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A Bootstrap Stationarity Test for Predictive Regression Invalidity

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In order for predictive regression tests to deliver asymptotically valid inference, account has to be taken of the degree of persistence of the predictors under test. There is also a maintained assumption that any predictability in the variable of interest is purely attributable to the predictors under test. Violation of this assumption by the omission of relevant persistent predictors renders the predictive regression invalid, and potentially also spurious, as both the finite sample and asymptotic size of the predictability tests can be significantly inflated. In response, we propose a predictive regression invalidity test based on a stationarity testing approach. To allow for an unknown degree of persistence in the putative predictors, and for heteroscedasticity in the data, we implement our proposed test using a fixed regressor wild bootstrap procedure. We demonstrate the asymptotic validity of the proposed bootstrap test by proving that the limit distribution of the bootstrap statistic, conditional on the data, is the same as the limit null distribution of the statistic computed on the original data, conditional on the predictor. This corrects a long-standing error in the bootstrap literature whereby it is incorrectly argued that for strongly persistent regressors and test statistics akin to ours the validity of the fixed regressor bootstrap obtains through equivalence to an unconditional limit distribution. Our bootstrap results are therefore of interest in their own right and are likely to have applications beyond the present context. An illustration is given by reexamining the results relating to U.S. stock returns data in Campbell and Yogo (2006). Supplementary materials for this article are available online.

KEY WORDS: Conditional distribution; Fixed regressor wild bootstrap; Granger causality; Persistence; Predictive regression; Stationarity test.

1. INTRODUCTION

Predictive regression (hereafter PR) is a widely used tool in applied finance and economics, and forms the basis for Granger causality testing. A very common application is in the context of testing the linear rational expectations hypothesis. A core example of this is testing whether future (excess) stock returns are predictable (Granger caused) by current information, such as the dividend yield or the term structure of interest rates. Often it is found that the posited predictor variable (e.g., dividend yield) exhibits persistence behavior akin to a (near) unit root autoregressive process, while the variable being predicted (e.g., the stock return) resembles a (near) martingale difference sequence (m.d.s.).

In basic form, a test of predictability involves running an OLS regression of the variable being predicted, y_t say, on the lagged value of a posited predictor variable, x_t say, and testing the significance of the estimated coefficient on x_{t-1} using a standard regression t-ratio. Here, the null hypothesis is that y_t is unpredictable (in mean) from ex ante information; the alternative is that it is predictable from x_{t-1}. Cavanagh, Elliott, and Stock (CES; 1995) showed that when the innovation driving x_t is correlated with y_t (as is often thought to be case in practice, for example, the stock price is a component of both the return and the dividend yield), then these tests can be badly over-sized if x_t is a local to unit root process but critical values appropriate for the case where x_t is a pure unit root process are used. This over-size can be interpreted as a tendency toward finding spurious predictability in y_t, in that it is incorrectly concluded that x_{t-1} can be used to predict y_t when in fact y_t is unpredictable; see also Rossi (2005) for a discussion of related issues. Attempting to address this issue, CES discuss Bonferroni bound-based procedures that yield conservative tests, while Campbell and Yogo (CY; 2006) considered a point optimal variant of the t-test and employed confidence belts. Phillips (2014) proposed a

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modification to the test proposed in CY, which is asymptotically valid in the case where x_t can be either local-to-unity or stationary. Recently, Breitung and Demetrescu (BD; 2015) considered variable addition and instrumental variable (IV) methods to correct test size. Near-optimal PR tests can also be found in Elliott, Müller, and Watson (2015) and Jansson and Moreira (2006).

A misspecified PR of y_t on x_{t-1} (with nonzero slope) can also arise from these tests in cases where y_t is in fact predictable and is Granger-caused (possibly by the process $\{x_t\}$ and) by some other persistent process, $\{z_t\}$ say. The variable z_t might be a manifest variable or an unobserved latent variable. (We distinguish between Granger causality, defined by conditioning on counterfactual information sets that can be chosen to contain the past of the variable z, observable or not, and predictability as a pragmatic concept based on available observations. Where z_t is latent it cannot therefore be termed a predictor.) Here, and in the special case where x_{t-1} is an invalid predictor variable (because y_t is Granger-caused solely by $\{z_t\}$ and x_t is uncorrelated with z_t), it is known that the regression of y_t on x_{t-1} can lead to serious upward size distortions in the standard PR tests, with the same conclusion of spurious predictability of y_t by x_{t-1} as discussed earlier; see Ferson, Sarkissian, and Simin (2003a, b) and Deng (2014). More generally, where both $\{x_t\}$ and $\{z_t\}$ Granger-cause y_t, or x_t and z_t are correlated, a linear predictor of y_t by x_{t-1} would still be misspecified because it would be suboptimal with respect to quadratic loss, even if the optimal linear predictor based on observables might involve x_{t-1}. (Even where y_t is not Granger-caused by $\{x_t\}$ but z_t is a latent variable correlated with x_t, x_{t-1} would pick up some of the information from the past of z_t and so x_{t-1} would not be a spurious predictor variable.) Specifically, in this case the optimal linear predictor for y_t would involve the past of z_t (if z_t is a manifest variable), or further variables among the lags of both y_t and x_{t-1} (if z_t is latent). This fundamental misspecification problem in the estimated PR will affect all of the predictability tests discussed above.

We demonstrate theoretically and by means of simulations the potential for a misspecified PR of y_t on x_{t-1} to arise in the context of a model where x_t and z_t follow persistent processes, which we model as local-to-unity autoregressions, while modeling the coefficient on z_{t-1} as being local-to-zero. As a consequence, it is important to be able to identify, a priori, if y_t is Granger caused by some ignored $\{z_t\}$. Our approach involves testing for persistence in the residuals from a regression of y_t on x_{t-1}. Consequently, any effect that x_{t-1} may have on y_t, through the value of its slope coefficient in the putative PR, is eliminated from the residuals, and any persistence they display thereafter is attributable to the unincluded variable z_{t-1}, and would signal that the PR is misspecified. The test for PR misspecification we suggest is based on the co-integration tests of Shin (1994) and Leybourne and McCabe (1994), themselves variants of the stationarity test of Kwiatkowski et al. (KPSS; 1992). Although originally designed to detect pure unit root behavior in regression residuals, Müller (2005) showed that these tests also reject when near unit root behavior is present, making them well-suited to the testing scenario of this article.

An issue arising with our proposed test is that under its null hypothesis that z_{t-1} plays no role in the data-generating process [DGP] for y_t, its limit distribution depends on the local-to-unity parameter in the process for x_t, even though the residuals used are invariant to the coefficient on x_{t-1} in the DGP. In principle, this makes it difficult to control the size of the test. However, we show a bootstrap procedure which treats x_{t-1} as a fixed regressor (i.e., the observed x_{t-1} is used in calculating bootstrap analogs of our test statistic) can be implemented to yield an asymptotically size-controlled test. This *fixed regressor bootstrap* approach is not itself new to the literature and has been employed by, among others, Gonçalves and Kilian (2004) and Hansen (2000). Because many financial and economic time series are thought to display nonstationary volatility and/or conditional heteroscedasticity in their innovations, it is also important for our proposed testing procedure to be (asymptotically) robust to these effects. We therefore use a heteroscedasticity-robust variant of the fixed regressor bootstrap along the lines proposed by Hansen (2000). This uses a wild bootstrap scheme to generate bootstrap analogs of y_t. We show that our proposed fixed regressor wild bootstrap test has local asymptotic power against the same local alternatives that give rise to a misspecified PR of y_t on x_{t-1}.

