rules as possible. Yet it is more powerful than the existing Step-RC test because it avoids a conservative configuration used in the RC test. On the application side, we employ the proposed test to obtain new evidence for the predictive ability of technical trading rules in both growth and emerging markets. Our empirical results are practically informative because they are based not only on market indices but also on ETFs which can be conveniently traded at low transaction costs. It is also worth mentioning that the proposed Step-SPA test is readily applicable to other similar, multiple testing problems, such as the performance of mutual funds (hedge funds), the performance of corporate managers, and the forecasting ability of different econometric models.

Appendix A: Proof of Theorems

Lemma A1: Suppose the hypotheses are re-labeled in the descending order of $\bar{d}_k$ and let $\hat{q}^*_{\alpha_0}(\ell)$ be the SPA critical value based on the subsample where we drop the data related to the first $\ell - 1$ hypotheses. Then, $\hat{q}^*_{\alpha_0}(\ell)$ is non-increasing in ℓ.

Proof: For $k > \ell$, $\hat{q}^*_{\alpha_0}(k)$ is determined by the distribution of the maximum of a smaller number of observations and hence can not be greater than $\hat{q}^*_{\alpha_0}(\ell)$. □

Lemma A2: Suppose the hypotheses are re-labeled in the descending order of $\bar{d}_k$ and $\hat{q}^*_{\alpha_0}(\ell)$ is defined as in Lemma A1. Then H_0^ℓ is rejected by the Step-SPA procedure defined in Section 2.2 if and only if $\sqrt{n}\bar{d}_j > \hat{q}^*_{\alpha_0}(j)$ for all $j = 1, ..., \ell$.

Proof: Suppose $\sqrt{n}\bar{d}_j > \hat{q}^*_{\alpha_0}(j)$ for all $j = 1, ..., \ell$. At the first stage of the Step-SPA test, $\sqrt{n}\bar{d}_1 > \hat{q}^*_{\alpha_0}(1)$, so H_0^1 is rejected at this stage and the procedure will continue. If H_0^ℓ is also rejected at this stage, then we are done. If not, suppose the first $k_1 < \ell$ hypotheses are rejected at the first stage. In the second stage, we have $\sqrt{n}\bar{d}_{k_1+1} > \hat{q}^*_{\alpha_0}(k_1 + 1)$. As a result, $H_0^{k_1+1}$ is rejected and procedure will continue. If H_0^ℓ is rejected at this stage, then we are done; otherwise, H_0^ℓ will be rejected in finite steps by the same argument.

Suppose the statement that $\sqrt{n}\bar{d}_j > \hat{q}^*_{\alpha_0}(j)$ for all $j = 1, ..., l$ is not true and $k_0 \leq \ell$ is the first hypothesis such that $\sqrt{n}\bar{d}_{k_0} \leq \hat{q}^*_{\alpha_0}(k_0)$. By the previous part, the Step-SPA test continues until the first $k_0 - 1$ hypotheses are rejected. It follows from Lemma A1 that $\sqrt{n}\bar{d}_{k_0} \leq \hat{q}^*_{\alpha_0}(k_0) \leq \hat{q}^*_{\alpha_0}(k)$ for all $k = 1, ..., k_0 - 1$, so $H_0^{k_0}$ will not be rejected in the previous stages no matter how the Step-SPA test procedure proceeds. After the first $k_0 - 1$ hypotheses are all rejected, we have $\sqrt{n}\bar{d}_{k_0} \leq \hat{q}^*_{\alpha_0}(k_0)$ and the procedure stops. Hence, H_0^ℓ will not be rejected. □

Similarly, we define the bootstrapped RC critical value, $\hat{r}^*_{\alpha_0}(\ell)$, as

$$\hat{r}^*_{\alpha_0}(\ell) = \max(\hat{r}_{\alpha_0}(\ell), 0),$$

where $\hat{r}_{\alpha_0}(\ell) = \inf\{r | P^*[\sqrt{n}\max_{k=\ell,\ell+1,...,m}(\bar{d}^*_k - \bar{d}_k) \leq r] \geq 1 - \alpha_0\}$.

Lemma A3: $\hat{r}^*_{\alpha_0}(\ell) \geq \hat{q}^*_{\alpha_0}(\ell)$ for all ℓ.

