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A Strategy for Testing the Unit Root in AR(1) Model with Intercept. A Monte Carlo Experiment

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RESUMEN
En este artículo se introduce una estrategia para contrastar la raíz unitaria en los procesos AR(1) con término constante donde el valor inicial es una constante conocida. En este contexto el test tradicional de Dickey-Fuller es no similar, siendo el término constante el parámetro molesto. La estrategia de contraste que proponemos tiene en cuenta la citada no similaridad. Concretamente, se trata de un test bilateral de la hipótesis de paseo aleatorio poco usual, pues la región de aceptación se construye eliminando áreas iguales de las colas de dos distribuciones diferentes: de la cola inferior de la *t* de Student y de la cola superior de la distribución tabulada por Dickey y Fuller. En algunos casos, la estrategia no permite tomar una decisión concreta sobre la existencia de raíz unitaria. Para resolver estas situaciones sugerimos contrastar la relevancia del término constante, y si la duda persiste, se lleva a cabo el contraste basado en el estadístico Φ_1 que propusieron Dickey y Fuller (1981). Finalmente, mediante un experimento Monte Carlo se pone de manifiesto que la estrategia propuesta es más potente y presenta menos distorsiones en el tamaño que el test convencional de Dickey-Fuller

Palabras clave: raíz unitaria, test de Dickey-Fuller, no similaridad, Monte Carlo, tamaño empírico, tamaño nominal

ABSTRACT
In this paper we introduce a strategy for testing the unit root hypothesis in a first-order autoregressive process with an unknown intercept where the initial value of the variable is a known constant. In the context of this model the standard Dickey-Fuller test is non-similar, the intercept being the nuisance parameter. The testing strategy we propose takes into account this non-similarity. It is an unusual two-sided test of the random walk hypothesis since it involves two distributions where the acceptance region is constructed by taking away equal areas for the lower tail of the Student's *t* distribution and the upper tail of the distribution tabulated by Dickey and Fuller under the null hypothesis of unit root. In some cases, this strategy does not allow the taking of a direct decision concerning the existence of a unit root. To deal with these situations we suggest testing for the significance of the intercept, and if doubt continues, we use Φ_1 test proposed by Dickey and Fuller (1981). Finally, in order to demonstrate the relevance of non-similarity, Monte Carlo simulations are used to show that the testing strategy is more powerful at stable alternatives and has less size distortions than the two-sided test considered by Dickey and Fuller.

Keywords: unit root, Dickey-Fuller tests, non-similarity, Monte Carlo simulations, empirical size, nominal size
JEL classification: C12, C15, C22

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A Strategy for Testing the Unit Root in AR(1) Model with Intercept. A Monte Carlo Experiment

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Abstract

In this paper we introduce a strategy for testing the unit root hypothesis in a first-order autoregressive process with an unknown intercept where the initial value of the variable is a known constant. In the context of this model the standard Dickey-Fuller test is non-similar, the intercept being the nuisance parameter. The testing strategy we propose takes into account this non-similarity. It is an unusual two-sided test of the random walk hypothesis since it involves two distributions where the acceptance region is constructed by taking away equal areas for the lower tail of the Student's *t* distribution and the upper tail of the distribution tabulated by Dickey and Fuller under the null hypothesis of unit root. In some cases, this strategy does not allow the taking of a direct decision concerning the existence of a unit root. To deal with these situations we suggest testing for the significance of the intercept, and if doubt continues, we use Φ_1 test proposed by Dickey and Fuller (1981). Finally, in order to demonstrate the relevance of non-similarity, Monte Carlo simulations are used to show that the testing strategy is more powerful at stable alternatives and has less size distortions than the two-sided test considered by Dickey and Fuller.

AMS Classification: 62M10

Keywords: unit root, Dickey-Fuller tests, non-similarity, Monte Carlo simulations, empirical size, nominal size

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1 Introduction

A problem arising in many time series applications is the question of whether a series is better characterized as stationary fluctuations around a deterministic trend or as non-stationary process that has no tendency to return to a deterministic path. The latter is equivalent to asking if the time series has a unit root and it is said that the series has stochastic trend.

The non-stationarity has important economic and statistical implications which differe acording to its nature (Granger and Newbold (1974), Nelson and Kang (1981), and Nelson and Plosser (1982)). Therefore, the distinction between the two classes of above mentioned processes is fundamental for the understanding of the nature of economic phenomena, and to carry out the appropriate statistical treatment.

The importance of distinguishing a deterministic trend from a stochastic trend motivates much of the interest in unit root tests. However, some practicioners decide to differentiate a time series on the basis of techniques less formal, such as visual inspection of the sample autocorrelation function of the series. With regard to this method, Roldan (2000) showed that it is good at detecting non-stationary but in some cases does not allow us to distinguish between the two types of trend.

Dickey (1976), Fuller (1976), and Dickey and Fuller (1979) proposed some test statistics for the unit root hypothesis for an observed time series which can be generated by three different processes

$$Y_t = rY_{t-1} + e_t \qquad t = 1, 2, \dots \qquad (1)$$

$$Y_t = m + rY_{t-1} + e_t \qquad t = 1, 2, \dots \qquad (2)$$

$$Y_t = m + bt + rY_{t-1} + e_t \qquad t = 1, 2, \dots \qquad (3)$$

where $\{ e_t \}$ is a sequence of independent normal random variables with mean zero and variance s^2. The unit root hypothesis corresponds to $r = 1$ in the three models and the statistics are based upon the usual OLS estimator of r in each model.

The distribution of Dickey and Fuller tests relied on the innovation process (e_t) being white noise, and so these tests are not appropriate if the innovations are ARMA process. However, independence and homoskedasticity are rather strong assumptions to make about the error in most empirical econometric work. Such a restriction is a considerable drawback in applying these tests to economic time series. Different testing procedures have been suggested to tackle this problem.

Dickey and Fuller (1981) extended the DF tests to an AR process of known order containing no more than one unit root. The procedure, called 'augmented' Dickey-Fuller (ADF) tests, consists of adding to the models (1), (2) or (3) lagged changes in the dependent variable to capture autocorrelated omitted variables which would otherwise, by default, appear in the error term. Said and Dickey (1984) provided a test procedure valid for a general ARIMA(p,1,q) process for which p and q are of unknown orders. The method involves approximating the true process by an autoregression in which the number of lags increase with sample size (this approach represents a generalization of the procedure in which the ADF tests are based). Solo (1984) developed a testing procedure based on the LM test. An alternative approach was suggested by Phillips (1987) in the context of model (1) where more general dependence in the error process is allowed for, including conditional heteroscedasticity. This procedure does not require the estimation of additional parameters in the regression model (1), but Phillips suggests accounting for the autocorrelation that will be present (when these terms are omitted) through a non-parametric correction to standard statistics. Phillips and Perron (1988) extended the Phillips procedure to models (2)and (3).

