Bootstrapping and Bartlett corrections in the cointegrated VAR model

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Abstract

The small sample properties of tests on long-run coefficients in cointegrated systems are still a matter of concern to applied econometricians. We compare the performance of the Bartlett correction, the bootstrap and the fast double bootstrap for tests on ccointegration parameters in the maximum likelihood framework. We show by means of a theoretical result and simulations that all three procedures should be based on the unrestricted estimate of the cointegration vectors. The fast double bootstrap delivers superior size correction, whereas the Bartlett correction leads to the least loss of power. However all three perform much better than the asymptotic tests and difference between them are small.

1 Introduction[1]

Long-run relationships have always been a key concept in economic analysis. It is thus hardly surprising that the analysis of cointegration, providing an elegant solution to the problem of estimating of the number and form of such relationships, has rapidly reached the status of one of the main breakthroughs in the history of econometrics. However, the small sample properties of the available tests on long-run coefficients in cointegrated systems are still a matter of concern to applied econometricians; Monte Carlo evidence is presented, among others, by Gonzalo (1994), Bewley, Orden, Yang and

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Fisher (1994), Li and Maddala (1997). Given all sample sizes commonly used by applied economists can be considered "small", the conclusion is that, over ten years after Johansen (1991) introduced the asymptotic procedures now standard, no reliable method to test if the estimated long-run relationships are compatible with those suggested by economic theory exist. Obviously, some attempts have been made. More precisely, two natural and complementary solutions have been proposed: (i) applying Bartlett corrections to the test statistics, in the hope that the corrected statistic will follow a small sample distribution closer to the asymptotic one, and thus bring actual sizes closer to the nominal sizes (Johansen, 1999); (ii), trying to estimate the actual small sample distribution by the bootstrap, a computer-intensive technique strictly linked with the Edgeworth expansion and indeed defined by Cribari-Neto and Cordeiro (1996) "a simulation based alternative to Bartlett and Bartlett-type corrections" (Li and Maddala, 1996, 1997, Fachin, 2000, Gredenhoff and Jacobson, 2001). For the time being, no definite solution has however appeared. Although the only aim of both the bootstrap and the Bartlett correction is to get the actual size closer to the nominal size, the final aim of any testing procedure must be that of distinguishing between valid and invalid hypotheses: the proportion of Type II errors of corrected tests is therefore crucial. To the best of our knowledge no evidence on the power properties of Bartlett corrected tests in the cointegrated VAR model has appeared in the literature; the only available evidence on power for bootstrapped test statistics is in Fachin (2000), with the bootstrap test examined reported to have a rather high Type II error in parts of the parameter space. The aim of this paper is thus a thorough scrutiny of both the size and power properties of Bartlett-corrected and bootstrap tests. For the latter we shall consider both a standard procedure and the "fast double bootstrap", recently proposed by Davidson and MacKinnon (2000). As we shall see below, the main result is that small sample corrections appear to be an absolutely necessary addition to the toolkit of the econometrician working with non stationary data, with the fast double bootstrap performing best then the other alternatives. A key result of our analysis is that both the Bartlett correction and the bootstrap tests should be based on the unrestricted estimate of the cointegration vectors.

The paper is organised as follows: in Section 2 we shall briefly review the model, the structure of Bartlett-corrected and bootstrap tests, as well as a theoretical result, motivating us to base both procedures on unrestricted estimates. In section 3 we shall discuss the design of the Monte Carlo experiment and in section 4 present the results of the simulations. Some conclusions, as well as tentative recommendations for applied work, are finally drawn in section 4.

2 Bartlett-corrected and Bootstrap Tests on Cointegrating Coefficients

2.1 The Model

The cointegrated p-dimensional VAR model with k lags in its autoregressive form is defined as:

$$\Delta \mathbf{X}_t = \boldsymbol{\alpha}\boldsymbol{\beta}' \begin{pmatrix} X_{t-1} \\ D_t \end{pmatrix} + \sum_{i=1}^{k-1} \Gamma_i \Delta \mathbf{X}_{t-i} + \Psi \mathbf{d}_t + \varepsilon_t \tag{1}$$

In this paper a linear trend is constrained to lie in the cointegration space and an unrestricted constant is included outside that space: $D_t = t$ and $\mathbf{d}_t = 1$. We define γ and ρ by $\beta' = (\gamma', \rho')$, where γ includes the coefficients linking the stochastic variables of the system and ρ are the coefficients of the deterministic part.

Three assumptions are made to make sure this is a stable I(1) model:

Assumption 1 $\boldsymbol{\alpha}$ and $\boldsymbol{\gamma}$ are two full rank matrices of dimension $p \times r$, $p > r$;

Assumption 2 the matrix $\boldsymbol{\alpha}'_\perp \left(I - \sum_{i=1}^{k-1} \Gamma_i \right) \boldsymbol{\gamma}_\perp$ is of full rank;

Assumption 3 The roots z of the characteristic polynomial are either 1: $z = 1$ ($p - r$ roots are equal to unity) or larger than 1 in absolute value: $|z| > 1$.

The first two assumption assures that the process is an I(1) process and not integrated of higher order, while the second assumption excludes explosive behaviour.

The stationary, stochastic part of (1) can be written in the so-called companion form[2]:

$$\begin{bmatrix} \gamma' \mathbf{X}_t \\ \Delta \mathbf{X}_t \\ \vdots \\ \vdots \\ \Delta \mathbf{X}_{t-k+2} \end{bmatrix} = \begin{bmatrix} I_r + \gamma'\alpha & \gamma'\Gamma_1 & \cdots & \gamma'\Gamma_{k-2} & \gamma'\Gamma_{k-1} \\ \boldsymbol{\alpha} & \Gamma_1 & \cdots & \Gamma_{k-2} & \Gamma_{k-1} \\ \mathbf{0} & \mathbf{I}_n & & \mathbf{0} & \mathbf{0} \\ \vdots & & \ddots & & \vdots \\ \mathbf{0} & \mathbf{0} & & \mathbf{I}_n & \mathbf{0} \end{bmatrix} \begin{bmatrix} \gamma'_{t-1} \mathbf{X}_{t-1} \\ \Delta \mathbf{X}_{t-1} \\ \vdots \\ \vdots \\ \Delta \mathbf{X}_{t-k+1} \end{bmatrix} + \begin{bmatrix} \gamma' \\ \mathbf{I} \\ \mathbf{0} \\ \vdots \\ \mathbf{0} \end{bmatrix} \varepsilon_t$$

[2] The deterministic part can be taken account of by adding an extra term in d_t and D_t.

or

$$\mathbf{Y}_t = \mathbf{P}\mathbf{Y}_{t-1} + \mathbf{F}\boldsymbol{\varepsilon}_t \tag{2}$$

The Bartlett correction, which shall be discussed in the next section, depends crucially on the matrix P.