Proof: Note that

$$\sqrt{n}\max_{k=\ell,\ell+1,...,m}(\bar{d}^*_k - \bar{d}_k) \geq \sqrt{n}\max_{k=\ell,\ell+1,...,m}(\bar{d}^*_k - \bar{d}_k + \hat{\mu}_k),$$

since $\hat{\mu}_k \leq 0$. Hence, the pth quantile of the left-hand side is never smaller than that of the right-hand side for $p \in (0,1)$. It follows that $\hat{r}^*_{\alpha_0}(\ell) \geq \hat{q}^*_{\alpha_0}(\ell)$. □

Proof of Theorem 2.2: If $\mu_k > 0$, then $\sqrt{n}\bar{d}_k \to \infty$ with probability 1. On the other hand, $\hat{q}^*_{\alpha_0}(1)$ is bounded in probability, since $\hat{q}^*_{\alpha_0}(1) \xrightarrow{p} q_{\alpha_0} < \infty$ where q_{α_0} is the $(1-\alpha_0)$th quantile of $\max\{N(\mathbf{0}, \mathbf{\Omega}_0)\}$ and $\mathbf{\Omega}_0$ is the submatrix of $\mathbf{\Omega}$ after we delete the jth row and jth column of $\mathbf{\Omega}$ if $\mu_j < 0$. As a result, $\sqrt{n}\bar{d}_k > \hat{q}^*_{\alpha_0}(1)$ with probability 1 which implies H_0^k will be rejected in the first step with probability 1 based on the procedure defined in Section 2.2.

Suppose there exists some j with $\mu_j = 0$. Let $I_0 = \{i | H_0^i \text{ is true}\}$ which is non-empty and suppose $\hat{q}^*_{\alpha_0}(I_0)$ is calculated based on the data of I_0. Those hypotheses with $\mu_i > 0$ will be rejected in the first step with probability 1. Similar to the proof in Romano and Wolf (2005), the familywise error rate in the limit is:

$$\begin{aligned}
\lim_{n\to\infty} FWE &= \lim_{n\to\infty} P(\sqrt{n}\bar{d}_i > \hat{q}^*_{\alpha_0}(I_0) \text{ for at least one } i \in I_0) \\
&= \lim_{n\to\infty} P\Big(\max_{i\in I_0}\{\sqrt{n}\bar{d}_i\} > \hat{q}^*_{\alpha_0}(I_0)\Big) \\
&= \alpha_0.
\end{aligned}$$

The last equality holds because when $\alpha_0 < 1/2$, $\hat{q}^*_{\alpha_0}(I_0)$ converges to the $(1-\alpha_0)$th quantile of the limiting distribution of $\max_{i\in I_0}\{\sqrt{n}\bar{d}_i\}$ which is strictly greater than 0 since $P(\max_{i\in I_0}\{\sqrt{n}\bar{d}_i\} \leq 0) \leq P(\sqrt{n}\bar{d}_j \leq 0) = 1/2$ when n tends to infinity. □

Proof of Theorem 2.3: First, the Step-RC test is defined as follows:

1. Re-arrange $\bar{d}_k$ in the descending order.
2. Reject H_0^1 if $\sqrt{n}\bar{d}_1 > \hat{r}^*_{\alpha_0}(1)$. If H_0^1 is not rejected, then stop; otherwise, go to next step.
3. Given H_0^j for $j = 1, ..., \ell$ are rejected, then reject $H_0^{\ell+1}$ if $\sqrt{n}\bar{d}_{\ell+1} > \hat{r}^*_{\alpha_0}(\ell+1)$. If $H_0^{\ell+1}$ is not rejected, then stop; otherwise, go to next step.
4. Repeat Step 3 till no hypothesis can be rejected.

By the same arguments of the proof of Lemma A2, we can show that H_0^ℓ is rejected by the Step-RC test procedure defined above if and only if $\sqrt{n}\bar{d}_j > \hat{r}^*_{\alpha_0}(j)$ for all $j = 1, ..., \ell$.

We prove the case of the average power which is defined as the average of the individual probabilities of rejecting each false null hypothesis. The proofs for other power definitions are similar. To show that the Step-SPA test is more powerful than the Step-RC test in terms of the average power, it suffices to show that all hypotheses rejected by the Step-RC test will also be rejected by the Step-SPA test. First, we define

$$K_s = \{i | H_0^i \text{ is rejected by the Step-SPA test}\}$$
$$K_r = \{i | H_0^i \text{ is rejected by the Step-RC test}\}.$$

If K_r is empty, it is obvious that $K_r \subseteq K_s$. If K_r is non-empty and the first $k_r > 0$ hypotheses are rejected by the Step-RC test, then $\sqrt{n}\bar{d}_j > \hat{r}^*_{\alpha_0}(j)$ for all $j = 1, ..., k_r$ and it follows from Lemma A3 that $\sqrt{n}\bar{d}_j > \hat{r}^*_{\alpha_0}(j) \geq \hat{q}^*_{\alpha_0}(j)$ for all $j = 1, ..., k_r$. Hence, by Lemma A2, the first k_r hypotheses are rejected by the Step-SPA test and we have $K_r \subseteq K_s$.