We establish large-sample validity of our bootstrap method by showing that the limit distribution of the bootstrap statistic, conditional on the data, is the same as the limit null distribution of the statistic computed on the original data, conditional on the posited predictor variable. Our method of proof has wider applicability to other scenarios where a fixed regressor bootstrap is used with (near-) integrated regressors. For instance, our proof corrects an error in the bootstrap literature arising from Hansen (2000) who incorrectly suggested, in the context of a closely related test statistic, that for strongly persistent regressors the validity of the fixed regressor bootstrap is due to the coincidence of the unconditional null limit distribution of the original statistic with that of the limit distribution of the bootstrap statistic conditional of the data; actually, by following our proof, this coincidence can be seen not to occur for Hansen's statistic.

The article is organized as follows. Section 2 presents the maintained DGP and sets out the various null and alternative hypotheses regarding predictability of y_t by x_{t-1} and z_{t-1}. To aid lucidity, we consider a single putative predictor variable, x_t, and single unincluded variable, z_t, both with m.d.s. errors. Generalizations to richer model specifications are straightforward and discussed at various points. Section 3 details the asymptotic distributions of standard PR statistics under the various hypotheses, demonstrating the inference problems caused by unincluded persistent variables. Section 4 introduces our proposed test for PR invalidity, detailing its limit distribution and showing the validity of the fixed regressor wild bootstrap scheme in providing asymptotic size control. The asymptotic power of this procedure is also examined here and compared with the degree of size distortions associated with PR tests. Section 5 presents the results of a set of finite sample simulations investigating the size and power of our proposed bootstrap tests. An empirical illustration reconsidering the results pertaining to U.S stock returns data in CY is given in Section 6. Proofs and additional simulation results appear in a supplementary appendix.

We use the following notation: $\lfloor \cdot \rfloor$ is the floor function; $\mathbb{I}(\cdot)$ is the indicator function; $x := y$ ($x =: y$) means that x is defined by y (y is defined by x); $\overset{w}{\to}$ and $\overset{p}{\to}$ for weak convergence and convergence in probability, respectively. For a vector, x, $\|x\| := (x'x)^{1/2}$, the Euclidean norm. Finally, $\mathcal{D}^k := D_k[0, 1]$ is the space of right continuous with left limit (càdlàg) functions from [0, 1] to $\mathbb{R}^k$, equipped with the Skorokhod topology, and $\mathcal{D} := \mathcal{D}^1$.

2. THE MODEL AND PREDICTABILITY HYPOTHESES

The basic DGP we consider for observed y_t is

$$y_t = \alpha_y + \beta_x x_{t-1} + \beta_z z_{t-1} + \epsilon_{yt}, \qquad t = 1, \ldots, T, \tag{1}$$

where x_t and z_t satisfy

$$x_t = \alpha_x + s_{x,t}, \qquad z_t = \alpha_z + s_{z,t}, \qquad t = 0, \ldots, T \tag{2}$$

$$s_{x,t} = \rho_x s_{x,t-1} + \epsilon_{xt}, \quad s_{z,t} = \rho_z s_{z,t-1} + \epsilon_{zt}, \quad t = 1, \ldots, T, \tag{3}$$

where $\rho_x := 1 - c_x T^{-1}$ and $\rho_z := 1 - c_z T^{-1}$, with $c_x \geq 0$ and $c_z \geq 0$, so that x_t and z_t are unit root or local-to-unit root autoregressive processes. We let $s_{x,0}$ and $s_{z,0}$ be $O_p(1)$ variates. Following CES and to examine the asymptotic local power of the test procedures we discuss, we parameterize β_x and β_z as $\beta_x = g_x T^{-1}$ and $\beta_z = g_z T^{-1}$, respectively, which entails that when g_x and/or g_z are nonzero, y_t is a persistent, but local-to-noise process. (Notice that an observationally equivalent formulation of the model can be obtained by treating β_x and β_z as fixed constants but parameterizing the variances of ϵ_{xt} and ϵ_{zt} to be local-to-zero; see, in particular, the discussion following Equation (10) later. We choose the local-to-zero coefficient formulation for consistency with CES.)

Our interest lies in examining the behavior of predictability tests derived from the PR of y_t on x_{t-1} when y_t is generated by the DGP in (1)–(3) with $\beta_z \neq 0$, and subsequently developing tests for the null hypothesis that $\beta_z = 0$. In doing so, it is important to note that the motivating issue of spurious predictability of y_t by x_{t-1}, in the case where there is no correlation between x_{t-1} and z_{t-1}, arises whenever x_{t-1} and the unincluded z_{t-1} are both persistent processes. In the general case where no dependence restrictions are placed between x_{t-1} and z_{t-1}, the presence of z_{t-1} in (1) does not entail that x_{t-1} is a spurious predictor for y_t. Rather it implies that the PR of y_t on x_{t-1} alone is misspecified.

In the context of (1), z_{t-1} could be either an omitted manifest variable or an unobserved latent variable. An example of the latter is given by the case where y_t are (currency, commodity, or bond) returns and x_{t-1} is either the lagged forward premium (spot minus forward price/rate) or a lagged futures basis (spot minus futures price/rate). Here, there is an unobserved latent risk premium which is believed to be strongly persistent, and which in combination with the strongly persistent predictor has been suggested as a possible driver for empirically unorthodox findings, such as the well-known forward premium (or Fama) puzzle; see Gospodinov (2009). A second example is provided by the long-run risk model of Bansal and Yaron (2004). Certain versions of their model can be rewritten as PRs for returns with an unobserved long-run persistent component in consumption. In the latent case, it would also be quite reasonable to view z_t not through a literal interpretation of the DGP in (1)–(3) but rather as a general proxy for underlying misspecification in the PR, under which interpretation it would clearly not make sense for z_t to be stationary rather than persistent. Possible examples are provided by the case where the coefficient on x_{t-1} displays time-varying behavior, such as has been considered in, for example, Paye and Timmermann (2006) and Cai, Wang, and Wang (2015), or where the data on x_t are observed with a strongly persistent measurement error driven by relatively low variance innovations.

The innovation vector $\epsilon_t := [\epsilon_{xt}, \epsilon_{zt}, \epsilon_{yt}]'$ is taken to satisfy the following conditions:

Assumption 1. The innovation process ϵ_t can be written as $\epsilon_t = HD_t e_t$ where:

(a) H and D_t are the 3×3 nonstochastic matrices

$$H := \begin{bmatrix} h_{11} & 0 & 0 \\ h_{21} & h_{22} & 0 \\ h_{31} & h_{32} & h_{33} \end{bmatrix}, \qquad D_t := \begin{bmatrix} d_{1t} & 0 & 0 \\ 0 & d_{2t} & 0 \\ 0 & 0 & d_{3t} \end{bmatrix}$$

with $h_{ij} \in \mathbb{R}$, $h_{ii} > 0$ $(i, j = 1, 2, 3)$, and HH' strictly positive definite. The volatility terms d_{it} satisfy $d_{it} = d_i(t/T)$, where $d_i \in \mathcal{D}$ are nonstochastic, strictly positive functions.

(b) e_t is a 3×1 vector martingale difference sequence (m.d.s.) with respect to a filtration $\mathcal{F}_t$, to which it is adapted, with conditional covariance matrix $\sigma_t := E(e_t e_t' | \mathcal{F}_{t-1})$ satisfying: (i) $T^{-1} \sum_{t=1}^T \sigma_t \xrightarrow{p} E(e_t e_t') = I_3$; (ii) $\sup_t E\|e_t\|^{4+\delta} < \infty$ for some $\delta > 0$.