2.2 The Bartlett Correction

The idea behind the Bartlett correction (Bartlett, 1937) is both simple and appealing. Suppose the aim is testing an hypothesis on a subset $\boldsymbol{\theta}$ of the parameters Θ, $H_0 : \boldsymbol{\theta} = \boldsymbol{\theta}^0$. In regular cases, the LR test statistic S has an expected value of

$$E\left[-2\ln(LR)\right] = h\left(1 + \frac{1}{T}g\left(\boldsymbol{\theta}\right)\right) + O\left(\frac{1}{T^2}\right) \tag{3}$$

where h denotes the number of restrictions tested. Then dividing the test statistic S by $\left(1 + \frac{1}{T}g\left(\boldsymbol{\theta}\right)\right)$ we may obtain the modified test statistic S_B and expect the resulting distribution to be closer to a χ^2 distribution. This division is called a Bartlett correction and $\frac{1}{T}g\left(\boldsymbol{\theta}\right)$ will be referred to as the Bartlett factor.

We obviously do not know the true values of the parameters, $\boldsymbol{\theta}$, and thus we substitute a consistent estimate of $\boldsymbol{\theta}$, $\widehat{\boldsymbol{\theta}}$, in expression (3) and obtain:

$$E\left[-2\ln\left(LR\right)\right] = h\left(1 + \frac{1}{T}g\left(\widehat{\boldsymbol{\theta}}\right)\right) + O\left(\frac{1}{T^2}\right) \tag{4}$$

The error still remains of the same order as before, but later in this paper, we shall see that the actual size differs substantially depending on which consistent estimate we use.

Lawley (1956) and Barndorff-Nielsen and Hall (1988) proved that under certain regularity conditions (which exclude cointegrated VAR models) for any real number x

$$p\left(S_B \leq x\right) = p\left(\chi^2(h) \leq x\right) + O\left(\frac{1}{T^2}\right) \tag{5}$$

So the whole χ^2 is better approximated after the correction.

Jensen and Wood (1997) showed by means of an example that (5) does not hold in a non-stationary model. This however does not mean that the size correction is not useful in practice. In fact Nielsen (1997) showed that a Bartlett correction in an AR(1) process with a unit root, does provide an improvement to the size of the test.

Under the assumption:

Assumption (Bartlett Correction) there exist matrices K and M such that $d_t = Md_{t-1}$ and $\Delta D_t = Kd_t$ where all the eigenvalues of the matrix M equal 1 in absolute value

Johansen (2000) derived the Bartlett correction for three different kind of hypotheses on β in (4), namely:

1. $\beta = \beta^0$, a simple hypothesis on all the cointegration vectors;
2. $\beta_1 = \beta_1^0$ whereβ_1^0 are the first r_1 relations $(1 \leq r_1 < r)$ and the other cointegration relations are unrestricted;
3. $\gamma = H\varphi$ where H is a $(p \times r)$ matrix of full rank and $s < r$. This hypothesis implies the same restriction on all relations in γ.

Corrections for other kinds of hypotheses, like restrictions of the kind $\beta_1 = H_1\varphi_1$ do not yet exist. We therefore limit ourselves to confronting the corrections 1 and 2 with the bootstrap. Further, we exclude the case of structural breaks in the DGP requiring the use of dummy variables in the estimation.

The correction term itself, for which we refer to the aforementioned article, depends crucially on the total number of parameters, the variance of Y_t in (2) and a number of times on $\sum_{i=0}^{\infty} P^i$. The last two terms only exist if all the eigenvalues of P are strictly smaller than one. . When the eigenvalues approach 1, the Bartlett correction factor increases rapidly. If (2) contains a unit root, the Bartlett correction is not defined.

The non-existence of the Bartlett correction can cause large problems in practice, because of the following theorem:

Theorem 1 *The rank of the matrix $\hat{\alpha}$ converges to $r-1$, when under the null hypothesis $\boldsymbol{\beta} = \boldsymbol{\beta}^0$ one of the cointegration vectors is misspecified. Consequently the matrix P contains an additional unit root in the limit*

Proof. Let $\boldsymbol{\beta} = (\boldsymbol{\beta}_1, \boldsymbol{\beta}_2)$, where $\boldsymbol{\beta}_1 \quad n \times (r-1)$ and $\boldsymbol{\beta}_2 \quad n \times 1$ is the misspecified part of $\boldsymbol{\beta}$. Without loss of generality assume that $\boldsymbol{\beta}_1 \perp \boldsymbol{\beta}_2$ and test that $\boldsymbol{\beta} = b = (\boldsymbol{\beta}_1, \mathbf{b}_2)$.
$sp(\boldsymbol{\beta}) \cap sp(\mathbf{b}) = sp(\boldsymbol{\beta}_1)$. Next decompose $\mathbf{b}_2$ as $\mathbf{b}_2 = \boldsymbol{\beta}_{\perp}\mathbf{C} + \boldsymbol{\beta}_2\mathbf{E} =: \boldsymbol{\beta}_{\perp 1} + \boldsymbol{\beta}_2 d$ where $\mathbf{E}$ is a matrix of full rank.

The estimate of α given b is then found by regression:

$$\begin{aligned} \hat{\alpha}(\mathbf{b}) &= S_{01}\mathbf{b}\left(\mathbf{b}'S_{11}\mathbf{b}\right)^{-1} \\ &= S_{01}\left(\boldsymbol{\beta}_1, \boldsymbol{\beta}_{\perp 1} + \boldsymbol{\beta}_2 d\right)\begin{bmatrix} \boldsymbol{\beta}_1' S_{11}\boldsymbol{\beta}_1 & \boldsymbol{\beta}_1' S_{11}\left(\boldsymbol{\beta}_{\perp 1} + \boldsymbol{\beta}_2\mathbf{E}\right) \\ \left(\boldsymbol{\beta}_{\perp 1} + \boldsymbol{\beta}_2\mathbf{E}\right)' S_{11}\boldsymbol{\beta}_1 & \left(\boldsymbol{\beta}_{\perp 1} + \boldsymbol{\beta}_2\mathbf{E}\right)' S_{11}\left(\boldsymbol{\beta}_{\perp 1} + \boldsymbol{\beta}_2\mathbf{E}\right) \end{bmatrix}^{-1} \end{aligned}$$

where S_{01} and $S_{11}$are defined in standard fashion (Johansen, 1996, page 90-91). From Chan and Wei (1988), we find that $S_{01}\boldsymbol{\beta}_1, \boldsymbol{\beta}_1' S_{11}\boldsymbol{\beta}_1, \boldsymbol{\beta}_1' S_{11}\left(\boldsymbol{\beta}_{\perp 1} + \boldsymbol{\beta}_2\mathbf{E}\right), S_{01}\left(\boldsymbol{\beta}_{\perp 1} + \boldsymbol{\beta}_2\mathbf{E}\right) \in$