Let $I_1 \equiv \{i | \mu_i > 0\}$ denote the set of the wrong null hypotheses. Let P_r and P_s denote the average powers of the Step-RC test and the Step-SPA test respectively. If I_1 is empty, $P_r = P_s = 0$. If I_1 is non-empty, then

$$P_r = \frac{E[\text{number of } K_r \cap I_1]}{\text{number of } I_1},$$

and

$$P_s = \frac{E[\text{number of } K_s \cap I_1]}{\text{number of } I_1}.$$

It follows that $P_r \leq P_s$, because $K_r \cap I_1 \subseteq K_s \cap I_1$. □

Appendix B: The Collection of Technical Trading Rules

We consider in this study 9,120 moving average rules and 7,260 filter rules. These rules are constructed by extending the rules studied in Sullivan et al. (1999); readers are referred to their article for details. We describe these rules using the same notations. Set m and n (the numbers of days for long and short moving averages) = 1, 2, 5, 10, 15, 20, 25, 30, 40, 50, 75, 100, 125, 150, 200, 250 (16 values and $m > n$). So, $m - n$ combinations = 120, b (fixed band multiplicative value) = 0.001, 0.005, 0.01, 0.015, 0.02, 0.03, 0.04, 0.05 (8 values), d (number of days for the time delay filter) = 2, 3, 4, 5 (4 values), and c (number of days a position is held, ignoring all other signals) = 2,

3, 4, 5, 10, 25, 50 (7 values). As a result, the total number of moving average rules is $[1+b+d+c+(b\times c)]\times m-n$ *combinations* $= 9,120$. It can be observed that we basically extend the nine rules in Brock et al. (1992) to 9,120 possibilities by considering different combinations of fixed band multiplicative values, fixed holding days, and moving average days.

Similarly, our filter rules are constructed as follows. We set x (change in security price to initiate a position) = 0.005, 0.01, 0.015, 0.02, 0.025, 0.03, 0.035, 0.04, 0.045, 0.05, 0.06, 0.07, 0.08, 0.09, 0.1, 0.12, 0.14, 0.16, 0.18, 0.2, 0.25, 0.3, 0.4, 0.5 (24 values), y (change in security price to liquidate a position) = the same 24 values as x with y less than x, e (an alternative definition of local extrema where a high (low) is defined as the most recent closing price to be greater (less) than the e previous closing prices) = 1, 2, 3, 4, 5, 10, 15, 20 (8 values), k (the number of days to define local extrema) = 5, 10, 20, 40, 60, 80, 100, 150, 200, 250 (10 values), and c (number of days a position is held, ignoring all other signals) = 2, 3, 4, 5, 10, 25, 50 (7 values). Note that we consider another way to define local maximum and minimum for the initiation of the first position. The maximum of the first k days is the "high" and the minimum of the first k days is the "low". As a result, the total number of filter rules is $x+(x\times k)+(x\times e)+(x\times k\times e)+(x\times c)+(x\times k\times c)+x-y$ *combinations* $+ (x - y$ *combinations* $\times k) = 7,260$.