Simulation evidence in Schwert (1989) showed that Said-Dickey and Phillips-Perron procedures cause size distortions for models with MA terms. Hall (1989, 1992) and Pantula and Hall (1991) proposed an alternative approach to testing for unit roots in a time series with moving average innovations based on an instrumental variable estimator.

On the other hand, Evans and Savin (1984) and Nankervis and Savin (1985, 1987) showed that the statistics proposed by Dickey and Fuller yield non-similar tests of the unit root hypothesis. Non-similarity implies that the distribution of a test statistic is affected by the value, under the null, of a nuisance parameter.

Specifically, the distributions of the DF tests based on equation (2) depend on m under the null, whereas in (3) the nuisance parameter is b. In any case, if a test is non-similar, then the appropriate critical values may depend upon nuisance parameters and if they are unknown we can mistakenly reject or not reject the null hypothesis. Dickey and Fuller (1981) showed that the statistic based on equation (3) they proposed does not depend on m under the null, so this statistic yields a similar test of the unit root hypothesis with drift. In this context, Nankervis and Savin (1985) proposed non-similar tests of the random walk hypothesis which involve two distributions and are substantially more powerful at most alternatives of interest than the similar tests considered by Dickey and Fuller (1981). Kiviet and Phillips (1990, 1992) considered exact and similar tests for the coefficient on a lagged dependent variable, in a first-order autoregressive model that may include multiple exogenous variable (these tests are known as KPh tests).

In this paper, our focus of attention is non-similarity. The motivation for investigating this property is we consider that if the influence of nuisance parameters is taken into account when the unit root hypothesis is tested in (2) and (3), the powers of Dickey and Fuller tests may be improved. Specifically, we propose a sequential procedure for testing the unit root in (2) on the basis of the idea of the two distributions test introduced by Nankervis and Savin (1985). Since we are interested in detecting the unit root, we state a two-sided test in the strategy, so the alternative hypothesis is: 'absence of unit root'.

The first proposal for a sequential procedure we can find appears in Perron (1988), and Dolado et al. (1990). They advocated the sequential use of Dickey-Fuller unit root tests and tests for the presence of a trend. On the other hand, Ayat and Burridge (2000) propose a sequential procedure for unit root testing and simultaneous identification of trend degree. Specifically, by using unit root tests as pre-tests before testing trend degree they improve efficiency of trend and parameter r estimators.

In general, the goal of these procedures is testing for the unit root (against stationary alternative) and the presence of trend at the same time. Besides, in all of them is necessary the estimation of different models. The strategy we propose is for testing the unit root in (2) irrespective of nuisance parameter m, against 'no unit root' and it is based on only one estimated model.

Thus, the aims of the present paper are as follows:

- To propose a testing strategy for the unit root hypothesis in (2) from the point of view of non-similarity.
- To compare the power and size of the two-sided tests of the unit root hypothesis proposed by Dickey and Fuller with the power and size of the testing strategy in the context of the model (2) using a Monte Carlo experiment.

The plan of the paper is as follows. In Section 2 we study the distributions of the usual regression *t* and *F* statistics in model (2) considering the effect of nuisance parameters. On the basis of these distributions we propose a strategy for tesing the unit root hypothesis in (2). We describe the Monte Carlo experiment in Section 3. In Section 4 we estimate the nominal size of the strategy. Monte Carlo estimates of the power of the Dickey and Fuller tests and the strategy are reported in Section 5. Section 6 contains concluding comments.

2 Testing the unit root in an AR(1) model with intercept in the context of non-similarity

The class of model we investigate consists of the model

$$Y_t = m + rY_{t-1} + e_t \qquad t = 1,2,\ldots,T \tag{5}$$

where m and r are unknown real numbers. We assume that Y_0 is a known constant and equal to zero and the $\{e_t\}$ is a sequence of independent normal random variables with mean zero and variance s^2.

2.1 Testing $r = 1$

For testing the unit root hypothesis in (5) a two-sided test is stated

$$H_0\colon r = 1$$

$$H_A\colon r \neq 1$$

Usually, this test is based on the *t* statistic associated with the ordinary least squares (OLS) estimator of r in (5). This *t* statistic for r will be denoted by t_{r_m}.

The distribution of t_{r_m} statistic under the null ($r = 1$) depends on the value of *m*. Specifically, if $m = 0$ the distribution is non-standard and Dickey (1976) obtained the empirical quantiles of the limiting and finite sample distribution by Monte Carlo methods (Fuller 1996, pp. 641-642)[1]. In this particular case the statistic is denoted by $\hat{t}_m$. These authors assumed Y_0 fixed in (5) but the distribution of $\hat{t}_m$ does not depend on the value of Y_0 .

In the context of (5) Dickey (1976, p.58) shows that when $r = 1$ and $m \neq 0$ the t_{r_m} statistic has asymptotically a standard normal distribution.

On the other hand, Nankervis and Savin (1985) establish that as *m* tends to infinity, the sampling distribution of t_{r_m} for $r = 1$ tends to Student's *t* with *T*–2 degrees of freedom, assuming the innovations are i.i.d. $(0, s^2)$ and $Y_0 = 0$. Actually, this is a particular result since they proved the result for a model in which there can be *K* exogenous variables. They showed empirically that when $m \geq 10$ Student's *t* provides a satisfactory approximate distribution of t_{r_m} for $r = 1$.

Therefore, the t_{r_m} statistic for $r = 1$ yields a non-similar test of the unit root hypothesis in (5) since its distribution under the null is influenced by the values of the nuisance parameter *m*.

In Figure 1 f_D is an approximation to the empirical density of Dickey-Fuller $\hat{t}_m$ statistic ($r = 1, m = 0$) for fixed *T*, and f_S is the density of the Student's *t* with *T*–2 degrees of freedom ($r = 1, m = \infty$).

Likewise, in this Figure 1, $DF_{1-a/2}$ and $DF_{a/2}$ denote the empirical quantiles of the distribution of $\hat{t}_m$. Thus, $(DF_{1-a/2}; DF_{a/2})$ is the acceptance region of the test

$$H_0: r = 1 \text{ (assuming } m = 0\text{)}$$

$$H_1: r \neq 1$$

[1] Extended tabulations can be found in Guilkey, D.K. and Schmidt, P. (1989).

On the other hand, $t_{T-2;a/2}$ denotes the quantile of order $a/2$ of Stundent's t with T–2 degrees of freedom, and ($-t_{T-2;a/2}$; $t_{T-2;a/2}$) is the acceptance region of the test

$$H_0\text{: } r = 1 \text{ (assuming } m \neq 0\text{)}$$

$$H_1\text{: } r \neq 1$$

As m is unknown, we propose to test the unit root hypothesis using the two distributions mentioned above simultaneously, where the acceptance region of the test would be ($-t_{T-2;a/2}$; $DF_{a/2}$) (region C in Figure 1). Thus, if the computed value of t_{r_m} statistic ($\hat{t}_{r_m}$ hereafter) lies in region C we do not reject the null hypothesis of a unit root with both tests. Therefore, $r = 1$ and m can be any real number.