Remark 1. Assumption 1 implies that ϵ_t is a vector m.d.s. relative to $\mathcal{F}_t$, with conditional variance matrix $\Omega_{t|t-1} := E(\epsilon_t \epsilon_t' | \mathcal{F}_{t-1}) = (HD_t)\sigma_t (HD_t)'$, and time-varying unconditional variance matrix $\Omega_t := E(\epsilon_t \epsilon_t') = (HD_t)(HD_t)'$. Stationary conditional heteroscedasticity and nonstationary unconditional volatility are obtained as special cases with $D_t = I_3$ (constant unconditional variance, hence only conditional heteroscedasticity) and $\sigma_t = I_3$ (so $\Omega_{t|t-1} = \Omega_t = \Omega(t/T)$, only unconditional nonstationary volatility), respectively. (The assumption that $E(e_t e_t') = I_3$ made in part (b) (i) and the parameterization of the unconditionally homoscedastic case by $D_t = I_3$ are without loss of generality, by nonidentification considerations.) As discussed in Cavaliere, Rahbek, and Taylor (2010), Assumption 1(a) implies that the elements of Ω_t are only required to be bounded and to display a countable number of jumps, therefore allowing for an extremely wide class of potential models for the behavior of the variance matrix of ϵ_t, including single or multiple variance or covariance shifts, variances which follow a broken trend, and smooth transition variance shifts.

Remark 2. Under Assumption 1, an identification issue regarding the parameters β_x, β_z, and h_{21} arises in the case where $c_x = c_z$. In this case, whenever the observables (y_t, x_t) satisfy (1) for certain $\beta_x, \beta_z \neq 0$, and z_t, they also satisfy (1) for $\beta_x^\lambda = \beta_x + \lambda$, $\beta_z^\lambda = \beta_z$, and $z_t^\lambda = z_t - \lambda\beta_z^{-1} x_t$, for any λ, where z_t^λ is a (local-to-) unit root autoregressive process and its innovations $\epsilon_{zt}^\lambda = \epsilon_{zt} - \lambda\beta_z^{-1}\epsilon_{xt}$ are such that $[\epsilon_{xt}, \epsilon_{zt}^\lambda, \epsilon_{yt}]'$ satisfies Assumption 1, upon a redefinition of the matrix H. In particular, if $\beta_z \neq 0$, then it is possible to choose $\lambda = h_{21} h_{11}^{-1} \beta_z$ such that ϵ_{xt} and ϵ_{zt}^λ, the innovations driving x_t and z_t^λ, respectively, are uncorrelated. In accordance with OLS identification conditions, we will discuss the predictive implications of (1) under the identifying condition $E(\epsilon_{xt}\epsilon_{zt}) = 0$ (equivalently, $h_{21} = 0$) if $\beta_z \neq 0$, and under the condition $\beta_z = 0$ otherwise. In the case where z_t is a named latent variable (such as an unobserved risk premium) or a manifest variable, the value of $E(\epsilon_{xt}\epsilon_{zt})$ is implicitly fixed by the choice of z_t and an alternative is to discuss (1) by using this value for identification.

Remark 3. We notice that a PR based on x_{t-1} alone is misspecified whenever $\beta_z \neq 0$, regardless of the value of either β_x or the correlation between ϵ_{xt} and ϵ_{zt}. If $h_{21} = 0$, x_{t-1}, and z_{t-1} would be uncorrelated with one another and any conclusion of predictability from the PR of y_t on x_{t-1} in the case where $\beta_x = 0$ and $\beta_z \neq 0$ in (1) would be purely spurious because the best linear predictor (BLP; with respect to symmetric quadratic loss) of y_t given the past of $\{y_t, x_t\}$ would not involve x_{t-1}, although the BLP with respect to a larger information set might involve x_{t-1}. When $h_{21} \neq 0$, x_{t-1}, and z_{t-1} are correlated, and thus, for forecasting purposes, x_{t-1} could act as a proxy for the information in z_{t-1}. Nonetheless, if $\beta_z \neq 0$, the BLP of y_t would not be a function of x_{t-1} alone: for a manifest variable z_t, the BLP given the past of $\{y_t, x_t, z_t\}$ would involve z_{t-1}, whereas for a latent variable z_t, the BLP given the past of $\{y_t, x_t\}$ would involve lags of y_t and x_t (even if $\beta_x = 0$, as some of the predictive power of z_{t-1} would be picked up by x_{t-1}).

Remark 4. For transparency, the structure in (1)–(3) is exposited for a scalar variable, z_t. This is without loss of generality, as one may consider that $z_t = \gamma' z_t^*$ where z_t^* is a vector of variables, which might therefore contain both omitted manifest and latent variables.

We are now ready to discuss, in the context of (1), the possibilities for the predictability and causation of y_t by the variables x_{t-1} and z_{t-1}, focusing on linear predictors. One potential case that has received much attention in the literature is that where y_t is Granger-caused only by the process $\{x_t\}$, so that it is predictable only by x_{t-1}, implying that $\beta_x \neq 0$ while $\beta_z = 0$ in (1). This forms the alternative hypothesis in the PR tests discussed in Section 3, where the corresponding null is that $\beta_x = 0$, and, in the context of our model, the maintained hypothesis that $\beta_z = 0$, so that y_t is unpredictable under the null. However, it is also a possibility that y_t is Granger-caused only by the process $\{z_t\}$, unincluded in the PR. In this case, $\beta_x = 0$ and $\beta_z \neq 0$, thereby violating the aforementioned maintained hypothesis, and a PR of y_t on x_{t-1} alone would be misspecified, regardless of whether z_t is a manifest or latent variable (see Remark 3). In the special case where $h_{21} = 0$ and x_{t-1} does not enter the BLP of y_t, a conclusion to the contrary is an instance of spurious predictability. A final possibility is that $\beta_x \neq 0$ and $\beta_z \neq 0$ so that y_t is Granger-caused by both processes $\{x_t\}$ and $\{z_t\}$. In this last case if z_t was an omitted manifest variable then a correctly specified PR could be obtained by including z_{t-1} in the PR. If, on the other hand, z_t was a latent variable, a correctly specified BLP of y_t would include more observables (e.g., y_{t-1}) than x_{t-1}. We summarize these four cases using the following taxonomy of hypotheses within the context of DGP (1):

$H_u : \beta_x = 0, \quad \beta_z = 0 \quad$ y_t is unpredictable (in mean)

$H_x : \beta_x \neq 0, \quad \beta_z = 0 \quad$ y_t is Granger-caused by $\{x_t\}$ alone

$H_z : \beta_x = 0, \quad \beta_z \neq 0 \quad$ y_t is Granger-caused by $\{z_t\}$ alone

$H_{xz} : \beta_x \neq 0, \quad \beta_z \neq 0 \quad$ y_t is Granger-caused by $\{x_t\}$ and $\{z_t\}$.

In hypothesis testing terms, standard PR tests attempt to distinguish between the null H_u and the alternative H_x. Here, we consider the impact of the presence of z_{t-1} in the DGP on such tests, that is, we investigate the behavior of PR tests of H_u against H_x when in fact H_z or H_{xz} is true. In addition, we propose a test for possible PR invalidity, where the appropriate composite null is H_u or H_x (H_u, H_x), and the alternative H_z or H_{zx} (H_z, H_{zx}).