$O(1)$ and $(\boldsymbol{\beta}_{\perp 1} + \boldsymbol{\beta}_2 \mathbf{E})' S_{11} (\boldsymbol{\beta}_{\perp 1} + \boldsymbol{\beta}_2 \mathbf{E}) \in O(T)$ such that $\hat{\alpha}_1 \xrightarrow{P} \boldsymbol{\alpha}_1$ and $\hat{\boldsymbol{\alpha}}_2 \xrightarrow{P} 0$ ■

This means that if we use the restricted estimate and the null hypothesis is false, the Bartlett correction is not defined. In practise assumption 1 is violated, such that we get a different model. We therefore require the Bartlett corrected test to have the following properties:

- Whenever the null hypothesis is true, the estimator $\widehat{\boldsymbol{\theta}}$ should be consistent, such that (4) is valid
- Whenever the null hypothesis is false, the matrix P should have stable roots, such that the Bartlett correction is defined. If possible these roots should in some sense be as stable as possible, for when they are very large, the Bartlett Factor explodes and a false hypothesis is accepted.

Solution 2 *Use the unrestricted estimates* $\hat{\beta}$ *of* β*in* (1) *and not the restricted estimates in the Bartlett correction factor. The Bartlett correction factor for* $\beta = \beta^0$ *and* $\gamma = \mathbf{H}\boldsymbol{\varphi}$ *only depends on* $\hat{\beta}$ *, such that this defines the solution in these cases.*
In case 2 ($\boldsymbol{\beta}_1 = \boldsymbol{\beta}_1^0$*, only some of the cointegration relations are restricted) we need estimates for both the restricted and the unrestricted vectors. In this case* $\boldsymbol{\beta}_1^0$ *and the associated restricted estimate* $\boldsymbol{\beta}_2(\boldsymbol{\beta}_1^0)$ *should not be used, as this will lead to instability of the P matrix when* H_0 *is false. Instead, we find a matrix* $\mathbf{b}_1$ *for which* $sp(\mathbf{b}_1) \subset sp(\hat{\beta})$ *and as close to* $\boldsymbol{\beta}_1^0$ *as possible. This means that we find a matrix* ξ *such that*

$$\xi = \left(\hat{\beta}'\hat{\beta}\right)^{-1} \hat{\beta}' \boldsymbol{\beta}_1^0 \tag{6}$$

Then the estimators $\mathbf{b}_1 = \hat{\beta}\xi$ *and* $\mathbf{b}_2 = \hat{\beta}\xi_\perp$ *are consistent when the null hypothesis is true and the companion matrix* $\mathbf{P}$ *is stable when it is false.*

2.3 Bootstrap Methods

In principle, the great advantage of the bootstrap[3] is that it can offer immediate solutions to new problems. However, in practice its ability to deliver good alternatives when reliable small sample parametric procedures are lacking must be accurately tested before its use may be recommended. This is especially true for the problem we are trying to solve, as the asymptotics of the bootstrap applied to integrated data is still largely unexplored: Horowitz (2000) summarises his survey stating that "at present (...) there are no

[3] General introductions to the bootstrap are provided, *inter alia*, by Efron and Tibshirani (1993), Hall (1995) and Horowitz (2000), while a recent review especially addressed at time series applications is Berkowitz and Kilian (2000).

theoretical results on the ability of the bootstrap to provide asymptotic refinements for tests or confidence intervals when the data are integrated or cointegrated". An early exception is Basawa *et al.* (1991), while recent developments for a few specific cases in the line advocated by Horowitz are Chang, Sickles and Song (2001), Davidson (2001), Paparoditis and Politis (2001) and Inoue and Kilian (2002). At the opposite, a striking example of how blind implementations of the bootstrap can deliver entirely wrong results is given by Phillips (2001) for the case of spurious regression with integrated variables.

Let us now see how the bootstrap may be applied to our problem. The general idea underlying bootstrap tests is to assess the value of a test statistic s of an hypothesis of interest H_0 on the basis of the distribution of a large number of statistics s^* computed from suitably constructed pseudodata, with the null hypothesis of the former consistent with the data generating process (DGP) of the latter. Thus, two interrelated choices have to be made: (i) the bootstrap DGP*, i.e. the mechanism used to generate the pseudodata; (ii) the null hypothesis of the tests performed on the pseudodata (say, H_0^*). In order to ensure reciprocal consistency of the bootstrap DGP* and null hypothesis H_0^*, H_0 may be imposed onto the bootstrap DGP* (as in some examples in Efron and Tibshirani, 1993), or, vice versa, the bootstrap DGP* taken as H_0^* (as recommended by Hall, 1992). Although in the first case H_0 is the same for s and s^*, while in the latter the two statistics derive from different null hypothesis, the null hypothesis H_0^* is always true for the pseudodata, and thus, assuming for instance a one-sided test, the proportion of s^* more extreme than s in the relevant direction is a natural estimate of the probability that H_0 is true for the original data as well. However, a key point is that these two approaches, equivalent from the logical point of view, are not always such in practice[4]. Let us go into some detail. With cointegrated VARs and some hypothesis on the long-run coefficients $H_0 : \boldsymbol{\beta} = \boldsymbol{\beta}^0$, the two approaches entail respectively:

(a) estimating a *constrained* VAR satisfying $H_0 : \boldsymbol{\beta} = \boldsymbol{\beta}^0$, generating the pseudodata on the basis of the estimated *constrained* coefficients and a set of random noises (we will discuss the choice of these below), and testing $H_0 : \boldsymbol{\beta} = \boldsymbol{\beta}^0$ both on the original data and on the pseudodata, obtaining respectively the statistics s and s^*;

(b) estimating an *unconstrained* VAR, generating the pseudodata on the basis of the estimated *unconstrained* coefficients and a set of random noises, testing $H_0 : \boldsymbol{\beta} = \boldsymbol{\beta}^0$ on the original data (obtaining the statistic s) and $H_0^* : \boldsymbol{\beta} = \widehat{\boldsymbol{\beta}}$ (where $\widehat{\boldsymbol{\beta}}$ are the unconstrained estimates of $\boldsymbol{\beta}$) on the pseudodata.(obtaining the statistic s^*)

[4]Discussions especially relevant to our case are included in Li and Maddala (1997) and Giersbergen (1998).