However, if $\hat{t}_{r_m}$ lies in region A or in region E we reject the unit root hypothesis with both Dickey-Fuller and Student's t critical values. In this situation we conclude that $r \neq 1$, m being any real number. Therefore, A and E are regions where we always reject the null hypothesis of unit root.

On the other hand, if the computed value of t_{r_m} statistic lies in region B = ($DF_{1-a/2}$; $-t_{T-2;a/2}$) we have the following:

a) On the basis of critical values tabulated by Dickey (1976) the null hypothesis is not rejected since in this region $\hat{t}_{r_m} \in$ ($DF_{1-a/2}$; $DF_{a/2}$). Therefore, we conclude that $r = 1, m = 0$.

b) If we use the critical values of Student's t we have that $\hat{t}_{r_m} \notin$ ($-t_{T-2;a/2}$; $t_{T-2;a/2}$), so we reject H_0: $r = 1, m \neq 0$ and conclude that $r \neq 1$. It is likely that the rejection in this region is due to the case $r < 1$ and $m \neq 0$.

As we can see there are two possible decisions. If we do not use the correct critical values we may take a wrong decison. Everything depends on m.

Region D = ($DF_{a/2}$; $t_{T-2;a/2}$) is an analogous case of region B. In this region

a) The critical values of Student's t do not allow us to reject H_0: $r = 1$, $m \neq 0$, since $\hat{t}_{r_m} \in (-t_{T-2;a/2}; t_{T-2;a/2})$.

b) If we consider the critical values tabulated by Dickey (1976) we have that $\hat{t}_{r_m} \notin (DF_{1-a/2}; DF_{a/2})$, so we reject the null hypothesis H_0: $r = 1$ (assuming $m = 0$) and conclude that $r \neq 1$. In this case, it is expected that $r > 1$ and $m = 0$.

Again, to take the correct decision depends on what is known about m. The problem is that m is unknown which is the most common situation in practice. To overcome this difficulty we propose to test for the significance of the parameter m.

Table 1 presents the regions mentioned in this section and the consequence derived when $\hat{t}_{r_m}$ lies in each one of them.

2.2 Testing m =0

To test for the significance of m in (5) we state a two-sided test

$$H_0\text{: } m = 0$$

$$H_A\text{: } m \neq 0$$

This test is based on the t statistic associated with the OLS estimator of m in (5). This t statistic for m will be denoted by t_m.

In this case the distribution of t_m under the null ($m = 0$) depends on r. Dickey and Fuller (1981) obtained the limiting distribution of t_m statistic for $m = 0$ under the assumption that $r = 1$, which they denoted by $\hat{t}_{am}$ (it is bimodal and symmetric with 5 percent points well beyond 2). This limiting distribution holds for any Y_0 fixed and for e_t a sequence of independent identically distributed random variables, but it is non-standard and Dickey and Fuller (1981, p.1062) obtained percentage points for it by Monte Carlo methods. They gave empirical quantiles of the limiting and finite sample distributions.

The asymtotic theory for autoregression is developed in Fuller, Hasza, and Goebel (1981) assuming the innovations are i.i.d. $(0, s^2)$. By Theorem 2 in Fuller, Hasza, and Goebel (1981), when $|r| < 1$ the limiting distribution of t_m statistic is normal, whereas it follows from Theorem 4 in Fuller, Hasza, and Goebel (1981) that when $|r| > 1$ the limiting distribution of t_m statistic is normal if, and only if, e_t are normal independent $(0, s^2)$ random variables. For these reasons the Student's *t* with *T*–2 degrees provides a satisfactory approximate distribution of t_m for $r \neq 1$.

Therefore, the distribution of t_m statistic is influenced by the values of the parameter r and it yields a non-similar test of the hypothesis $m = 0$ in (5).

An approximation to the empirical density of Dickey-Fuller $\hat{t}_{am}$ statistic for fixed T (f_D), and the density of the Student's *t* with *T*–2 degrees of freedom (f_S) are plotted in Figure 2.

We note that the distribution of $\hat{t}_{am}$ is much larger than that of Student's *t* distribution, so that the critical values tabulated by Dickey and Fuller (1981, p.1062) are higher than those Student's *t* in absolute value at the same significance level.

In Figure 2, $DF_{a/2}$ denote the empirical quantile of the distribution of $\hat{t}_{am}$. Thus, $(-DF_{a/2}; DF_{a/2})$ is the acceptance region of the test

$$H_0: m = 0 \quad \text{(assuming } r = 1\text{)}$$

$$H_A: m \neq 0$$

On the other hand, $(-t_{T-2;\alpha/2}; t_{T-2;\alpha/2})$ is the acceptance region of the test

$$H_0:\ m = 0\ \text{(assuming}\ r \neq 1)$$

$$H_1:\ m \neq 0$$

We take into account the non-similarity of t_m statistic if we use simultaneously the two distributions plotted in Figure 2 to test the hypothesis $m = 0$. In this case, the critical values of these distributions establish five regions which are labelled A, B, C, D, and E.

Thus, C=$(-t_{T-2;\alpha/2}; t_{T-2;\alpha/2})$ is the acceptance region of the test based on the two distributions. Therefore, if the computed value of t_m statistic ($\hat{t}_m$ hereafter) lies in region C the null hypothesis is not rejected, that is, we conclude that $m = 0$ and r can be any real number.

If $\hat{t}_m$ lies in region A or in region E, it would lead to the rejection of the hypothesis that $m = 0$ since in these regions $|\hat{t}_m| > |DF_{\alpha/2}| > |t_{T-2;\alpha/2}|$. We would conclude that $m \neq 0$ and r can be any real number.

Finally, if the computed value of t_m statistic lies in region B = $(-DF_{\alpha/2},-t_{T-2;\alpha/2})$ or in region D = $(t_{T-2;\alpha/2}, DF_{\alpha/2})$ we can take one of two different decisions. Everything depends on the critical values we use:

a) On the basis of critical values tabulated by Dickey and Fuller the null hypothesis is not rejected since in these regions is $|\hat{t}_m| < |DF_{\alpha/2}|$. Therefore, we can not reject the hypothesis that $m = 0$ assuming $r = 1$.

b)The critical values of Student's t would lead to the rejection of $m = 0$ since $|\hat{t}_m| > |t_{T-2;\alpha/2}|$. We conclude that $m \neq 0$, and the most likely is that $r \neq 1$.

We note that the decision in regions B and D depends on what we know about parameter r.