We end this section by stating some implications of Assumption 1 for our asymptotic analysis. Associated with a standard Brownian motion $B = [B_1, B_2, B_3]'$ in $\mathbb{R}^3$, let $B_\eta = [B_{\eta 1}, B_{\eta 2}, B_{\eta 3}]'$ be the heteroscedastic Gaussian motion defined by $B_{\eta i}(r) := f_i^{-1/2} \int_0^r d_i(s) dB_i(s)$, $r \in [0, 1]$, where $f_i := \int_0^1 d_i(s)^2 ds$, $i = 1, 2, 3$. We can also write $B_{\eta i} \overset{d}{=} B_i(\eta_i)$, $i = 1, 2, 3$, where η_i denotes the variance profile $\eta_i(r) := f_i^{-1} \int_0^r d_i(s)^2 ds$, $r \in [0, 1]$, such that $B_{\eta i}$ is a time-changed Brownian motion; see, for example, Davidson (1994, p. 486). In particular, $\eta_i(r) = r$, $r \in [0, 1]$, under unconditional homoscedasticity. Then the following functional weak convergence result holds in $\mathcal{D}^3 \times \mathbb{R}^{3\times 3}$, by Lemma 1 of Boswijk et al. (2016):

$$\left(T^{-1/2} \sum_{t=1}^{\lfloor Tr \rfloor} \epsilon_t, T^{-1} \sum_{t=1}^{T} \sum_{s=1}^{t-1} \epsilon_s \epsilon_t' \right) \xrightarrow{w} \left(M_\eta(r), \int_0^1 M_\eta(s) dM_\eta(s)' \right), \quad r \in [0, 1], \tag{4}$$

where $M_\eta := [M_{\eta x}, M_{\eta z}, M_{\eta y}]' := HF^{1/2} B_\eta$ for the diagonal matrix $F := \mathrm{diag}\{f_1, f_2, f_3\}$. Let $\Omega_\eta := \{\omega_{ab}\}_{a,b \in \{x,y,z\}} := \mathrm{var}\{M_\eta(1)\} = HFH'$, which in the unconditionally homoscedastic case $D_t = I_3$ reduces to

$$HH' = \begin{bmatrix} h_{11}^2 & h_{11}h_{21} & h_{11}h_{31} \\ h_{11}h_{21} & h_{21}^2 + h_{22}^2 & h_{21}h_{31} + h_{22}h_{32} \\ h_{11}h_{31} & h_{21}h_{31} + h_{22}h_{32} & h_{31}^2 + h_{32}^2 + h_{33}^2 \end{bmatrix}$$

$$=: \begin{bmatrix} \sigma_{xx} & \sigma_{xz} & \sigma_{xy} \\ \sigma_{xz} & \sigma_{zz} & \sigma_{zy} \\ \sigma_{xy} & \sigma_{zy} & \sigma_{yy} \end{bmatrix} =: \Omega.$$

It will prove convenient to define the two Ornstein–Uhlenbeck-type processes $M_{\eta c,u}(r) := \int_0^r e^{(s-r)c_u} dM_{\eta u}(s)$ for $u = x, z$ and $r \in [0, 1]$, along with the standardized analogs $B_{\eta c,u}(r) := \omega_{uu}^{-1/2} M_{\eta c,u}(r)$ and their demeaned counterparts $\bar{B}_{\eta c,u}(r) := B_{\eta c,u}(r) - \int_0^1 B_{\eta c,u}(s)$.

3. ASYMPTOTIC BEHAVIOR OF PREDICTIVE REGRESSION TESTS

To fix ideas, as in CES, we first consider the basic PR test of H_u against H_x, based on the t-ratio for testing $\beta_x = 0$ in the fitted linear regression

$$y_t = \hat{\alpha}_y + \hat{\beta}_x x_{t-1} + \hat{\epsilon}_{yt}, \qquad t = 1, \ldots, T. \tag{5}$$

The test statistic is given by

$$t_u := \frac{\hat{\beta}_x}{\sqrt{s_y^2 / \sum_{t=1}^T (x_{t-1} - \bar{x}_{-1})^2}}, \quad \hat{\beta}_x := \frac{\sum_{t=1}^T (x_{t-1} - \bar{x}_{-1}) y_t}{\sum_{t=1}^T (x_{t-1} - \bar{x}_{-1})^2}$$

and $s_y^2 := (T-2)^{-1}\sum_{t=1}^{T}\hat{\epsilon}_{yt}^2$, with $\bar{x}_{-1} := T^{-1}\sum_{t=1}^{T} x_{t-1}$.

In addition to the t-test, we also analyze a point optimal variant introduced by CY. For a known value of ρ_x, the (infeasible) test statistic takes the following form:

$$Q := \frac{\hat{\beta}_x - (s_{xy}/s_x^2)(\hat{\rho}_x - \rho_x)}{\sqrt{s_y^2\{1-(s_{xy}^2/s_y^2 s_x^2)\}/\sum_{t=1}^{T}(x_{t-1}-\bar{x}_{-1})^2}},$$

where $\hat{\beta}_x$ and s_y^2 are as defined above, $s_{xy} := (T-2)^{-1}\sum_{t=1}^{T}\hat{\epsilon}_{xt}\hat{\epsilon}_{yt}$ and $s_x^2 := (T-2)^{-1}\sum_{t=1}^{T}\hat{\epsilon}_{xt}^2$ with $\hat{\epsilon}_{xt}$ denoting the OLS residuals from regressing x_t on a constant and x_{t-1}, and where $\hat{\rho}_x := \sum_{t=1}^{T}(x_{t-1}-\bar{x}_{-1})x_t/\sum_{t=1}^{T}(x_{t-1}-\bar{x}_{-1})^2$. In the case where $s_{xy}=0$, Q and t_u coincide.

The limit distributions of t_u and Q under Assumption 1 are shown in the next theorem.

Theorem 1. For the DGP (1), (2), (3) and under Assumption 1, the weak limits of t_u and Q as $T \to \infty$ are of the form

$$\frac{\int_0^1 \bar{M}_{\eta c,x}(r)dN_{\eta y}(r)}{\sqrt{\int_0^1 \bar{M}_{\eta c,x}(r)^2}} + \frac{g_x\int_0^1 \bar{M}_{\eta c,x}(r)^2 + g_z\int_0^1 \bar{M}_{\eta c,x}(r)M_{\eta c,z}(r)}{\sqrt{n_y\int_0^1 \bar{M}_{\eta c,x}(r)^2}}, \quad (6)$$

where $\bar{M}_{\eta c,x}(r) := M_{\eta c,x}(r) - \int_0^1 M_{\eta c,x}(s)ds$, $r \in [0,1]$, and $N_{\eta y}$, n_y are statistic-specific. Thus, for the t_u statistic, $N_{\eta y} := \omega_{yy}^{-1/2}M_{\eta y}$ and $n_y := \omega_{yy}$, whereas for the Q statistic, $N_{\eta y} := \omega_{y|x}^{-1/2}\{M_{\eta y} - \omega_{xy}\omega_{xx}^{-1}M_{\eta x}\}$ and $n_y := \omega_{yy} - \omega_{xy}^2/\omega_{xx} =: \omega_{y|x}$.

Remark 5. Notice that the limit expressions for t_u and Q in (6) are identical when $h_{31}=0$ (i.e., $\omega_{xy}=0$). The limit expression in (6) shows the dependence of t_u and Q on g_z under H_z (where $g_x = 0$ but $g_z \neq 0$). Consequently, even for infeasible versions of these tests where all other nuisance parameters were known, the use of asymptotic critical values appropriate for these tests under H_u will not result in size-controlled procedures under H_z and raises the possibility that spurious rejections in favor of predictability of y_t by x_{t-1} will be encountered when y_t is actually predictable by z_{t-1} (see Ferson Sarkissian, and Simin 2003a, Ferson Sarkissian, and Simin 2003a,b, and Deng 2014, for related results under nonlocalized β_z). Under H_{xz}, where both $g_x \neq 0$ and $g_z \neq 0$, any rejection by t_u or Q could not uniquely be ascribed to the role of x_{t-1}, potentially suggesting the existence of a well-specified PR that is in fact under-specified due to the omission of z_{t-1}. The same issues also hold for the feasible versions of the t_u and Q tests developed in CES and in CY and Phillips (2014), respectively.