In either case, the bootstrap estimate of the p-value of the test will be given, as described above, by $p^* = prop(s^* > s)$. So far, approach (a) has been favoured with no exception in the applications of interest here. However, a point of crucial importance for testing in the maximum likelihood estimation of cointegrated VARs seems to have gone unnoticed: although both approaches are valid and asymptotically equivalent under H_0, this is not true any more when it is false. To see this, consider the case of a test $H_0 : \boldsymbol{\beta} = \boldsymbol{\beta}^0$ in a model without lags and just one cointegration vector. If this vector is misspecified, then $\boldsymbol{\beta}^{0\prime} X_{t-1}$ is clearly an $I(1)$ process, whereas ΔX_t is $I(0)$. The only congruent values for the loading factors α are therefore zero. Hence all the element of the matrix $\hat{\Pi} = \hat{\alpha}\boldsymbol{\beta}^{0\prime}$ equal zero (asymptotically) and the rank of such a matrix is 0, not 1 as inthe original data. If one were to use this matrix for the bootstrap DGP*, one would generate just random walks without any cointegration (this is essentially a different version of exactly the same issue already discussed in section 2 with respect to the computation of the Bartlett factor when H_0 is false). Thus, only bootstrap tests of type (b), with DGP* given by the uncostrained estimates of the VAR coefficients and bootstrap null hypothesis $H_0^* : \boldsymbol{\beta} = \widehat{\boldsymbol{\beta}}$, appear to be admissable.

Remark 3 *This problem pertains to the maximum likelihood framework of Johansen and not to the triangular form of the system, considered inter alia by Li and Maddala (1996,1997) and Fachin (2000). In the Johansen framework short and long run dynamics are modelled contemporaneously: a false restrictions on the latter, lead to reduced rank of the former. In the triangular form, short run dynamics are modelled separately, such that none of the problems described above arise. Consider an Engle-Granger type regression of the form:*

$$y_t = \kappa x_t + \varepsilon_t$$

and the hypothesis $\kappa = \kappa_0$. *Then any bootstrap sample generated by the mechanism*

$$y_t^* = \kappa_0 x_t + \varepsilon_t^*$$

contains the cointegration relation $y_t^* - \kappa_0 x_t$, *as long as the errors are stationary (Phillips, 2001).*

So far we have discussed the choice of the structure of the systematic part of the bootstrap DGP. For the noise there are again essentially two alternatives, which are (i) some parametric hypothesis (typically, NID) or (ii) resampling from a set of residuals of a VAR. Given that resampling requires independence, a natural choice are the residuals of the unconstrained VAR, which will be empirically IID by construction. Gredenhoff and Jacobson (2001) favoured the parametric option, while Fachin (2000) and Li and Maddala (1997) the non-parametric one[5]. Here we will consider both alternatives. Block-resampling methods, such as the "Continuous-Path Block Bootstrap" proposed by Paparoditis and Politis (2001), which may be potentially powerful in dealing with the stochastic trends present in the system, will the subject of future research.

Defining Θ the entire parameter set of the VAR and assuming we are interested in running a one-sided test on a subset $\boldsymbol{\theta}$, with H_0: $\boldsymbol{\theta} = \boldsymbol{\theta}^0$, the general structure of the bootstrap test we shall implement is thus the following:

- *Bootstrap Test*

1. Estimate VAR on data $\mathbf{X}$; for given cointegrating rank obtain estimates $\widehat{\Theta}$, residuals $\widehat{\varepsilon}$, and test statistic s for the hypothesis $H_0 : \boldsymbol{\theta} = \boldsymbol{\theta}^0$;
2. Construct pseudodata: $X^* = \phi(\widehat{\Theta}, \varepsilon^*), \varepsilon^*$ drawn at random with replacement from $\widehat{\varepsilon}$ or NID.
3. Estimate VAR on pseudodata X^*; obtain coefficients $\widehat{\Theta}^*$ and test statistic s^* for the hypothesis H_0^*: $\boldsymbol{\theta} = \widehat{\boldsymbol{\theta}}$;

Repeat 2-3 a large number of times

4. Compute bootstrap p-value: $p^* = prop(s^* > s)$.

The test statistic is the likelihood ratio test (which is the only one allowing a Bartlett correction). If we have a simple hypothesis on the whole cointegration space, that is $H_0 : \boldsymbol{\beta} = \boldsymbol{\beta}^0$, we obtain the unrestricted estimate $\hat{\boldsymbol{\beta}}$ and use that for generating the pseudodata. For the likelihood ratio test in step 3. we test $H_0^* : \boldsymbol{\beta} = \hat{\boldsymbol{\beta}}$. If we have a simple hypothesis on only part of the cointegration space, $\boldsymbol{\beta}_1 = \boldsymbol{\beta}_1^0$, we take the following null hypothesis in step 3:

$$\beta_1 = \hat{\beta}'(\hat{\beta}'\hat{\beta})^{-1}\hat{\beta}'\boldsymbol{\beta}_1^0 \tag{7}$$

which is easily seen to converge to $\boldsymbol{\beta}_1^0$ if H_0 is true.

As mentioned in the introduction, Davidson and MacKinnon (2000) recently put forth a computationally cheap double bootstrap procedure which may deliver results superior to the standard bootstrap just outlined[6]. The

[5] Note that there is a possible source of confusion here, as the terms "parametric" and "non-parametric" have been used in the bootstrap literature with different meanings. We define procedures based on resampling from estimated residuals as "non parametric", and that involving drawings from a theoretical distribution as "parametric".