However, r is also unknown, so we cannot draw any conclusions about r and m. To solve this situation we propose to test the joint hypothesis $(m, r) = (0, 1)$ using an F statistic.

The regions introduced in this section and the derived consequence when $\hat{t}_m$ lies in each one of them are reported in Table 2.

2.3 *Testing* $(m, r) = (0, 1)$

The statistic Φ_1 is the likelihood ratio test of $(m, r) = (0, 1)$ against the alternative $(m, r) \neq (0, 1)$ for model (1). It is an *F* statistic that is computed by ordinary least squares, but its distribution under the null hypothesis is not that of Snedecor's *F*.

Dickey and Fuller (1981, p.1063) have characterized the limiting distribution of Φ_1 when $r = 1$, $m = 0$ and have given the empirical quantiles of the limiting and finite sample distributions.

2.4 *A Testing Strategy*

From the previous discussion we propose the following strategy to test the unit root hypothesis in the context of model (5). First, we test the null hypothesis of a unit root using simultaneously the two distributions of t_{r_m} statistic mentioned in section 2.1. If the computed value of t_{r_m} statistic lies in region A or in E we conclude that $r \neq 1$. However, when this value is in region C we do not reject the null and differencing is necessary to achieve a stationary series. To solve the doubt that arises when the computed value of t_{r_m} statistic lies in region B or in D, we test the hypothesis that $m = 0$ against the alternative $m \neq 0$ for the model (5).

This test is based on t_m statistic and we use again a non-similar test which involves two distributions. If we cannot reject $m = 0$, then we test the unit root hypothesis again, but now using the critical values tabulated by Dickey and Fuller. If we reject $m = 0$, we conclude that $m \neq 0$ and we test the unit root hypothesis using the Student´s *t* distribution. Finally, if we cannot take a decision about *m* (that is, the computed value of t_m statistic lies in region B or in D) we test the joint hypothesis H_0: $(m, r) = (0,1)$ against H_A: not H_0 for the model (5) using Φ_1 statistic.

3 Monte Carlo Experiment

In order to compare the powers of the two-sided tests of the random walk hypothesis (DF*m* test) and of the random walk hypothesis with drift (DF*t* test) considered by Dickey and Fuller with the powers of the strategy, we develop a Monte Carlo experiment using the model

$$Y_t = m + rY_{t-1} + e_t \qquad t = 1,2,\ldots,T \tag{6}$$

with $Y_0 = 0$ and e_t ~NID(0,1).

Ten thousand samples of size $T = 50$, 100, 250 and 500 were generated for $r = 0.8$, 0.9, 0.95, 1.00, 1.05, 1.1, and 1.2, and $m = 0$, 0.5, 1, 2 and 10. DF tests and strategy were performed on each data series. All simulations were carried out using routines developed in Eviews 4.1 with the random number generator contained therein.

The experiment allowed us to obtain the empirical quantile functions of the t_{r_m} and t_m statistics of the hypothesis $r = 1$ and $m = 0$ in (6), respectively. Thus, we estimated the quantiles of t_{r_m} statistic for $r = 1$ for each of the values of *m*. For the case *m* = 0, the results confirm the estimated percentiles in Dickey (1976) and Fuller (1996). Likewise, our estimates of t_m statistic for $m = 0$ and $r = 1$ are the same as those reported in Dickey and Fuller (1981). Also, we calculated the percentage of times that the computed values of t_{r_m} and t_m lie in each one of the regions introduced in this paper. These results (available on request) are reported in Roldan (2000) and confirm that the tests based on t_{r_m} and t_m are non-similar.

4 Nominal size

Before comparing the behaviour of the strategy with the DFì and DF*t* tests, it is worth determining the nominal size of each of these, i.e., calculating in each case the maximum probability of rejecting the null hypothesis of unit root when this is true. Given that the distribution of the statistic t_{r_m} under this null hypothesis is not similar to the nuisance parameter ì, this probability will depend on the true value of the parameter. Therefore, in the case of the DFì test we must not expect the nominal size to coincide with the significance level á which was set in order to perform the test. In the case of the strategy, although we bear in mind the lack of similarity with the statistic t_{r_m} , the possibility of applying up to three tests in succession leads us to believe that neither will the nominal size coincide with the level of significance á which is fixed in each of the tests mentioned. Finally, for the DF*t* test it is not necessary to perform any calculations, since its similarity respect to ì allows us to claim that its nominal size will coincide with the level of significance á which has been set in order to perform the test.

In this study, we estimate the nominal size of both tests for a range of sample sizes ($T = 50$, 100, 250 and 500) and significance levels (a = 0.02, 0.05 and 0.1). Specifically, once some values of *T* and á have been set, we calculate the probability of rejecting the null hypothesis ($r = 1$) on the assumption that this is true (probability of committing a type I error (*P*(I)), for the various values of ì considered in the study, such that the nominal size estimated by these values of *T* and á will be the largest of the probabilities calculated.

4.1 Strategy

Since the strategy may require the successive application of up to three tests, the final decision to reject the null hypothesis may be the result of a chain of decisions. In such a case, therefore, the final probability of rejecting the null hypothesis of $r = 1$ will be the product of the probabilities of making these successive decisions. If there exist different sequences of decisions which lead to the final decision to reject $r = 1$, the total probability of rejection will be the sum of the probabilities of rejection associated with each of these sequences.

The probability of making a concrete decision in a sequence of decisions is the probability that the calculated value of the corresponding statistic (t_{r_m} , t_m or Φ_3) will fall within one of the specific zones established by the strategy. The probability is thus the area under the density function of the statistic, between the critical values that delimit the zone concerned.

Irrespective of which statistic is employed, the density function referred to in the previous paragraph is that which corresponds to the distribution of the statistic under the null hypothesis, i.e. its distribution for ñ = 1 and the value of ì that is being considered. For the calculation of nominal size, we have taken the distributions of these statistics which Dickey and Fuller obtained for $r = 1$ and $m = 0$, while for the cases of $r = 1$ and m = 0.5, 1, 2 and 10, the calculations are based on the distributions obtained empirically by the Monte Carlo experiment which we have described in detail in section 3.

4.2 DFì Test

The nominal size of the DFì test is estimated in a way similar to that described by the strategy. However, in this case it will only be necessary to consider the distribution of the statistic t_{r_m} in each specific case of $r = 1$ and $m = m_0$ since the DFì test only involves performing a test (that based on the statistic $\hat{t}_m$),

4.3 Results

Tables 3-6 illustrate, for each value of *T* and ì, and for a given level of significance á in each of the tests involved in the strategy, the estimates of the probability of rejecting, with this strategy, the null hypothesis of unit root when this is true, calculated as described in section 4.1.

These results demonstrate that, when $T = 50$ or 100, in cases where m = 0, 0.5 or 1, the probability of rejection is greater than the level of significance á that had been set. In the other cases, the probability is very similar to the value of á, i.e. it approaches á as ì grows for each value of *T*, as well as for each value of ì as *T* increases.