Appendix C: Best Technical Trading Rules

Period	Index/ETF	Performance measure			
		Mean return	Sharpe ratio	x-statistic	St. mean ret.
Pre-	S&P600SG	MA$(n = 2, 1)$	MA$(n = 2, 1)$	MA$(n = 2, 1)$	MA$(n = 2, 1)$
ETF	RUT2000	MA$(n = 2, 1)$	MA$(n = 2, 1)$	MA$(n = 2, 1)$	MA$(n = 2, 1)$
	NASDAQ	MA$(n = 2, 1; b = 0.001)$	MA$(n = 2, 1; b = 0.001)$	MA$(n = 2, 1; b = 0.001)$	MA$(n = 2, 1; b = 0.001)$
Post-	IJT	MA$(n = 15, 10; d = 3)$	FR$(x = 0.04; c = 4)$	MA$(n = 15, 10; d = 3)$	FR$(x = 0.04; c = 4)$
ETFs	IWM	MA$(n = 15, 10; d = 2)$	FR$(x = 0.06; c = 4)*$	MA$(n = 15, 10; d = 2)$	MA$(n = 15, 10; d = 3)$
	ONEQ	FR$(x = 0.01; c = 25)$	FR$(x = 0.01; c = 25)$	FR$(x = 0.01; c = 25)$	FR$(x = 0.015; k = 40)$
	Emerging	MA$(n = 2, 1; b = 0.001)$	MA$(n = 2, 1; b = 0.001)$	MA$(n = 2, 1; b = 0.001)$	MA$(n = 2, 1; b = 0.001)$
Pre-	Brazil	FR$(x = 0.035; e = 1)*$	FR$(x = 0.035; e = 1)*$	FR$(x = 0.01)$	FR$(x = 0.035; e = 1)*$
ETFs	Korea	MA$(n = 20, 2)*$	FR$(x = 0.07)$	MA$(n = 20, 2)*$	MA$(n = 20, 2)*$
	Malaysia	MA$(n = 2, 1; b = 0.001)$	FR$(x = 0.005)$	MA$(n = 2, 1; b = 0.001)$	FR$(x = 0.005)$
	Mexico	FR$(x = 0.01; k = 250; e = 2)$	FR$(x = 0.005)$	FR$(x = 0.03; k = 250)$	FR$(x = 0.005)$
	Taiwan	MA$(n = 4, 1)*$	FR$(x = 0.18, 0.045; k = 5)$	MA$(n = 4, 1)*$	FR$(x = 0.18, 0.045; k = 5)*$
	EEM	MA$(n = 15, 1; c = 50)*$	FR$(x = 0.18; k = 40; c = 3)*$	MA$(n = 15, 1; c = 50)*$	FR$(x = 0.18; k = 40; c = 3)*$
Post-	EWZ	FR$(x = 0.005; e = 3)*$	FR$(x = 0.01)$	FR$(x = 0.005; e = 3)*$	FR$(x = 0.01)$
ETFs	EWY	FR$(x = 0.01; c = 4)$	FR$(x = 0.01; c = 4)$	FR$(x = 0.01; c = 4)$	FR$(x = 0.01; c = 4)$
	EWM	FR$(x = 0.01)*$	FR$(x = 0.015, 0.01)*$	FR$(x = 0.01)*$	FR$(x = 0.015, 0.01)*$
	EWW	FR$(x = 0.005; e = 2)*$	FR$(x = 0.005)$	FR$(x = 0.005; e = 2)*$	FR$(x = 0.005)$
	EWT	MA$(n = 4, 1)$	FR$(x = 0.12; c = 4)*$	MA$(n = 20, 15; c = 50)$	FR$(x = 0.035; k = 5; c = 25)$

Notes: (1) All parameters are described in Appendix B. (2) The $*$ mark indicates that there are more than one best rules. In those cases, we report the one that appears first in our trading rule set. (3) MA denotes moving average rules, and FR denotes filter rules. (4) "St. mean ret." denotes studentized mean returns.

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Table 1: Simulated average rejection rates and familywise error rates of the Step-SPA and Step-RC tests.

	Equal group			Unequal group			All neutral
Test	AR rate (1-step)	AR rate (all-steps)	FWE rate	AR rate (1-step)	AR rate (all-steps)	FWE rate	FWE rate
	30 outperforming + 30 neutral + 30 poor			10 outperforming + 10 neutral + 70 poor			90 neutral
Step-SPA	96.3	97.7	4.8	98.3	99.0	4.6	3.0
Step-RC	95.1	96.2	2.2	95.1	95.5	0.2	3.0
	300 outperforming + 300 neutral + 300 poor			100 outperforming + 100 neutral + 700 poor			900 neutral
Step-SPA	85.9	89.2	3.2	91.9	94.2	3.0	1.4
Step-RC	83.5	85.6	1.2	83.8	84.4	0.2	1.4
	3000 outperforming + 3000 neutral + 3000 poor			1000 outperforming + 1000 neutral + 7000 poor			9000 neutral
Step-SPA	68.7	72.5	1.6	77.7	81.4	0.8	1.0
Step-RC	64.9	67.6	0.8	64.8	65.4	0.0	1.0