[6] Although Davidson and MacKinnon's analytical results are valid only for one-sided tests with asymptotic N(0,1) distributions, some simulation evidence suggests that the properties may extend to the asymptotic χ^2 of interest here.

idea behind the double bootstrap, proposed by Beran (1988), is that of correcting the possible bias in the bootstrap procedure implemented by a second application of the bootstrap. For instance, in the case of a test the aim of the second-level application of the bootstrap would be to estimate, and thus correct for, the bias $(p_i - i)$, where p_i is the p-value of the i-level bootstrap test. Although the principle is certainly attractive, it is also very expensive, as it involves the construction of a bootstrap pseudo-population for each bootstrap redraw. It is thus practically impossible to evaluate by means of Monte Carlo experiments with the currently available computing power. On the contrary, in Davidson and MacKinnon's method there is only one second level bootstrap redraw for each first level one, so that the computing time is of the same order of magnitude of the standard bootstrap. Monte Carlo experiments are thus feasible. Given that going into the details of the method is clearly beyond the scope of this paper, we shall just provide a basic intuition, which is very simple. Consider a one-sided test: if the bootstrap estimate $p^* = prop(s^* > s)$ of true p-value of the test is distorted, a way to obtain a better estimate is replacing s with some $\widetilde{s}$ chosen so to counterbalance the distortion. If for instance $p^* > p$, we may use an $\widetilde{s} > s$. The point is clearly how to choose $\widetilde{s}$: luckily, this turns out to be more natural than may be exptected. Considering that s is by definition the $p^* - th$ quantile of the distribution of the s^*, an obvious candidate for $\widetilde{s}$ is indeed the same quantile of the distribution of a *second-level* bootstrap distribution. If p^* is distorted upwards, such a quantile will tend to be larger than the true quantile s, and viceversa, thus delivering the desired effect. Going into the details of our case, the general structure of the fast double bootstrap test we shall implement turns out to be the following:

- *Fast Double Bootstrap Test*

1. Estimate VAR on data $\mathbf{X}$; for given cointegrating rank obtain estimates $\widehat{\Theta}$, residuals $\widehat{\varepsilon}$ and test statistic s for the hypothesis $H_0 : \boldsymbol{\theta} = \boldsymbol{\theta}^0$;

2. Construct pseudodata: $X^* = \phi(\widehat{\Theta}, \boldsymbol{\varepsilon}^*), \boldsymbol{\varepsilon}^*$ drawn at random with replacement from $\widehat{\varepsilon}$ or NID;

3. Estimate VAR on pseudodata X^*; obtain coefficients estimates $\widehat{\Theta}^*$, residuals $\widehat{\varepsilon}^*$ and test statistic s^* for the hypothesis $H_0^* : \boldsymbol{\theta} = \widehat{\boldsymbol{\theta}}$;

2a. Construct second-level pseudodata $X^{**} = \phi(\widehat{\Theta}^*, \boldsymbol{\varepsilon}^{**}), \boldsymbol{\varepsilon}^{**}$ drawn at random with replacement from $\widehat{\varepsilon}^*$ or NID;

3a. Estimate VAR on second-level pseudodata X^{**}; obtain coefficients estimates $\widehat{\Theta}^{**}$ and test statistic s^{**} for the hypothesis $H_0^* : \boldsymbol{\theta} = \widehat{\boldsymbol{\theta}}^*$;

Repeat 2-3a a large number of times

4. Compute bootstrap p-value: $p^* = prop(s^* > s)$.
5. Compute fast double bootstrap p-value type 1: $p_1^{**} = prop(s^* > Q_{p^*}^{**})$,where $Q_{p^*}^{**}$ is the p^* quantile of the $s^{**'}$s.

A (costless) further step is advisable:

6. Compute fast double bootstrap p-value type 2: $p_2^{**} = 2p^* - prop(s^{**} > s)$.

Again, the intuition here is that if for instance $p^* > p$, we can expect $prop(s^{**} > s) > p^*$, so that $(p_2^{**} - p) < (p^* - p)$. However, p_2^{**} may not be greater than $2p^*$ and it may be negative, two undesirable features that suggest limiting its use to a reliability check: if the difference between the two p-values is sizable neither of them should be trusted.

3 Design of the Monte Carlo Experiment

On the basis of the simulation results reported by Gredenhoff and Jacobson (2001) and Fachin (2000), the key characteristics of the DGP to be controlled in the experiments are the dimension of the system, i.e. number of variables and lags, and its long-run structure, i.e. number of the cointegrating relationships and the speed at which the system adjusts to them. Estimation of systems of higher dimension (both in terms of number of variables and lags) demand more from the data, and thus it is (ex-post) not surprising to see that both the asymptotic test and the bootstrap test proposed by Gredenhoff and Jacobson (2001) perform better in smaller systems. A crucial remark here is that the simple bivariate DGPs employed in virtually all simulation studies do suffer from loss of generality, a fact not suspected so far. The experimental design adopted here will thus generalize to a multivariate system the classical DGP used by a number of studies starting with Engle and Granger (1987), which allows an easy control of the speed of adjustment. We shall consider systems including $p = 5$ random variables and with $r = 1$ or 2 cointegrating relationships. Let $\mathbf{x}_t = [x_{1t} \dots x_{pt}]'$ be the column vector of the realizations of the random variables of interest at time $t = 1, \dots, T$, $\mathbf{u}_t = [u_{1t} \dots u_{pt}]'$ the errors, $\epsilon_t = [\varepsilon_{1t} \dots \varepsilon_{pt}]'$ the noise, whose stochastic structure will be discussed in detail below, and τ a time trend. Our DGP is then given by

$$\mathbf{G}\mathbf{x}_t + \boldsymbol{\rho}\tau = \mathbf{u}_t \tag{1a}$$

$$\Phi\mathbf{u}_t' = \epsilon_t \tag{1b}$$

with

$\mathbf{G} = [\boldsymbol{\gamma}_1 \ldots \boldsymbol{\gamma}_r]'$, $\boldsymbol{\gamma}_j = \left[\gamma_{j1} \ldots \gamma_{jk}\right]$, $\boldsymbol{\rho} = [\rho_1 \ldots \rho_k]'$,
and
$\Phi = diag(\boldsymbol{\phi})$, $\boldsymbol{\phi} = \left[\begin{array}{ccccc} \phi_1(L) & \phi_2(L) & \phi_3(L) & \phi_4(L) & \phi_5(L) \end{array}\right]$.

Although the Bartlett corrections do depend on the parameters of the system, in order to keep the size of the experiment within manageable dimensions in the size simulations the cointegrating coefficients will be kept fixed across trials to either zero or 1, with the vectors resembling quite closely those used by Haug (1996), while in the power simulations we shall consider a few values in the range $[0.5, 1.5]$. Given that we are using a full-information method we do not need to worry about endogeneity; we shall thus consider a very simple structure, with one stochastic trend (X_p) transmitted to the first r variables of the system, while the remaining $p - r - 1$ follow independent random walks. The details in the two cases are as follows:

(a) $r = 1$

$\boldsymbol{\gamma}_1 = \left[\begin{array}{ccccc} 1 & 0 & 0 & 0 & \beta_{15} \end{array}\right]$;
$\boldsymbol{\gamma}_2 = \left[\begin{array}{ccccc} 0 & 1 & 0 & 0 & 0 \end{array}\right]$;
$\boldsymbol{\gamma}_3 = \left[\begin{array}{ccccc} 0 & 0 & 1 & 0 & 0 \end{array}\right]$;
$\boldsymbol{\gamma}_4 = \left[\begin{array}{ccccc} 0 & 0 & 0 & 1 & 0 \end{array}\right]$;
$\boldsymbol{\gamma}_5 = \left[\begin{array}{ccccc} 0 & 0 & 0 & 0 & 1 \end{array}\right]$;
$\boldsymbol{\rho} = \left[\begin{array}{ccccc} 0.01 & 0 & 0 & 0 & 0 \end{array}\right]'$;
$\phi_1(L) = (1, \varphi_1 L, \ldots, \varphi_k L^k)$;
$\phi_2(L) = \phi_3(L) = \phi_4(L) = \phi_5(L) = (1, -L)$.