Similarly, Tables 7 to 10 show the estimates of rejecting the null hypothesis using the DFì test for the same values of *T*, á and ì. The first thing to note about Tables 7-10 is that DF*m* test is severely affected by the true value of the intercept *m* in the data-generation process. We can see that when $m = 0$, the estimated *P*(I) for this test is equal to the significance level a fixed for each *T*. This is not surprising since DF*m* test is based on $\hat{t}_m$ statistic which incorporates the knowledge that the true value of the intercept is zero. However, as the value of m increases, the P(I) estimates for each T become farther away from the value of a considered. These distortions do not dissapear with the increasing of T.

On the basis of the results shown in Tables 3-6 and 7-10 we have estimated the respective nominal sizes of the strategy and the DFì test for a range of values of T and á, in each case taking the largest probability of rejection of the null hypothesis of unit root when this is true for the various values of the nuisance parameter ì considered. These values are presented in Table 11.

In the case of the strategy, these results enable the researcher to know, for a given value of T and irrespective of the value of ì, the theoretical size with which he is working when he sets a given level of significance á in the three tests involved in the strategy. For example, if for $T = 100$, the researcher sets a value of $a = 0.02$ in the three tests, he will be working with a theoretical size estimate of 0.0563.

On the other hand, it can clearly be seen that in every case the DFì test produces a much larger size distortion than does the strategy. Furthermore, in the case of the DFì test, not only does this distortion not disappear when the sample size T increases, but it even increases slightly, apparently stabilizing (at around 0.27, 0.41 and 0.52, for values of a = 0.02, 0.05 and 0.10 respectively). In the strategy, meanwhile, the distortion practically disappears as T increases, since the estimated nominal size for $T = 500$ is very close to the significance level á (0.022, 0.06 and 0.134 for values of a = 0.02, 0.05 and 0.10 respectively).

5 Strategy vs. DF tests

5.1 Empirical size

Table 12 shows the Monte Carlo powers of two-sided size tests (the strategy and DF tests) for $r = 1$, i.e. the empirical probability of Type I error (P(I)) using 5% critical values for both tests.

In the case of the DFì test, these probabilities are identical to the theoretical probabilities presented in Tables 7-10 for $a = 0.05$. As far as the strategy is concerned, we can see that the empirical probabilities when $m = 0.5$ and $T = 50$ or 100 are much higher than the corresponding theoretical probabilities estimated for

$a = 0.05$ (Tables 3-6), while in the other cases the empirical and theoretical results are similar. Finally, we can see that the *P*(I) values estimated for the DF*t* test are very close to 0.05 in every case.

In Table 12, taking the maximum probability of Type I error for each *T* irrespective of the value of *ì*, we obtain the empirical size of the three tests for each value of *T* and a significance level of $a = 0.05$. These values are shown in Table 13, where we can see that the empirical size of the DF*ì* test is identical to the nominal size estimated (Table 11) for each *T*, always being above 0.37. In the case of the strategy, the empirical results are higher than the corresponding nominal results estimated for $a = 0.05$ (these values, which are always higher than 0.22, correspond to the cases $m = 0.5$ when $T = 50$ and 100, and $m = 0$ when $T = 250$ and 500). Finally, the empirical sizes for the DF*t* test are a consequence of the similar character of this test with respect to the nuisance parameter *ì*, whose nominal size in all the cases considered in the Monte Carlo experiment is 0.05.

Figure 3 is a graphical representation of the information shown in Table 13.

5.2 Monte Carlo powers

Tables 14 to 17 report the power calculations of DF tests and strategy. The most striking feature of these results is that the estimated powers of the strategy are greater than or equal to the estimated powers of the DF tests.

At the same time, it is quite clear that in all the alternatives, the least powerful of the three tests is the DF*t* test. This was only to be expected, since this test is based on estimating the model (6) with the inclusion of the irrelevant regressor *t*. The inclusion of this regressor means that the DF*t* test will be similar respect to *ì*, while it results in a considerable loss of power, particularly in the stable alternatives, with the other two tests always being more powerful. For this reason, we restrict ourselves to comparing the power of the strategy with that of the DF*ì* test.

Stable alternatives

At stable alternatives ($r < 1$) the powers of the DFm test and the strategy are strongly influenced by the value of the nuisance parameter m. As m increases the powers tend to unity at each sample size. Thus, we can see that at $m = 10$ the power of each test is 100 per cent at all stable alternatives. However, when m is near zero the powers of both tests are poor for small samples and values of r near but less than unity. For instance, at $r = 0.95$, $m = 0$ and for a sample size of $T = 100$, the strategy and DFm test only achieve a power of 0.142 and 0.0594 respectively.

On the other hand, the above mentioned convergence is more rapid the larger T, provided r is not very close to unity. For example, at $T = 500$ we can say that the powers of both tests are equal to unity at the stable alternatives when $m \geq 1$. However, for small values of T and r near but less than unity, the power is 100 per cent only when $m > 2$. For instance, at $T = 50$ the power of DFm test when $m = 2$ is below 0.9 at all alternatives. In this case, the strategy always performs better since the minimum estimated power is 0.9867 corresponding to $r = 0.95$. Finally, at $T = 100$ the velocity of convergence increases since the powers of both tests are practically 1 when $m = 2$.

We note that the convergence is more rapid in the case of the strategy for each r and T. This is particularly evident for $T = 50$ and $r = 0.9$, where the power of DFm test increases from 0.0535 to 0.086, 0.2384 and 0.8868 for $m = 0.5$, 1 and 2, respectively, while the sequence of estimated powers of the strategy at the same alternative is 0.062, 0.1868, 0.5855 and 0.9985. Similar results occur at $r = 0.8$ and 0.95.

Explosive alternatives

For $r > 1$ the powers of the DFm test and the strategy are much the same for all T and m. Specifically, for $r = 1.1$ and 1.2 the powers are aproximately equal to unity. Only, when $r = 1.1$, $m = 0$ and $T = 50$ are the powers of both tests slightly below 1 (0.9686 and 0.9699, respectively).

At the nearest alternative to unity ($r = 1.05$) the powers of the tests are, in general, very close to 1 for each value of *m* and *T* except at $m = 0$ and $T = 50$. In this case, the power of DF*m* test is only 0.6771 and the power of strategy is 0.7006.

5 Concluding comments

This paper has examined the unit root test in an AR(1) model with unknown intercept. We have analyzed the distributions of the usual *t* and *F* statistics for the model considered taking into account the property of non-similarity, and the influence of the values of the nuisance parameters has been evident.