Notes: (1) All numbers are in percentage (%). (2) We consider m return series with three different means: "Outperforming" returns with $c_i = 8$ bps (means = 8.1 bps) for $i = 1, ..., m_1$, "neutral" returns with $c_i = 0$ (zero mean) for $i = m_1 + 1, ..., m_1 + m_2$, and "poor" returns with $c_i = -8$ bps (means = -8.1 bps) for $i = m_1 + m_2 + 1, ..., m$. (3) Other parameters in the simulation are: $n = 1000$, $R = 500$, $B = 500$, and $Q = 0.9$. (4) The significance level of the Step-SPA and Step-RC tests is 5%. (5) "AR rate" stands for average rejection rate which is the percentages of correctly rejected return series with $c_i = 8$ bps. (6) "FWE rate" is the familywsie error rate, the percentage of incorrectly rejected neutral and poor return series. (7) "1-step" denotes the results from the first step of the Step-SPA and Step-RC tests which are just the original SPA and RC tests; "all-steps" denotes the final results of the Step-SPA and Step-RC tests.

Table 2: Simulated average rejection rates and familywise error rates of the Step-SPA and Step-RC tests (Studentized).

	Equal group			Unequal group			All neutral
Test	AR rate (1-step)	AR rate (all-steps)	FWE rate	AR rate (1-step)	AR rate (all-steps)	FWE rate	FWE rate
	30 outperforming + 30 neutral + 30 poor			10 outperforming + 10 neutral + 70 poor			90 neutral
Step-SPA	96.6	98.0	4.8	98.8	99.4	4.8	3.2
Step-RC	95.3	96.4	2.2	95.4	95.6	0.8	3.2
	300 outperforming + 300 neutral + 300 poor			100 outperforming + 100 neutral + 700 poor			900 neutral
Step-SPA	86.1	89.4	3.0	92.0	94.3	3.4	1.8
Step-RC	83.8	85.9	1.2	84.0	84.6	0.4	1.8
	3000 outperforming + 3000 neutral + 3000 poor			1000 outperforming + 1000 neutral + 7000 poor			9000 neutral
Step-SPA	68.8	72.6	2.0	78.6	82.5	1.6	1.2
Step-RC	65.2	67.8	1.0	65.0	65.6	0.6	1.2

Notes: (1) All numbers are in percentage (%). (2) We consider m return series with three different means: "Outperforming" returns with $c_i = 8$ bps (means = 8.1 bps) for $i = 1, ..., m_1$, "neutral" returns with $c_i = 0$ (zero mean) for $i = m_1 + 1, ..., m_1 + m_2$, and "poor" returns with $c_i = -8$ bps (means = -8.1 bps) for $i = m_1 + m_2 + 1, ..., m$. (3) Other parameters in the simulation are: $n = 1000$, $R = 500$, $B = 500$, and $Q = 0.9$. (4) We test the significance of the studentized statistics using the Step-SPA and Step-RC tests at 5% level. (5) "AR rate" stands for average rejection rate which is the percentages of correctly rejected return series with $c_i = 8$ bps. (6) "FWE rate" is the familywsie error rate, the percentage of incorrectly rejected neutral and poor return series. (7) "1-step" denotes the results from the first step of the Step-SPA and Step-RC tests which are just the original SPA and RC tests; "all-steps" denotes the final results of the Step-SPA and Step-RC tests.

Table 3: The pre- and post-ETF periods for market indices and their ETFs.