(b) $r = 2$

$\boldsymbol{\gamma}_1, \boldsymbol{\gamma}_3, \boldsymbol{\gamma}_4, \boldsymbol{\gamma}_5$ as in case (a);

$\boldsymbol{\gamma}_2 = \left[\begin{array}{ccccc} 0 & 1 & 0 & 0 & \beta_{25} \end{array}\right]$;
$\boldsymbol{\rho} = \left[\begin{array}{ccccc} 0.01 & 0.01 & 0 & 0 & 0 \end{array}\right]'$;
$\phi_1(L) = \phi_2(L) = (1, \varphi_1 L, \ldots, \varphi_k L^k)$;
$\phi_3(L) = \phi_4(L) = \phi_5(L) = (1, -L)$.

The values taken by the two free parameters β_{15} and β_{25} in the various experiments will be detailed below. The order k of the autoregressive polynomial governing the dynamic structure of the noise in the cointegrating relationships will be set to either 2 or 4; in the main block of experiments the sum of the coefficients (on which depends the spectral mass at zero frequency, governing the speed of adjustment to long-run equilibrium) will be kept fixed at $\phi = 0.7$ so to examine the performances of the tests in rather unfavourable conditions at the same avoiding regions too close to non-stationarity. The individual coefficients of the lag polynomial will be fixed at the following values, chosen so to have a large part of the adjustment taking place in the first periods:

(i) $k = 2 : \phi_j(L) = (1, -\frac{\varphi}{2}L, -\frac{\varphi}{2}L^2)$;

(ii) $k = 4$: $\phi_j(L) = (1, -\frac{\varphi}{2}L, -\frac{\varphi}{3}L^2, -\frac{\varphi}{16}L^3, -\frac{\varphi}{16}L^4)$;

where $j = 1, \ldots, r$.

Some simple considerations will allow great simplification of the design as far as the ε's are concerned. First of all, in previous work on the related topic of stationary VARs (Fachin and Bravetti, 1996) one of the authors of this paper found that the shape of the distribution of the shocks does not appear to have a significant impact on the performances of asymptotic procedures. Further, the expectation that with a full-information method, their covariance structure should not matter either has been confirmed in the case of a simple bivariate DGP by Fachin (2000). We shall thus assume $\varepsilon = [\varepsilon_1 \ldots \varepsilon_p] \sim MNID(0, \mathbf{I}_p)$. The last aspect to be discussed is sample size. In order to shed some light on both the performances which can be expected in empirical work and on the asymptotic properties of the tests we shall consider a base case $T = 100$, with a control experiment replicated with $T = 400$. Finally, the number of both Monte Carlo replications and bootstrap redrawings has been fixed to 500: on the basis of previous work and some pilot experiments we concluded that the gain in precision deliver by higher numbers of either was not worth the higher computing costs and longer calendar time required. At 0.05 the Monte Carlo standard error can thus be estimated to be about 0.010.

Although in principle both Wald and LR tests might be used, we shall limit the experiments to the latter in order to facilitate comparisons with other published results. The tests will be applied to the hypothesis that one or more of the cointegrating vectors are known.

The cointegrating vectors and trend coefficients will be fixed in the DGP ($\boldsymbol{\beta}_i = \begin{bmatrix} \boldsymbol{\gamma}_i & \boldsymbol{\rho}_i \end{bmatrix}$) and in the null hypothesis H_0 ($\boldsymbol{\beta}_i^0 = \begin{bmatrix} \boldsymbol{\gamma}_i^0 & \boldsymbol{\rho}_i \end{bmatrix}$) according to the following scheme:

- size simulations:

  $r = 1$, one tested vector:
  $\boldsymbol{\beta}_1^0 = \boldsymbol{\beta}_1 = \begin{bmatrix} 1 & 0 & 0 & 0 & 1 & 0.01 \end{bmatrix}$;

  $r = 2$, one tested vector:
  $\boldsymbol{\beta}_1^0 = \boldsymbol{\beta}_1 = \begin{bmatrix} 1 & 0 & 0 & 0 & 1 & 0.01 \end{bmatrix}$;
  $\boldsymbol{\beta}_2 = \begin{bmatrix} 0 & 1 & 0 & 0 & 1 & 0.01 \end{bmatrix}$;

  $r = 2$, two tested vectors:
  $\boldsymbol{\beta}_1^0 = \boldsymbol{\beta}_1 = \begin{bmatrix} 1 & 0 & 0 & 0 & 1 & 0.01 \end{bmatrix}$,
  $\boldsymbol{\beta}_2 = \boldsymbol{\beta}_2^0 = \begin{bmatrix} 0 & 1 & 0 & 0 & 1 & 0.01 \end{bmatrix}$;

- power simulations (main block; a power curve will also be computed for a specific case, see below):

$r = 1$, one tested vector:
$\boldsymbol{\beta}_1^0 = \begin{bmatrix} 1 & 0 & 0 & 0 & 1 & 0.01 \end{bmatrix},$
$\boldsymbol{\beta}_1 = \begin{bmatrix} 1 & 0 & 0 & 0 & 0.5 & 0.01 \end{bmatrix}$

$r = 2$, one tested vector:
$\boldsymbol{\beta}_1^0 = \begin{bmatrix} 1 & 0 & 0 & 0 & 1 & 0.01 \end{bmatrix},$
$\boldsymbol{\beta}_1 = \begin{bmatrix} 1 & 0 & 0 & 0 & 0.5 & 0.01 \end{bmatrix};$
$\boldsymbol{\beta}_2 = \left[\begin{bmatrix} 0 & 1 & 0 & 0 & 1 & 0.01 \end{bmatrix}\right];$

$r = 2$, two tested vectors:
$\boldsymbol{\beta}_1^0 = \begin{bmatrix} 1 & 0 & 0 & 0 & 1 & 0.01 \end{bmatrix},$
$\boldsymbol{\beta}_1 = \begin{bmatrix} 1 & 0 & 0 & 0 & 0.5 & 0.01 \end{bmatrix};$
$\boldsymbol{\beta}_2^0 = \begin{bmatrix} 0 & 1 & 0 & 0 & 1 & 0.01 \end{bmatrix},$
$\boldsymbol{\beta}_2 = \begin{bmatrix} 0 & 1 & 0 & 0 & 1 & 0.01 \end{bmatrix}$

Finally, in order to keep the mass of results within manageable limits we shall report results relative to a few specific cases for the case of rank = 1, one tested vector only (in other terms, the combination rank = 1, one tested vector, $T = 100$, $\phi = 0.7$, $k = 2$ will be taken as the reference case). More specifically, we will test the effect of a higher speed of adjustment ($\phi = 0.4$),larger sample size ($T = 400$) and richer dynamic structure of the VAR ($k = 4$) as well as compute a power curve considering for $\beta_{15} \in \{0.5, 0.75, 0.95, 1.05, 1.25, 1.50\}$, with $\beta_{15}^0 = 1$ as usual.