A testing strategy based on the idea of the two distributions test considered by Nankervis and Savin (1985) has been proposed to test the unit root hypothesis in the context of the first-order autoregressive process with unknown intercept. It takes into account the non-similarity, and the critical values establish uncertain regions which can lead to a wrong decision if we do not use the correct distribution. However, these uncertain situations are solved testing for the significance of the intercept, and if a new doubt arises we apply an *F* test of the random walk hypothesis.

The two-sided test developed by Dickey and Fuller (1979) in the context of model (2) (DF*m* test) presents serious size distortion for each *T*. This distortion not only does not disappear when the sample size *T* increases, but it even increases slightly, apparently stabilizing (at around 0.27, 0.41 and 0.52, for values of a = 0.02, 0.05 and 0.10 respectively). In the strategy, meanwhile, the distortion is less serious and practically disappears as *T* increases, since the estimated nominal size for $T = 500$ is very close to the significance level *á* (0.022, 0.06 and 0.134 for values of a = 0.02, 0.05 and 0.10 respectively).

Monte Carlo simulations in this article show that, compared to the two-sided tests considered by Dickey and Fuller (1979), the strategy has superior power at stable alternatives. In particular, when the value of the intercept is very close to zero, the three tests have low power at stable alternatives near unity, even for samples as large as *T* = 100. As the value of the intercept increases, the power of three tests tends to 100 per cent, although the convergence of the strategy is much more rapid. This means that the strategy is better at detecting a false unit root hypothesis when the value of the intercept is not equal to zero, but less than 10. At explosive alternatives, the powers of three tests are much the same and equal to unity at almost all alternatives considered.

On the other hand, Monte Carlo experiment (with a significance level of $a = 0.05$) shows that the DF*m* test is identical to its nominal size estimated for each *T*, always being above 0.37. In the case of the strategy, the empirical size is around 0.22 for each *T*.

In respect to the size, the two-sided test proposed by Dickey and Fuller (1979) based on model (3) (DF*t* test) is the best because it is a similar test of the random walk hypothesis with drift. However, as the strategy is much more powerful than DF*t* test at all alternatives, we consider that, in general, the strategy is preferable for testing the unit root in (2).

A reasonable extension of this paper is to extend the strategy to model (3). In this respect we may refer to the work of Roldan (2000). The author shows that the strategy performs better than the two-sided test developed by Dickey and Fuller (1979) in the context of model (3) (DF*t* test). Specifically, the strategy is substantially more powerful at most alternatives of interest than the DF*t* test. Likewise, the strategy presents less size distortions.

Finally, it may be of interest to know the performance of the strategy respect to another unit root tests. We are investigating these comparisions and will be introduced in a subsequent paper.

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Figure 1: Density of t_{r_m} for $r = 1$ and T fixed when $m = 0$ and $m \neq 0$

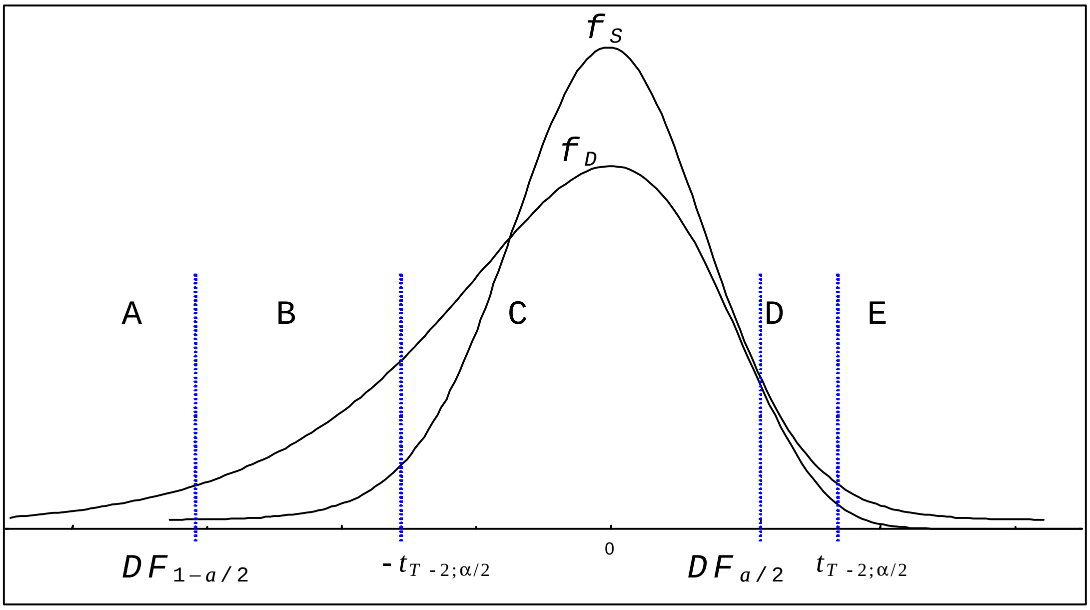

Figure 2: Density of t_m statistic for $m = 0$ and T fixed, when $r = 1$ and $r \neq 1$