Remark 6. In the special case where $c_x = c_z$, the limit of t_u in (6) can be written as

$$\frac{\int_0^1 \bar{B}_{\eta c,x}(r)dM_{\eta y}(r)}{\sqrt{\omega_{yy}\int_0^1 \bar{B}_{\eta c,x}(r)^2}} + g_x^{\perp}\left(\frac{\omega_{xx}}{\omega_{yy}}\right)^{1/2}\sqrt{\int_0^1 \bar{B}_{\eta c,x}(r)^2}$$
$$+\ g_z\left(\frac{\omega_{z|x}}{\omega_{yy}}\right)^{1/2}\frac{\int_0^1 \bar{B}_{\eta c,x}(r)B_{\eta c,2}(r)}{\sqrt{\int_0^1 \bar{B}_{\eta c,x}(r)^2}} \quad (7)$$

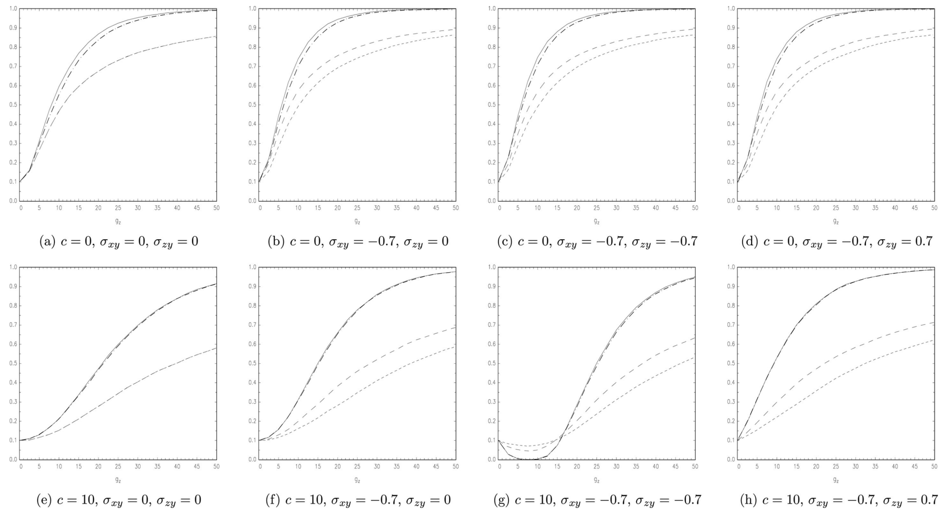
with $B_{\eta c,2}(r) := \int_0^r e^{(s-r)c_z}dB_{\eta 2}(s)$ for $r \in [0,1]$, $\omega_{z|x} := \omega_{zz} - \omega_{xz}^2/\omega_{xx}$, and $g_x^{\perp}T^{-1} := (g_x + \omega_{xz}\omega_{xx}^{-1}g_z)T^{-1}$ representing the coefficient of x_{t-1} in a redefinition of (1) where x_{t-1} is orthogonal to the unincluded persistent variable (see Remark 2 with $\lambda = h_{21}h_{11}^{-1}\beta_z = \omega_{xz}\omega_{xx}^{-1}g_zT^{-1}$). Not surprisingly, therefore, t_u can be anticipated to have relatively low power to reject H_u in favor of H_{xz} when the contribution of x_{t-1} to the variability of y_t (as measured by $|g_x^{\perp}|\omega_{xx}^{1/2}\omega_{yy}^{-1/2}$) is low, and also the contribution of z_{t-1} corrected for x_{t-1} (as measured by $|g_z|\omega_{z|x}^{1/2}\omega_{yy}^{-1/2}$) is low. Additionally, the correlation between $\bar{B}_{\eta c,x}$ and $M_{\eta y}$ (for $h_{31} \neq 0$) renders the leading term in (7) non-Gaussian, affecting both the size and the power of the test. These comments also apply to the limit of the Q statistic, except that the first term in (7) is then standard Gaussian.

We will now proceed to investigate the extent of the size distortions that occur in the t_u and Q tests when $g_z \neq 0$. Before doing so, it should be noted that other PR tests have been proposed in the literature, including the near-optimal tests of Elliott, Müller, and Watson (2015) and Jansson and Moreira (2006); see the useful recent summaries provided in BD and Cai, Wang, and Wang (2015). The issues we discuss in this article are pertinent irrespective of which particular PR test one uses, in cases where the putative and unincluded predictors are persistent. They are also relevant for the case where a putative PR contains multiple predictors.

3.1 Asymptotic Size of Predictive Regression Tests Under H_z

To obtain as transparent as possible a picture of the large-sample size properties of t_u and Q under H_z, we abstract from any role that nonstationary volatility plays by setting $d_i = 1$, $i = 1, 2, 3$. We then simulate the limit distributions using 10,000 Monte Carlo replications, approximating the Brownian motion processes in the limiting functionals for (6) using independent $N(0,1)$ random variates, with the integrals approximated by normalized sums of 2000 steps. Critical values are obtained by setting $g_x = g_z = 0$; for t_u these depend on c_x and also (it can be shown) $h_{31}^2/(h_{31}^2 + h_{32}^2 + h_{33}^2) = \sigma_{xy}^2/\sigma_{xx}\sigma_{yy}$, while for Q, these depend on c_x alone. These quantities are assumed known, so we are essentially analyzing the large-sample behavior of infeasible variants of t_u and Q. We graph nominal 0.10-level sizes of two-sided tests as functions of the parameter $g_z = \{0, 2.5, 5.0, \ldots, 50.0\}$ with $g_x = 0$. For $c_x = c_z = c = \{0, 10\}$, we set $\sigma_{xx} = \sigma_{zz} = \sigma_{yy} = 1$, and consider $\sigma_{xy} = \sigma_{zy} = 0$ plus $\sigma_{xy} = -0.70$ with $\sigma_{zy} = \{0, -0.70, 0.70\}$ where $\sigma_{xz} = 0$ throughout. Setting $c_x = c_z$ is not a requirement here, but simply facilitates keeping x_t and z_t balanced in terms of their persistence properties.

The results of this size simulation exercise are shown in Figure 1. For $c = 0$ we observe the sizes of t_u and Q growing monotonically from the baseline 0.10 level with increasing g_z, thereby giving rise to an ever-increasing likelihood of ascribing spurious predictive ability to x_{t-1}. Both tests' sizes are seen to exceed 0.85 for $g_z = 50$, while even a value of g_z as small as $g_z = 12.5$ produces sizes in excess of 0.50. The size patterns for t_u and Q are also quite similar, which is as we would expect given that g_z impacts upon their limit distributions in a very similar way. Of course, when $\sigma_{xy} = 0$, the tests have identical limits, while for $\sigma_{xy} = -0.7$, there is a general tendency for Q to show slightly more pronounced over-sizing than t_u (possibly reflecting the relatively higher power that this test can achieve under



(a) $c=0$, $\sigma_{xy}=0$, $\sigma_{zy}=0$ (b) $c=0$, $\sigma_{xy}=-0.7$, $\sigma_{zy}=0$ (c) $c=0$, $\sigma_{xy}=-0.7$, $\sigma_{zy}=-0.7$ (d) $c=0$, $\sigma_{xy}=-0.7$, $\sigma_{zy}=0.7$

(e) $c=10$, $\sigma_{xy}=0$, $\sigma_{zy}=0$ (f) $c=10$, $\sigma_{xy}=-0.7$, $\sigma_{zy}=0$ (g) $c=10$, $\sigma_{xy}=-0.7$, $\sigma_{zy}=-0.7$ (h) $c=10$, $\sigma_{xy}=-0.7$, $\sigma_{zy}=0.7$

Figure 1. Asymptotic rejection frequencies of S, S_B (power) and t_u, Q (size): $g_x = 0$, $c_x = c_z = c$; S: $-\cdot-$, S_B: —, t_u: $- - -$, Q: $--$.

H_x). Size distortions appear little influenced by the value taken by σ_{zy}. With $c = 10$ qualitatively, the same comments apply here as for the case $c = 0$. That said, we do observe that the over-sizing now manifests itself more slowly with increasing g_z. Indeed, when $\sigma_{zy} = -0.70$ some modest under-size is observed for small values of g_z. However, both sizes are still above 0.50 once $g_z = 50$ so spurious predictability does remain a serious issue. That the problem is less severe here simply reflects the fact that x_{t-1} and z_{t-1} are lower (but still high) persistence processes.