4 Results

Although the results of the simulations amount to a considerable mass, their essence is quite simple, and summarised in Table 1; in the following tables a few details are highlighted, with baseline results repeated in different tables in order to facilitate comparisons. In all the cases reported and discussed in this section the nominal significance level of the tests is always 5%, with results for different values available on request.

First of all, for the sample size $T = 100$, which may be considered representative of those typical in applied econometric work, the asymptotic tests has disastrous Type I errors, at times over 50%. All the alternative procedures (Bartlett correction, simple and fast double bootstrap) are able to reduce substantially the size distortion in all our experiments, but are unable to eliminate it: in the case of rank=1 the minimum rejection rate, delivered by the fast double bootstrap type 1, is 26%, while in the case of rank=2 and test on one vector the rejection rate of all bootstrap procedures and the Bartlett corrected test is 15%. The two types of fast double bootstrap p-values are always very close, confirming that the procedure is reliable in our context. The power loss from using the procedures with lower size distortion is acceptable, with the rejection rates always over 70%. This finding will be confirmed by the power curve reported in Table 6. To understand the point of basing both the bootstrap and Bartlett correction on the unrestricted estimates, a glance at the power curves in table 7 suffices: whereas the size performance is indeed slightly better than in table 6, the level of type II errors is unacceptably high, such that the power curves are almost flat. In the last row we report the number of cases where the highest estimated root is explosive: when the discrepancy between DGP and model becomes large, this percentage rises rapidly and corroborates theorem 1 of this paper[7].

For $T = 400$ all corrected tests achieve correct size and 100% power while the Type I error of the asymptotic test is still higher than the nominal size (cf. Table 2).

A key point from Table 1, is that the size performance of all test procedures of $H_0 : \boldsymbol{\beta}_1 = \boldsymbol{\beta}_1^0$ in a model with two vectors is markedly better than the hypothesis $H_0 : \boldsymbol{\beta} = \boldsymbol{\beta}^0$ in a model with one cointegrating vector.

Increasing the length of the VAR also has large adverse effects on the test (cf. Table 3): thus, contrary to somehow common wisdom and in accord with Abadir, Hadri and Tzavalis (1999), parsimony in the estimation of the VAR seems to be a rather important virtue.

How sensitive are the performances of the tests to the speed of adjustment to equilibrium? Unsurprisingly, the answer is, a lot. Cutting ϕ (the sum of the coefficients of the autoregressive polynomial describing the dynamics of the errors in the cointegrating relationships) from 0.7 to 0.4 causes generally a more than proportional fall of the Type I error (for instance, that of the fast double bootstrap type 1 falls from 26% to 10%).

Given the good results delivered by the bootstrapped tests, it is of some interest to check if using resampled or parametrically generated errors makes any difference. The results reported in Table 5 suggest that it does not, and thus the parametric bootstrap (easier to implement) may be adopted in practice. However, some caution is needed here, as in our experiments the

[7] $tr(\sum_{i=0}^{\infty} P^i)$ does not converge is P contains an explosive root. However computationally we use the standard formula $(\sum_{i=0}^{\infty} \lambda^i) = \frac{1}{1-\lambda}$ to calculate the Bartlett correction both in the convergent case (when it is valid) and the non-convergent case.

There is nothing, which prevents the Bartlett correction factor from being smaller than -1: this is a known problem in the literature. We assign a p-value of 1 to these cases. In all the published and unpublised simulations we did, this only happened in those of table 7.

same parametric hypothesis (normality) is used both in the generation of the Monte Carlo and bootstrap errors. Further research with different error processes for the Monte Carlo and bootstrap DGPs (for instance a leptocurtic error distribution in the DPG and resampling for a normal distribution) is needed.

Finally, a noteworthy finding is that the power curves of the all the variants of bootstrap tests are rather steep (table 6). Although these results are specific to a single signal/noise ratio, they do suggest that the risk of unacceptable power losses from using some type of bootstrap test rather than the asymptotic or Bartlett corrected tests is likely to be remote.

5 Conclusions

We have compared different variants of bootstrap and Bartlett-corrected tests in a DGP which is relatively unfavourable, but reproduces some features of real life empirical applications: a relatively large system (5 variables and 2 or 4 lags), and rather slow adjustment to long-run equilibrium. With such a complex DGP the caveats common to all simulation studies are even more important than usual. Our design depends on over 120 parameters, the vast majority of which had to be kept fixed across all experiments, and thus we must be extremely cautious in reaching any conclusion.

Further, the type of tests examined assumes full knowledge of the tested cointegrating vectors, a rare event in practice: however, they are the only tests for which the Bartlett correction is available. Indeed, the Bartlett correction has not been derived yet for many cases of strong empirical interest (e.g., hypotheses of the kind $\boldsymbol{\beta}_i = H_i\varphi_i$ and in general models with impulse dummies) and hence the bootstrap may in fact be the only alternative to the asymptotic p-values. With all these caveats, our recommendations are the following:

- *(i)* Asymptotic tests should be used in no circumstance;
- *(ii)* Bartlett-corrected tests may be used provided considerable caution is exercised, as their Type I error is often much larger than the nominal size;
- *(iii)* Bootstrap tests, with a somehow lower size distortion than the Bartlett corrected tests accompanied by limited power losses, may also be used; the fast double bootstrap of Davidson and MacKinnon (2000) delivers the best performance, and thus it appears to be a powerful tool for applied work, especially in the many cases when the Bartlett correction is not available.

We stress that both the Bartlett correction and the bootstrap should always be based on the unrestricted estimate of $\boldsymbol{\beta}$.