Market	Identifier	Index	Pre-ETF Period	Obs.	ETF Incept. date
U.S.	SP600SG	S&P SmallCap 600/Citigroup Growth Index	1/4/1989 – 12/31/1999	2779	July 24, 2000
Markets	RUT2000	Russell 2000 Index	1/3/1990 – 12/31/1999	2527	March 1, 2000
	NASDAQ	NASDAQ Composite Index	1/3/1990 – 12/31/1998	2275	Sept. 25, 2003
	Emerging	MSCI Emerging Markets Index	1/4/1993 – 12/31/2002	2601	April 7, 2003
	Brazil	MSCI Brazil Index	1/1/1990 – 12/31/1999	2610	July 10, 2000
Emerging	Korea	MSCI South Korea Index	1/2/1990 – 12/31/1999	2865	May 9, 2000
Markets	Malaysia	MSCI Malaysia Index	1/1/1988 – 12/29/1995	2086	March 12, 1996
	Mexico	MSCI Mexico Index	1/1/1988 – 12/29/1995	2086	March 12, 1996
	Taiwan	MSCI Taiwan Index	1/1/1990 – 12/31/1999	2610	June 20, 2000
Market	Ticker	ETF	Post-ETF Period	Obs.	ETF Incept. date
U.S.	IJT	S&P SmallCap 600 Growth Index Fund	7/28/2000 – 12/30/2005	1364	July 24, 2000
Markets	IWM	Russell 2000 Index Fund	5/30/2000 – 12/30/2005	1406	March 1, 2000
	ONEQ	NASDAQ Composite Index Tracking Fund	10/1/2003 – 12/30/2005	568	Sept. 25, 2003
	EEM	MSCI Emerging Markets Index Fund	10/2/2003 – 12/30/2005	566	April 7, 2003
	EWZ	MSCI Brazil Index Fund	7/14/2000 – 12/30/2005	1368	July 10, 2000
Emerging	EWY	MSCI South Korea Index Fund	6/1/2000 – 12/30/2005	1401	May 9, 2000
Markets	EWM	MSCI Malaysia Index Fund	4/1/1996 – 12/30/2005	2453	March 12, 1996
	EWW	MSCI Mexico Index Fund	4/1/1996 – 12/30/2005	2453	March 12, 1996
	EWT	MSCI Taiwan Index FUnd	6/26/2000 – 12/30/2005	1384	June 20, 2000

Note: The pre- and post-ETF periods of NASDAQ Composite Index separate for almost 5 years because there was another ETF (PowerShares QQQ Fund that tracks NASDAQ-100 Index) during that time. The pre-ETF period ends on Dec. 31, 1998, which is before the inception of PowerShares QQQ Fund on March 9, 1999, and the post-ETF period begins on Oct. 1, 2003, which is after the inception of NASDAQ Composite Index Tracking Fund on Sept. 25, 2003.

Table 4: Descriptive statistics of daily returns on indices and ETFs.

Marklet	Index/ETF	Period	Mean (bps)	St. dev. (bps)	First AC	Skewness	Kurtosis
U.S.	S&P SmallCap 600/Citigroup Growth Index	pre-ETF	4.75	79.67	0.24	−0.78	8.04
Indices	Russell 2000 Index	pre-ETF	4.62	78.83	0.24	−0.84	8.26
	NASDAQ Composite Index	pre-ETF	7.40	102.15	0.14	−0.58	8.93
U.S.	SmallCap 600 Growth Index Fund	post-ETF	4.09	138.65	0.03	0.11	4.20
ETFs	Russell 2000 Index Fund	post-ETF	4.13	138.32	−0.04	0.05	3.46
	NASDAQ Composite Index Tracking Fund	post-ETF	3.82	93.72	0.02	−0.03	3.09
	MSCI Emerging Markets Index	pre-ETF	0.25	103.28	0.28	−0.55	6.91
Emerging	MSCI Brazil Index	pre-ETF	9.37	304.04	0.13	0.04	10.19
Market	MSCI South Korea Index	pre-ETF	0.08	256.30	0.08	1.05	18.76
Indices	MSCI Malaysia Index	pre-ETF	7.02	125.12	0.09	−0.12	16.59
	MSCI Mexico Index	pre-ETF	12.5	197.99	0.16	0.63	16.37
	MSCI Taiwan Index	pre-ETF	1.71	210.15	0.05	0.19	6.42
	MSCI Emerging Markets Index Fund	post-ETF	3.87	93.88	0.02	−0.03	3.08
Emerging	MSCI Brazil Index Fund	post-ETF	7.75	237.53	0.01	−0.17	4.94
Market	MSCI South Korea Index Fund	post-ETF	9.30	237.71	−0.04	−0.24	6.06
ETFs	MSCI Malaysia Index Fund	post-ETF	1.62	246.62	−0.00	0.94	11.63
	MSCI Mexico Index Fund	post-ETF	7.89	208.93	−0.00	0.11	11.65
	MSCI Taiwan Index Fund	post-ETF	0.10	234.30	−0.10	−0.12	5.28

Notes: (1) Mean and standard deviations are reported in bps (basis point); "First AC" stands for first-order autocorrelation. (2) Ljung-Box Q statistics indicate significant first-order autocorrelation for all index returns at 5% level but insignificant first-order autocorrelation for all ETF returns except MSCI Taiwan Index Fund at 5% level.

Table 5: The numbers of outperforming rules in pre- and post-ETF periods.