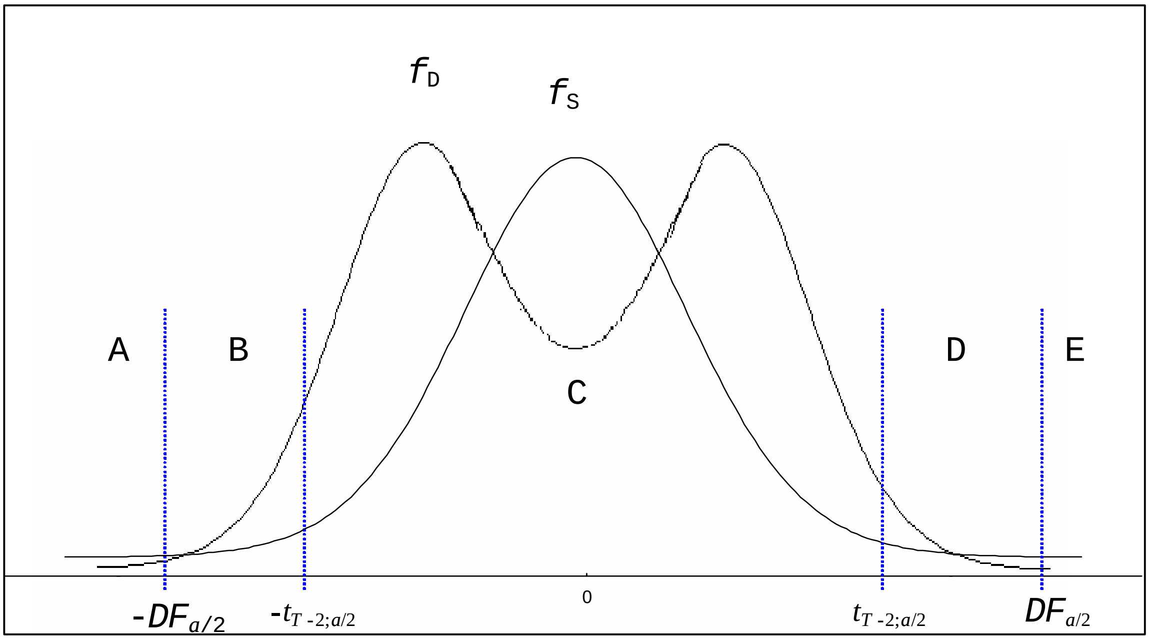

Figure 3: Empirical size

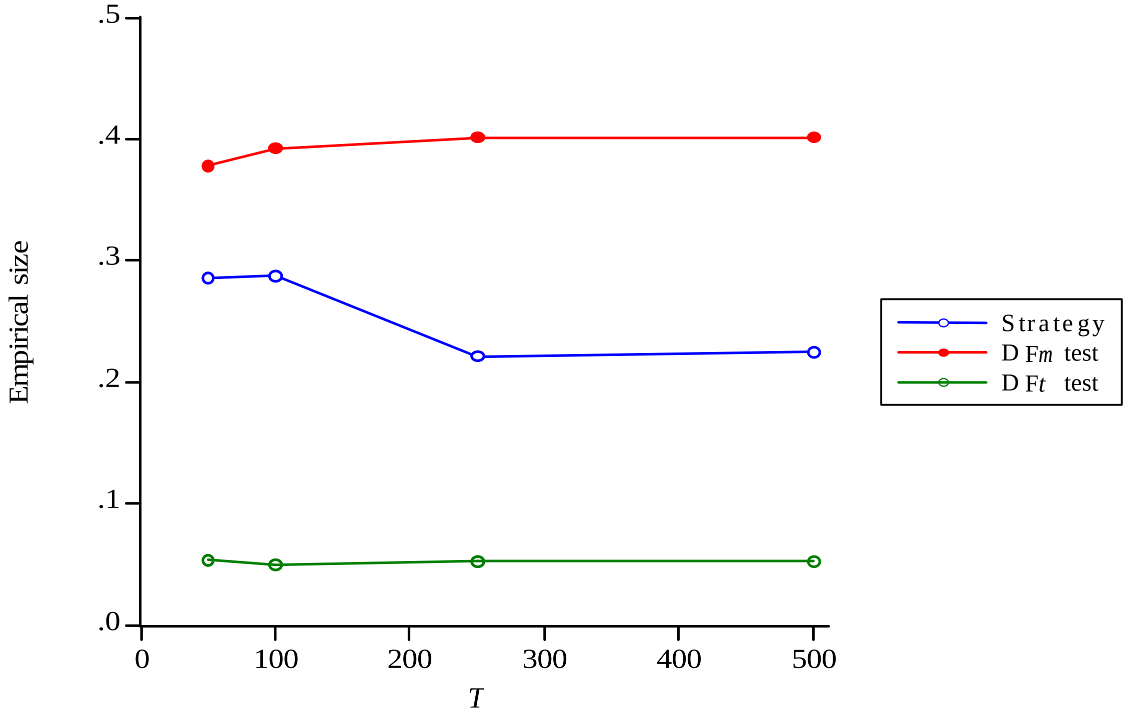

Table 1. Consequences depending on the region in which $\hat{t}_{r_m}$ lies

REGION	INTERVAL	CONSEQUENCE
A	$(-\infty; DF_{1-a/2})$	$r < 1$, m any
B	$(DF_{1-a/2}; -t_{T-2;a/2})$	$r = 1$ (assuming $m = 0$) or $r < 1$, $m \neq 0$
C	$(-t_{T-2;a/2}; DF_{a/2})$	$r = 1$, m any
D	$(DF_{a/2}; t_{T-2;a/2})$	$r = 1$ (assuming $m \neq 0$) or $r > 1$, $m \neq 0$
E	$(t_{T-2;a/2}; +\infty)$	$r > 1$, m any

Table 2. Consequences depending on the region in which $\hat{t}_m$ lies

REGION	INTERVAL	CONSEQUENCE
C	$(-t_{T-2;a/2};\ t_{T-2;a/2})$	$m = 0$, r any
A	$(-\infty;\ -DF_{a/2})$	$m \neq 0$, r any
E	$(DF_{a/2};\ +\infty)$	
B	$(-DF_{a/2};\ -t_{T-2;a/2})$	$m = 0$, $r = 1$ or $m \neq 0$, $r \neq 1$
D	$(t_{T-2;a/2};\ DF_{a/2})$	

Tables 3-6: Estimates of probability of Type I error for the strategy using the significance level *a*

Table 3

T= 50

m | *a*	**0.02**	**0.05**	**0.10**
0	0.0216	0.0592	0.1323
0.5	0.0803	0.1098	0.1423
1	0.0266	0.0564	0.1096
2	0.0186	0.0464	0.1005
10	0.0197	0.0487	0.0984

Table 4

T= 100

m | *a*	**0.02**	**0.05**	**0.10**
0	0.0218	0.0596	0.1323
0.5	0.0563	0.0739	0.1167
1	0.0217	0.0528	0.1024
2	0.0201	0.0509	0.1015
10	0.0192	0.0501	0.1013

Table 6

T= 250

m | *a*	**0.02**	**0.05**	**0.10**
0	0.0222	0.0600	0.1343
0.5	0.0262	0.0601	0.1141
1	0.0194	0.0522	0.1012
2	0.0179	0.0452	0.0948
10	0.0182	0.0477	0.0990

Table 6

T= 500

m | *a*	**0.02**	**0.05**	**0.10**
0	0.0220	0.0604	0.1348
0.5	0.0214	0.0505	0.0985
1	0.0215	0.0533	0.1025
2	0.0194	0.0503	0.1010
10	0.0215	0.0510	0.0973

Tables 7-10: Estimates of probability of Type I error for DFm test using the significance level a

Table 7

T= 50

m | a	**0.02**	**0.05**	**0.10**
0	0.0200	0.0500	0.1000
0.5	0.1293	0.2216	0.3223
1	0.1854	0.3029	0.4207
2	0.2199	0.3451	0.4653
10	0.2503	0.3780	0.4972

Table 8

T= 100

m | a	**0.02**	**0.05**	**0.10**
0	0.0200	0.0500	0.1000
0.5	0.1685	0.2770	0.3972
1	0.2098	0.3327	0.4523
2	0.2324	0.3619	0.4826
10	0.2588	0.3918	0.5191

Table 9

T= 250

m | a	**0.02**	**0.05**	**0.10**
0	0.0200	0.0500	0.1000
0.5	0.2009	0.3201	0.4389
1	0.2392	0.3730	0.4913
2	0.2486	0.3825	0.5058
10	0.2666	0.4011	0.5224