Among the many points that remain open, two are especially important: (a) the development of equivalent hypothesis, like (7) for $H_0 : \boldsymbol{\beta}_1 = \boldsymbol{\beta}_1^0$ for more general restrictions on the cointegrating vectors, like $\boldsymbol{\beta}_i = H_i\varphi_i$, with an accurate monte carlo study of their properties and (b) theoretical results on the asymptotics of the (fast double) bootstrap in cointegrated systems.

Table 1

Size and Power: summary results

1 to 2 cointegration vectors, test on 1 to 2 vectors

$\phi = 0.7$, $T = 100$, $k = 2$

rank, tested vectors	1,1		2,1		2,2	
Test	*size*	*power*	*size*	*power*	*size*	*power*
Asymptotic	66.0	99.0	39.2	97.6	68.6	98.2
Bartlett	35.8	92.2	15.8	79.2	33.2	82.2
Bootstrap	32.0	86.0	15.2	77.2	28.2	74.6
FDB_1	26.2	76.0	13.4	68.0	20.0	62.2
FDB_2	27.8	81.8	14.2	71.4	23.6	68.2

nominal significance level: 5%; FDB_i: Fast Double Bootstrap type i
power simulations:
case (1,1) $H_0 : \boldsymbol{\beta}_1^0 = \begin{bmatrix} 1 & 0 & 0 & 0 & 1 \end{bmatrix}$, DGP: $\boldsymbol{\beta}_1 = \begin{bmatrix} 1 & 0 & 0 & 0 & 0.5 \end{bmatrix}$
case (2,1): as case (1,1) with DGP: $\boldsymbol{\beta}_2 = \begin{bmatrix} 0 & 1 & 0 & 0 & 1 \end{bmatrix}$
case (2,2): as case (2,1) with $H_0 : \boldsymbol{\beta}_2^0 = \begin{bmatrix} 0 & 1 & 0 & 0 & 1 \end{bmatrix}$

Table 2

Increasing the sample size

1 cointegrating vector, test on 1 vector

$\phi = 0.7$, $T = 100$ and 400, $k = 2$

T	100		400	
Test	*size*	*power*	*size*	*power*
Asymptotic	66.0	99.0	11.0	100.0
Bartlett	35.8	92.2	5.6	100.0
Bootstrap	32.0	86.0	6.2	100.0
FDB_1	26.2	76.0	5.6	100.0
FDB_2	27.8	81.8	5.8	100.0

nominal significance level: 5%
power simulations: see Table 1

Table 3
Increasing the VAR length
1 cointegrating vector, test on 1 vector
$\phi = 0.7, T = 100, k = 2$ and 4

lags	2		4	
Test	*size*	*power*	*size*	*power*
Asymptotic	66.0	99.0	85.4	99.4
Bartlett	35.8	92.2	53.2	91.0
Bootstrap	32.0	86.0	39.2	82.0
FDB_1	26.2	76.0	32.4	68.8
FDB_2	27.8	81.8	35.6	74.4

nominal significance level: 5%
power simulations: see Table 1

Table 4
Increasing the speed of adjustment
1 cointegrating vector, test on 1 vector
$\phi = 0.7$ and 0.4, $T = 100$, $k = 2$

ϕ	0.7		0.4	
Test	*size*	*power*	*size*	*power*
Asymptotic	66.0	99.0	33.0	99.6
Bartlett	35.8	92.2	17.0	97.6
Bootstrap	32.0	86.0	14.2	96.8
FDB_1	26.2	76.0	10.8	94.4
FDB_2	27.8	81.8	11.8	95.6

nominal significance level: 5%
power simulations: see Table 1

Table 5
Parametric vs Non-parametric bootstrap
1 cointegrating vector, test on 1 vector
$\phi = 0.7, T = 100, k = 2$

Type of bootstrap	Parametric		Non-Parametric	
Test	*size*	*power*	*size*	*power*
Asymptotic	66.0	99.0	66.0	99.0
Bartlett	35.8	92.2	35.8	92.2
Bootstrap	32.0	86.4	32.0	86.0
FDB_1	25.0	76.2	26.2	76.0
FDB_2	27.0	80.2	27.8	81.8

nominal significance level: 5%
power simulations: see Table 1

Table 6: Power curve
$\phi = 0.7, T = 100, k = 2$

Test / β_{15}	*Asymptotic*	*Bartlett*	*Bootstrap*	FDB_1	FDB_2
0.5	99.0	92.2	86.0	76.9	81.8
0.6	99.0	91.8	85.8	76.6	81.8
0.7	99.0	91.2	86.6	76.6	81.6
0.8	99.0	91.4	86.4	76.0	81.6
0.9	98.8	88.8	83.6	73.8	78.0
0.92	98.8	86.4	80.8	72.0	75.6
0.94	98.0	83.2	75.6	65.2	70.2
0.96	93.8	72.2	66.4	54.6	58.6
0.98	81.4	50.6	43.4	35.2	37.8
1.0	*66.0*	*35.8*	*32.0*	*26.2*	*27.8*
1.02	81.6	50.6	45.4	38.0	41.8
1.04	94.4	71.6	63.6	56.6	58.8
1.06	98.6	81.2	76.0	65.6	69.0
1.08	99.2	84.6	79.6	70.4	74.2
1.1	99.0	86.6	81.8	72.2	77.4
1.2	99.4	89.6	85.8	76.8	80.4
1.3	99.4	90.6	86.8	77.8	81.4
1.4	99.6	91.0	87.6	77.4	82.2
1.5	99.6	91.0	88.0	78.6	82.2

nominal significance level: 5%

Table 7: Power curve
Bootstrap data constructed from the constrained VAR estimates
$\phi = 0.7, T = 100, k = 2$

Test / β_{15}	*Asymptotic*	*Bartlett*	*Bootstrap*	FDB_1	FDB_2	Explosive Roots *(% of simulations)*
0.5	99.0	11.4	15.8	10.2	12.2	34.8
0.6	99.0	14.0	17.8	9.6	12.6	30.4
0.7	99.0	20.2	18.0	10.0	12.2	23.4
0.8	99.0	23.2	18.4	10.6	14.0	12.6
0.9	98.8	15.8	22.2	12.2	14.4	0.2
1.0	*66.0*	*18.0*	*8.6*	*5.4*	*6.6*	*0*
1.1	99.0	16.4	21.4	12.8	14.6	0.6
1.2	99.4	23.0	18.4	10.4	13.8	9.2
1.3	99.4	21.6	18.4	9.2	13.2	20.4
1.4	99.6	18.4	16.6	9.8	12.0	28.8
1.5	99.6	15.4	16.8	8.8	12.0	32.2

nominal significance level: 5%

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