Market	Index/ETF	Period	Outperforming rules			
			Mean return	Sharpe ratio	x-statistic	St. mean ret.
U.S.	S&P600SG	pre-ETF	269	136	220	230
Indices	RUT2000	pre-ETF	186	109	179	171
	NASDAQ	pre-ETF	33	1	5	7
U.S.	IJT	post-ETF	0	0	0	0
ETFs	IWM	post-ETF	0	0	0	0
	ONEQ	post-ETF	0	0	0	0
	Emerging	pre-ETF	797	414	917	758
Emerging	Brazil	pre-ETF	117	88	0	143
Market	Korea	pre-ETF	0	0	0	0
Indices	Malaysia	pre-ETF	81	2	70	68
	Mexico	pre-ETF	559	370	331	490
	Taiwan	pre-ETF	0	0	0	0
	EEM	post-ETF	0	0	0	0
Emerging	EWZ	post-ETF	0	0	0	0
Market	EWY	post-ETF	0	0	0	0
ETFs	EWM	post-ETF	55	0	66	0
	EWW	post-ETF	241	152	285	198
	EWT	post-ETF	0	0	0	0

Notes: (1) The last three columns are the numbers of outperforming rules identified by the Step-SPA test under 5% level, based on mean return, Sharpe ratio, x-statistic, and studentized mean returns, respectively. (2) We impose a 0.05% one-way transaction cost for all trades.

Table 6: The identified best rules' performance measures and break-even transaction costs.

Market	Index/ETF	Best rule and break-even transaction cost							
		Mean (p-value)	Break-even cost (bps)	Sharpe (p-value)	Break-even cost (bps)	x-stat (p-value)	Break-even cost (bps)	St. mean (p-value)	Break-even cost (bps)
U.S.	S&P600SG	0.16 (.00)	26	2.69 (.01)	24	0.15 (.00)	26	9.55 (.00)	26
Indices	RUT2000	0.15 (.00)	25	2.68 (.00)	23	0.14 (.00)	24	8.99 (.00)	25
	NASDAQ	0.13 (.00)	24	1.74 (.00)	22	0.11 (.00)	23	5.48 (.00)	24
U.S.	IJT	0.06 (.99)	N/A	0.95 (.94)	N/A	0.06 (.82)	N/A	1.88 (.99)	N/A
ETFs	IWM	0.06 (.97)	N/A	0.79 (.96)	N/A	0.06 (.86)	N/A	1.89 (.99)	N/A
	ONEQ	0.09 (.65)	N/A	1.42 (.89)	N/A	0.09 (.37)	N/A	2.29 (.80)	N/A
	Emerging	0.22 (.00)	44	3.01 (.00)	41	0.22 (.00)	44	10.96 (.00)	36
Emerging	Brazil	0.30 (.00)	44	1.58 (.01)	41	0.18 (.58)	N/A	5.17 (.01)	42
Market	Korea	0.14 (.38)	N/A	0.79 (.84)	N/A	0.14 (.37)	N/A	2.58 (.55)	N/A
Indices	Malaysia	0.15 (.00)	29	1.74 (.03)	23	0.15 (.00)	28	5.37 (.01)	26
	Mexico	0.25 (.00)	75	2.05 (.00)	34	0.24 (.00)	70	5.86 (.00)	36
	Taiwan	0.09 (.66)	N/A	0.80 (.75)	N/A	0.09 (.40)	N/A	2.67 (.68)	N/A
	EEM	0.06 (.86)	N/A	1.27 (.88)	N/A	0.06 (.81)	N/A	1.91 (.98)	N/A
Emerging	EWZ	0.17 (.44)	N/A	1.25 (.35)	N/A	0.17 (.32)	N/A	3.04 (.25)	N/A
Market	EWY	0.14 (.77)	N/A	0.96 (.90)	N/A	0.13 (.42)	N/A	2.34 (.75)	N/A
ETFs	EWM	0.21 (.04)	66	1.23 (.19)	N/A	0.21 (.03)	67	4.31 (.16)	N/A
	EWW	0.24 (.00)	44	2.06 (.00)	23	0.24 (.00)	43	6.51 (.02)	25
	EWT	0.09 (.99)	N/A	0.63 (1.00)	N/A	0.09 (.72)	N/A	1.65 (.98)	N/A

Notes: (1) The best rules are identified by the Step-SPA test with the highest test statistics in terms of, respectively, the mean returns (%), annualized Sharpe ratios, x-statistics (%), and studentized mean returns at 5% level. The identities and details of the best rules are summarized in Appendix C. (2) The 3rd, 5th, 7th, and 9th columns are the mean returns, annualized Sharpe ratios, x-statistics, and studentized mean returns of the best rules and their p-values (in parentheses). (3) The 4th, 6th, 8th, and 10th columns are the break-even costs (in bps) to take the performance of the best rules down to zero.