It would be difficult to argue that spurious predictive ability is not a potentially important consideration to take into account when employing either of the t_u and Q tests to infer predictability with high persistence processes. Although we have focused this analysis on OLS-based PR tests, similar qualitative results will pertain for other PR tests including the recently proposed IV-based tests of BD whenever a high persistence IV is used. A low persistence IV test should be less prone to over-size in the presence of a high persistence unincluded variable z_{t-1}, but the price paid for employing such an IV is that when a true predictor x_{t-1} is highly persistent, the IV test will have very poor power. Basically, whenever there is scope for high persistence properties of regressors to yield good power for PR tests, we should always remain alert to the possibility of spurious predictability.

4. A TEST FOR PREDICTIVE REGRESSION INVALIDITY

Given the potential for standard PR tests to spuriously signal predictability of y_t by x_{t-1} (alone) when $\beta_z \neq 0$, we now consider a test devised to distinguish between $\beta_z = 0$ and $\beta_z \neq 0$. Nonrejection by such a test would indicate that z_{t-1} plays no role in predicting y_t, and hence that standard PR tests based on x_{t-1} are valid. Rejection, however, would indicate the presence of an unincluded variable z_{t-1} in the DGP for y_t, signaling the invalidity of PR tests based on x_{t-1}. Formally, then, we wish to test the null hypothesis that $\beta_z = 0$, that is, H_u, H_x, against the alternative that $\beta_z \neq 0$, that is, H_z, H_{xz}, in (1).

4.1 The Test Statistic and Conventional Asymptotics

The test we develop is based on testing a null hypothesis of stationarity; specifically, we adapt the co-integration tests of Shin (1994) and Leybourne and McCabe (1994), which are themselves variants of the KPSS test. We employ the statistic

$$S := s^{-2}T^{-2}\sum_{t=1}^{T}\left(\sum_{i=1}^{t}\hat{e}_i\right)^2, \tag{8}$$

where $s^2 := (T-3)^{-1}\sum_{t=1}^{T}\hat{e}_t^2$ and $\hat{e}_t$ are the OLS residuals from the fitted regression

$$y_t = \hat{\alpha}_y + \hat{\beta}_x x_{t-1} + \hat{\beta}_{\Delta x}\Delta x_t + \hat{e}_t, \qquad t = 1, \ldots, T, \tag{9}$$

where, as in Shin (1994), the regressor Δx_t is included in (9) to account for the possibility of correlation between ϵ_{xt} and ϵ_{yt} ($h_{31} \neq 0$). Abstracting from the role of the regressor Δx_t, when $\beta_z \neq 0$, the residuals $\hat{e}_t$ incorporate a contribution of the unincluded z_{t-1} term in (1), hence the persistence in z_{t-1} is passed to $\hat{e}_t$, and the statistic S is a test of $\beta_z = 0$ against $\beta_z \neq 0$, rejecting for large values of S. Specifically, assuming $c_z = 0$, we can rewrite (1) as

$$y_t = \alpha_y + \beta_x x_{t-1} + r_{t-1} + \epsilon_{yt}, \tag{10}$$

where $r_t = r_{t-1} + u_t$, initialized at $r_0 = \beta_z\alpha_z$ (on setting $s_{z,0} = 0$ with no loss of generality) with innovations $u_t = \beta_z\epsilon_{zt}$. Testing the null of $\beta_z = 0$ against $\beta_z = g_z T^{-1}$ in (1) is then seen to be precisely the same problem as testing the null of $V(u_t) =: \sigma_{uu} = 0$ against $\sigma_{uu} = g_z^2 T^{-2}\sigma_{zz}$ in the context of (10), with $g_z = 0$ under both nulls. If we temporarily assume that x_t is strictly exogenous and ϵ_{yt} and ϵ_{zt} are independent IID normal random variates, then S is the locally best invariant (to α_y, α_x, α_z, β_x, and σ_{yy}) test of the null $\sigma_{uu} = 0$ against the local alternative $\sigma_{uu} = g_z^2 T^{-2}\sigma_{zz}$ in (10). As such, the statistic S is relevant for our testing problem where we seek to distinguish between $\beta_z = 0$ and $\beta_z \neq 0$. In our model we do not impose $c_z = 0$ (nor the other temporary assumptions above), so in these more general circumstances we consider S to deliver a near locally best invariant test.

Notwithstanding the foregoing motivation, it is important to stress that a test based on S should properly be viewed as a misspecification test for the linear regression in (9). As such, a rejection by this test indicates that the fitted regression in (9) is not a valid PR. As with the failure of any misspecification test, this does not tell us why the regression has failed. We do know that S delivers a test which is (approximately) locally optimal in the direction of z_{t-1} being an unincluded variable (be it manifest or latent), but a rejection does not mean that x_{t-1} is not a valid predictor for y_t. Therefore, our proposed test is one for the invalidity of the putative PR, not of the putative predictor, x_{t-1}; see again the discussion on this point in Section 2.

In Theorem 2 we now detail the limiting distribution of S under Assumption 1.

Theorem 2. For the DGP (1), (2), (3) and under Assumption 1,

$$S \xrightarrow{w} \int_0^1 \{F(r, c_x) + g_z G(r, c_x, c_z)\}^2 dr, \tag{11}$$

where

$$F(r, c_x) := \mathbb{B}_{\eta,y|x}(r) - \int_0^1 \bar{B}_{\eta c,x}(s) dB_{\eta,y|x}(s) \times \left\{\int_0^1 \bar{B}_{\eta c,x}(s)^2\right\}^{-1} \int_0^r \bar{B}_{\eta c,x}(s),$$

$$G(r, c_x, c_z) := \left(\frac{\omega_{zz}}{\omega_{y|x}}\right)^{1/2} \left\{\int_0^r \bar{B}_{\eta c,z}(s) - \frac{\int_0^1 \bar{B}_{\eta c,x}(s) B_{\eta c,z}(s)}{\int_0^1 \bar{B}^2_{\eta c,x}(s)} \int_0^r \bar{B}_{\eta c,x}(s)\right\}$$

with $\omega_{y|x} := \omega_{yy} - \omega_{xy}^2/\omega_{xx}$, $\mathbb{B}_{\eta,y|x}(r) := B_{\eta,y|x}(r) - rB_{\eta,y|x}(1)$, $r \in [0, 1]$, and $B_{\eta,y|x} := \omega_{y|x}^{-1/2}\{M_{\eta y} - \omega_{xy}\omega_{xx}^{-1} M_{\eta x}\}$ a standardized heteroscedastic Brownian motion independent of B_1.

Remark 7. Notice that the limit in (11) does not depend on h_{31} owing to the invariance of the residuals $\hat{e}_t$ to this parameter arising from the presence of the regressor Δx_t in (9). In the special case $c_x = c_z$, the limit is also invariant to h_{21} (see Remark 2). In fact, as $M_{\eta z} = \omega_{xz}\omega_{xx}^{-1} M_{\eta x} + \omega_{z|x}^{1/2} B_{\eta 2}$ for $\omega_{z|x} := \omega_{zz} - \omega_{xz}^2/\omega_{xx}$, in this case the equality of the decay rate in the Ornstein–Uhlenbeck processes $M_{\eta c,x}$ and $M_{\eta c,z}$ ensures that $B_{\eta c,z|x} := \omega_{z|x}^{-1/2}\{M_{\eta c,z} - \omega_{xz}\omega_{xx}^{-1} M_{\eta c,x}\}$ equals the Ornstein–Uhlenbeck process $B_{\eta c,2}$ so $G(r, c_x, c_z)$ reduces to

$$G(r, c_x, c_x) = \left(\frac{\omega_{z|x}}{\omega_{y|x}}\right)^{1/2} \left\{\int_0^r \bar{B}_{\eta c,2}(s) - \frac{\int_0^1 \bar{B}_{\eta c,x}(s) B_{\eta c,2}(s)}{\int_0^1 \bar{B}^2_{\eta c,x}(s)} \int_0^r \bar{B}_{\eta c,x}(s)\right\}.$$

The term $g_z G(r, c_x, c_z)$ in (11) is key in enabling the test S to potentially distinguish between H_u, H_x and H_z, H_{xz}. Clearly if $\omega_{z|x}/\omega_{y|x} \simeq 0$, then such a test has low power. This occurs when ϵ_{xt} and ϵ_{zt} are highly correlated (so $\omega_{z|x} \simeq 0$, corresponding to the part of z_{t-1} that is not shared and therefore not removed by the regressor x_{t-1}, on average over t), or more generally, when ϵ_{zt} corrected for ϵ_{xt} varies little relatively to ϵ_{yt} corrected for ϵ_{xt}. For $c_x \neq c_z$ the limit of S depends on h_{21} as $G(r, c_x, c_x) - G(r, c_x, c_z)$ is proportional to $h_{21}h_{11}^{-1}$.