Table 10

T= 500

m | a	**0.02**	**0.05**	**0.10**
0	0.0200	0.0500	0.1000
0.5	0.2242	0.3441	0.4647
1	0.2498	0.3784	0.5040
2	0.2640	0.3919	0.5139
10	0.2654	0.4010	0.5264

Table 11: Estimated nominal size for strategy, DF*m* test and DF*t* test

$T \mid a^*$	**0.02**		**0.05**		**0.10**	
	Strategy	**DF*m***	**Strategy**	**DF*m***	**Strategy**	**DF*m***
50	0.0803	0.2503	0.1098	0.3780	0.1423	0.4972
100	0.0563	0.2588	0.0739	0.3918	0.1323	0.5191
250	0.0262	0.2666	0.0601	0.4011	0.1343	0.5224
500	0.0220	0.2654	0.0604	0.4010	0.1348	0.5264

*significance level

Table 12: Empirical probability of Type I error for strategy and DF tests

		m				
		0	0.5	1	2	10
T = 50	**Strategy**	0.2100	0.2853	0.1461	0.0464	0.0487
	DF*m* test	0.0471	0.2216	0.3029	0.3451	0.3780
	DF*t* test	0.0479	0.0533	0.0512	0.0490	0.0510
T = 100	**Strategy**	0.2135	0.2870	0.0544	0.0509	0.0501
	DF*m* test	0.0501	0.2770	0.3327	0.3619	0.3918
	DF*t* test	0.0477	0.0458	0.0496	0.0486	0.0494
T = 250	**Strategy**	0.2210	0.0975	0.0522	0.0452	0.0477
	DF*m* test	0.0528	0.3201	0.3730	0.3825	0.4011
	DF*t* test	0.0489	0.0521	0.0458	0.0480	0.0508
T = 500	**Strategy**	0.2242	0.0505	0.0533	0.0503	0.0510
	DF*m* test	0.0533	0.3441	0.3784	0.3919	0.4010
	DF*t* test	0.0479	0.0484	0.0502	0.0527	0.0506

Table 13: Empirical size*

T	**Strategy**	**DF*m* test**	**DF*t* test**
50	0.2853	0.3780	0.0533
100	0.2870	0.3918	0.0496
250	0.2210	0.4011	0.0521
500	0.2242	0.4010	0.0527

*significance level (a = 0.05)

Table 14: Monte Carlo Power of Two-Sided 0.05 Tests for $r = 1$ and $T = 50$ $(Y_0 = 0)$

	r = 0.8					*r* = 0.9					*r* = 0.95				
m	0	0.5	1	2	10	0	0.5	1	2	10	0	0.5	1	2	10
Strategy	0.2170	0.7293	0.9550	0.9993	1	0.1341	0.5725	0.8472	0.9982	1	0.1575	0.4496	0.6770	0.9870	1
DF test	0.1915	0.2240	0.3734	0.8870	1	0.0616	0.0869	0.2394	0.8916	1	0.0376	0.0614	0.1801	0.8072	1
DFt test	0.1116	0.1192	0.1592	0.3896	1	0.0473	0.0496	0.0677	0.1622	1	0.0410	0.0495	0.0702	0.0923	0.9796

Table 14: (cont.)

	r = 1.05					*r* = 1.1					*r* = 1.2				
m	0	0.5	1	2	10	0	0.5	1	2	10	0	0.5	1	2	10
Strategy	0.7006	0.9976	1	1	1	0.9699	0.9978	1	1	1	0.9993	1	1	1	1
DF test	0.6771	0.9973	1	1	1	0.9686	0.9973	1	1	1	0.9992	0.9999	1	1	1
DFt test	0.3890	0.9807	1	1	1	0.9538	0.9964	1	1	1	0.9991	0.9999	1	1	1

Table 15: Monte Carlo Power of Two-Sided 0.05 Tests for $r = 1$ and $T = 100$ $(Y_0 = 0)$

	r = 0.8					*r* = 0.9					*r* = 0.95				
m	0	0.5	1	2	10	0	0.5	1	2	10	0	0.5	1	2	10
Strategy	0.7160	0.9732	0.9996	1	1	0.2112	0.8775	0.9904	1	1	0.1420	0.7203	0.9620	1	1
DF test	0.7148	0.7744	0.8895	0.9982	1	0.1793	0.2656	0.5861	0.9985	1	0.0594	0.1257	0.5078	0.9991	1
DFt test	0.4629	0.5122	0.6187	0.9209	1	0.1055	0.1268	0.2201	0.6995	1	0.0472	0.0518	0.0848	0.3402	1

Table 15: (cont.)

	r = 1.05					*r* = 1.1					*r* = 1.2				
m	0	0.5	1	2	10	0	0.5	1	2	10	0	0.5	1	2	10
Strategy	0.9737	0.9999	1	1	1	0.9999	1	1	1	1	1	1	1	1	1
DF test	0.9716	0.9999	1	1	1	0.9999	1	1	1	1	1	1	1	1	1
DFt test	0.9596	0.9999	1	1	1	0.9999	1	1	1	1	1	1	1	1	1

Table 16: Monte Carlo Power of Two-Sided 0.05 Tests for $r = 1$ and $T = 250$ $(Y_0 = 0)$

	r = 0.8					*r* = 0.9					*r* = 0.95				
m	0	0.5	1	2	10	0	0.5	1	2	10	0	0.5	1	2	10
Strategy	1	1	1	1	1	0.9012	0.9999	1	1	1	0.3110	0.9860	0.9999	1	1
DF test	1	1	1	1	1	0.9011	0.9465	0.9945	1	1	0.2913	0.5099	0.9525	1	1
DFt test	0.9999	1	1	1	1	0.6842	0.7526	0.9044	0.9999	1	0.1562	0.2336	0.5399	0.9968	1

Table 16: (cont.)

	r = 1.05					*r* = 1.1					*r* = 1.2				
m	0	0.5	1	2	10	0	0.5	1	2	10	0	0.5	1	2	10
Strategy	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
DF test	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
DFt test	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1

Table 17: Monte Carlo Power of Two-Sided 0.05 Tests for $r = 1$ and $T = 500$ $(Y_0 = 0)$

	r = 0.8					*r* = 0.9					*r* = 0.95				
m	0	0.5	1	2	10	0	0.5	1	2	10	0	0.5	1	2	10
Strategy	1	1	1	1	1	1	1	1	1	1	0.8904	1	1	1	1
DF test	1	1	1	1	1	1	1	1	1	1	0.8904	0.9703	0.9999	1	1
DFt test	1	1	1	1	1	0.9999	0.9999	1	1	1	0.6692	0.8099	0.9813	1	1

Table 15: (cont.)

	r = 1.05					*r* = 1.1					*r* = 1.2				
m	0	0.5	1	2	10	0	0.5	1	2	10	0	0.5	1	2	10
Strategy	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
DF test	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
DFt test	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1