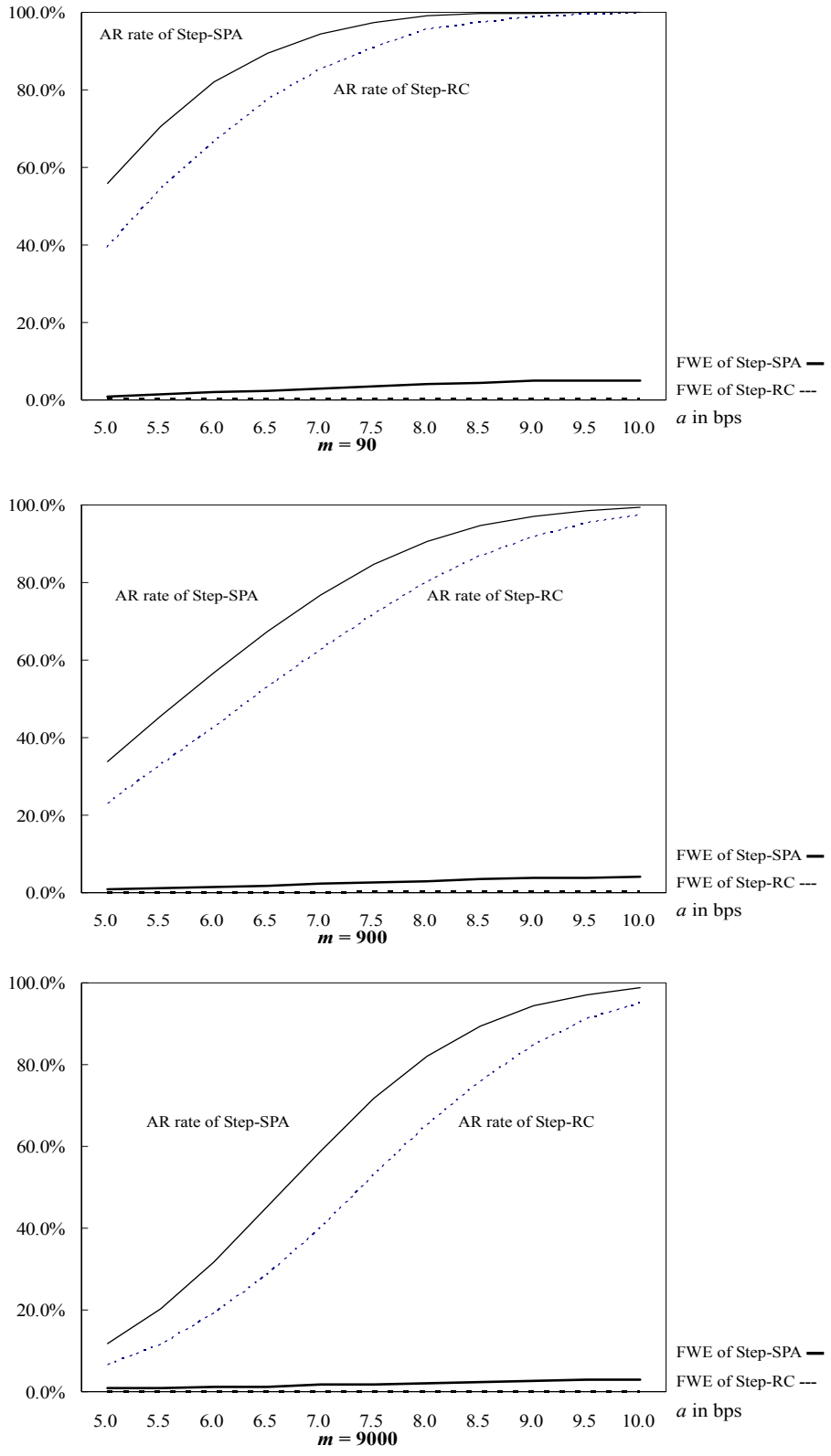

Figure 1: Average rejection rates and familywise error rates of the Step-SPA and Step-RC tests.