Remark 8. Under H_u, H_x, where $g_z = 0$, the limit distribution of S in (11) simplifies to $\int_0^1 F(r, c_x)^2$ and depends only on c_x and any unconditional heteroscedasticity present in ϵ_t.

Remark 9. We have assumed thus far that the ϵ_{xt} are serially uncorrelated, with e_t being an m.d.s. More generally we may consider a linear process assumption for ϵ_{xt} of the form $\epsilon_{xt} = \sum_{i=0}^{\infty} \theta_i \upsilon_{x,t-i}$ where $\upsilon_{x,t}$ is the first element of $HD_t e_t$ with the standard summability and invertibility conditions $\sum_{i=0}^{\infty} i|\theta_i| < \infty$ and $\sum_{i=0}^{\infty} \theta_i z^i \neq 0$ for all $|z| \leq 1$, respectively, satisfied. Under homoscedasticity, this would include all stationary and invertible ARMA processes. Notice that ϵ_{yt} remains uncorrelated with the increments of x_t at all lags (i.e., x_t is weakly exogenous with respect to ϵ_{yt}) under this structure. Here, it may be shown that the limiting results given in Theorem 2 and in Theorems 3–5 continue to hold provided we replace (9) in the calculation of S with the augmented variant

$$y_t = \hat{\alpha}_y + \hat{\beta}_x x_{t-1} + \hat{\beta}_{\Delta x}\Delta x_t + \sum_{i=1}^{p} \hat{\delta}_i \Delta x_{t-i} + \hat{e}_t, \quad t = p+1, \ldots, T, \tag{12}$$

where p satisfies the standard rate condition that $1/p + p^3/T \to 0$, as $T \to \infty$, and it is assumed that $T^{1/2}\sum_{i=p+1}^{\infty} |\delta_i| \to 0$, where $\{\delta_i\}_{i=1}^{\infty}$ are the coefficients of the $AR(\infty)$ process obtained by inverting the $MA(\infty)$ for ϵ_{xt}. Similarly to BD, we would also need to restrict the amount of serial dependence allowed in the conditional variances via the assumption that $\sup_{i,j\geq 1} \|\tau_{ij}\| < \infty$, where $\tau_{ij} := E(e_t e_t' \otimes e_{t-i} e_{t-j}')$, with $\otimes$ denoting the Kronecker product. Serial correlation of a similar form in ϵ_{zt} will have no impact on our large-sample results under the null hypothesis, H_u, H_x, although an effect does arise under H_z, H_{xz}. As is standard in the PR literature, we maintain the assumption that ϵ_{yt} is serially uncorrelated.

Remark 10. Extensions to the case where the putative PR contains multiple regressors and/or more general deterministic components can easily be handled in the context of our proposed PR invalidity test. Specifically, denoting the deterministic component as $\tau'\mathbf{f}_t$, where $\mathbf{f}_t$ is as defined in Section 3.2 of BD, an obvious example being the linear trend case where $\mathbf{f}_t := (1, t)'$, and the vector of putative regressors as $\mathbf{x}_{t-1}$, then we would need to correspondingly construct S using the residuals from the regression of y_t on $\mathbf{f}_t$, $\mathbf{x}_{t-1}$, and $\Delta\mathbf{x}_{t-1}$. Doing so would alter the form of the limit distributions given in Theorem 2 and in the sequel, but would not alter the primary conclusion given in Corollary 1, that the fixed regressor wild bootstrap implementation of this test is asymptotically valid.

A consequence of the result in Theorem 2 is therefore that if we wish to base a test for PR invalidity on S, then we need to address the fact that under the null H_u, H_x the limit distribution of S is not pivotal. To account for the dependence of inference on any unconditional heteroscedasticity present, we employ a wild bootstrap procedure based on the residuals $\hat{e}_t$. However, we also need to account for the dependence of the limit distribution of S on c_x, and this we carry out by using the observed outcome on

$x := [x_0, \ldots, x_T]'$ as a fixed regressor in the bootstrap procedure which we detail next.

4.2 A Fixed Regressor Wild Bootstrap Stationarity Test

A standard approach to obtaining bootstrap critical values for S would involve repeated generation of bootstrap samples for the original y_t, such that they mimic (in a statistical sense) the behavior of y_t with the null H_u, H_x imposed, together with repeated generation of bootstrap samples for the original x_t, to mimic the behavior of x_t. For each bootstrap sample, these would then be used to calculate a bootstrap analog of S, which should reflect the behavior of S under the null. Generation of bootstrap samples of y_t with suitable properties is quite straightforward, at least in large samples, using a standard wild bootstrap resampling scheme from the residuals $\hat{e}_t$ from (9). However, finding bootstrap samples of x_t presents a significant problem since $x_t = (1 - c_x T^{-1})x_{t-1} + \epsilon_{xt}$ (assuming $\alpha_x = 0$ for simplicity) and so any corresponding recursion used to construct bootstrap samples for x_t from bootstrap samples of e_{xt} requires, for a size-controlled test, that c_x should be known or consistently estimated. Unfortunately, it is well-known that consistent estimation of c_x is not feasible. To avoid this problem, we circumvent estimation of c_x altogether and instead follow the approach taken in Hansen (2000), considering a bootstrap procedure which uses x as a fixed regressor, that is, the bootstrap statistic S^* is calculated from the *same* observed x_t as was used in the construction of S itself.

We now outline the steps involved in our proposed fixed regressor wild bootstrap.

Algorithm 1 (Fixed Regressor Wild Bootstrap):

(i) Construct the wild bootstrap innovations $y_t^* := \hat{e}_t w_t$, where $w_t, t = 1, \ldots, T$, is an IID $N(0, 1)$ sequence independent of the data and $\hat{e}_t$ are the residuals from either (9) or (12).

(ii) Calculate the fixed regressor wild bootstrap analog of S,

$$S^* := (s_y^*)^{-2} T^{-2} \sum_{t=1}^{T} \left(\sum_{i=1}^{t} \hat{\epsilon}_{yi}^* \right)^2,$$

where $(s_y^*)^2 := (T-2)^{-1} \sum_{t=1}^{T} (\hat{\epsilon}_{yt}^*)^2$ and $\hat{\epsilon}_{yt}^*$ are OLS residuals from the fitted regression

$$y_t^* = \hat{\alpha}_y^* + \hat{\beta}_x^* x_{t-1} + \hat{\epsilon}_{yt}^*, \qquad t = 1, \ldots, T. \tag{13}$$

(iii) Define the corresponding p-value as $P_T^* := 1 - G_T^*(S)$ with G_T^* denoting the conditional (on the original data) cumulative distribution function (cdf) of S^*. In practice, G_T^* is unknown, but can be approximated in the usual way by numerical simulation.

(iv) The wild bootstrap test of H_u, H_x at level ξ rejects in favor of H_z, H_{xz} if $P_T^* \leq \xi$.

Remark 11. The wild bootstrap scheme used to generate y_t^* is constructed so as to replicate the pattern of heteroscedasticity present in the original innovations; this follows because, conditionally on $\hat{e}_t$, y_t^* is independent over time with zero mean and variance $\hat{e}_t^2$.

Remark 12. By definition, the residuals $\hat{e}_t$ from (9) are invariant to the value of β_x in (1), and so we can assume that $\beta_x = 0$ with no loss of generality when generating the bootstrap y_t^* data. We also do not include Δx_t as an additional regressor (or lags thereof in the case considered in Remark 9) in (13) because the $\hat{e}_t$ are asymptotically free of any effects arising from correlation between ϵ_{xt} and ϵ_{yt}, or from any weak dependence in ϵ_{xt}.

Remark 13. Although $\hat{e}_t$ depends on g_z under H_z, H_{xz}, we show in the next subsection that this does not translate into large-sample dependence of S^* on g_z.

4.3 Conditional Asymptotics and Bootstrap Validity

We show that the use of x_{t-1} as a fixed regressor in the construction of the bootstrap statistic S^* prevents S^* from converging weakly in probability to any nonrandom distribution, in contradistinction to most standard bootstrap applications we are aware of. Rather, under Assumption 1 and any of the hypotheses H_u, H_x, H_z, and H_{xz}, the distribution of S^*, given the data, converges weakly to the random distribution which obtains by conditioning the limit in (11) corresponding to $g_z = 0$, on the weak limit B_1 of the process $T^{-1/2} \sum_{t=1}^{\lfloor Tr \rfloor} e_{1t}$, $r \in [0, 1]$. This fact (along with some regularity conditions) makes it possible to conclude that the bootstrap p-value P_T^* is asymptotically uniform $U[0, 1]$-distributed under H_u, H_x, by using a general result on bootstrap validity from Cavaliere and Georgiev (2017, Theorem 2). From a pragmatic perspective, such a conclusion ensures that the bootstrap test is asymptotically size controlled under the conditions of Assumption 1 alone.

However, under Assumption 1 alone, the shortcoming remains that the meaning of the large-sample inference performed by our bootstrap test is unclear. Certainly, asymptotic bootstrap inference is not unconditional because S^* given the data does not converge to the unconditional limit distribution of S. On the other hand, bootstrap inference need not be asymptotically equivalent to conditional inference on x either. Indeed, it is well known that Theorem 2, where the limit distribution of S is established, cannot be taken to imply that S conditional on x converges weakly to the limit in (11) conditioned on B_1 (the implication is falsified by, for example, Example 1 of LePage, Podgórski, and Ryznar 1997). Nevertheless, it is not unreasonable to expect that this result holds true under certain additional requirements, and we prove that this is in fact the case. We strengthen Assumption 1, so that under H_u, H_x the distribution of the statistic S conditional on x converges weakly to the same random distribution as S^* given the data, which allows us to establish that our bootstrap test in large samples has the meaning of a test conditional on x.

The results we present differ from those given by Hansen (2000) who considers a joint structural stability test on the constant and slope parameters in a general regression setting; our test of $\beta_z = 0$ for the PR in (5) can be seen as the corresponding individual test for stability of just the intercept. Hansen argues that, under his Assumption 2, the fixed regressor (wild) bootstrap asymptotically implements unconditional inference (see Theorems 5 and 6, Hansen 2000) and that the convergence $P_T^* \stackrel{w}{\to} U[0, 1]$ of bootstrap p-values under the null hypothesis follows from the equivalence of the unconditional limiting null distribution of the original statistic and the limiting distribution of the bootstrap statistic given the data (see Corollaries 1 and 2, Hansen 2000). The results given in this section show that any such claim about unconditional inference is not correct, at least for the nonempty class of models satisfying both Hansen's and our assumptions. Nonetheless the stated convergence of bootstrap p-values is correct, albeit for a different reason. A fuller treatment of this specific issue is given by Georgiev, Harvey, Leybourne, and Taylor (2018).

Theorem 2 is based on the invariance principle given in (4). Conditional and bootstrap analogs of that theorem can be based on a *conditional* joint invariance principle for the original and the bootstrap data. To obtain this result, we will strengthen Assumption 1 as follows:

Assumption 2. Let Assumption 1 hold, together with the following conditions:

(a) e_t is drawn from a doubly infinite strictly stationary and ergodic sequence $\{e_t\}_{t=-\infty}^{\infty}$, which is a martingale difference w.r.t. its own past.
(b) $\{[e_{2t}, e_{3t}]\}_{t=-\infty}^{\infty}$ is an m.d.s. also w.r.t. $\mathcal{X} \vee \mathcal{F}_t$, where $\mathcal{X}$ and $\mathcal{F}_t$ are the σ-algebras generated by $\{e_{1t}\}_{t=-\infty}^{\infty}$ and $\{[e_{2s}, e_{3s}]\}_{s=-\infty}^{t}$, respectively, and $\mathcal{X} \vee \mathcal{F}_t$ denotes the smallest σ-algebra containing both $\mathcal{X}$ and $\mathcal{F}_t$.
(c) The initial values $s_{x,0}$ and $s_{z,0}$ are measurable w.r.t. $\mathcal{X}$ (in particular, they could be fixed constants).

Remark 14. Arguably, the most restrictive condition in Assumption 2 is given in part (b). A first leading example where it is satisfied is that of a symmetric multivariate GARCH process with neither leverage nor asymmetric clustering. Specifically, let $e_t = \Omega_t^{1/2}\varepsilon_t$, where Ω_t is measurable with respect to the past $[\varepsilon_{1s}^2, \varepsilon_{2s}^2, \varepsilon_{3s}^2]'$, $s \le t-1$, and $\{\varepsilon_t\}_{t=-\infty}^{\infty}$ is an iid sequence such that $E(\varepsilon_{it}|\varepsilon_{1t}, \varepsilon_{2t}^2, \varepsilon_{3t}^2) = 0, i = 2, 3$. If $E\|e_t\| < \infty$, then it could be seen that $E(e_{it}|\mathcal{X} \vee \mathcal{F}_{t-1}) = 0$, $i = 2, 3$. Another example is that of a multivariate stochastic volatility process $e_t = H_t^{1/2}\varepsilon_t$ with $\{H_t\}_{t=-\infty}^{\infty}$ independent of $\{\varepsilon_t\}_{t=-\infty}^{\infty}$ and where $\{\varepsilon_t\}_{t=-\infty}^{\infty}$ is an iid sequence with $E(\varepsilon_{it}|\varepsilon_{1t}) = 0$, $i = 2, 3$ (which is certainly true if ε_t is multivariate standard Gaussian, as is usually assumed in the stochastic volatility framework). If $E\|e_t\| < \infty$, then again $E(e_{it}|\mathcal{X} \vee \mathcal{F}_{t-1}) = 0$, $i = 2, 3$. These two examples are also the leading examples given in the univariate context by Deo (2000), and in sec. 3 of Gonçalves and Kilian (2004). It would be interesting, although beyond the scope of our article, to investigate how Assumption 2(b) could be weakened to the case where $\{e_t\}$ could be well approximated by a sequence satisfying Assumption 2(b). For instance, following Rubshtein (1996), the conclusions of Theorem 5 in the supplementary appendix would remain valid if Assumption 2(b) was replaced by the condition that $\sup_{t\ge 1} E\{E(\sum_{s=1}^{t} e_{is}|\mathcal{X})\}^2 < \infty$, $i = 2, 3$.

In Theorem 3, we now establish three things: first, a conditional invariance principle that can be assembled from results and ideas disseminated throughout the probabilistic literature (see, in particular, Awad 1981; Rubshtein 1996), second, a bootstrap extension of that result, and third, associated convergence results for stochastic integrals. For simplicity, a one-dimensional bootstrap partial-sum process is considered; it is constructed from quantities $\tilde{e}_{Tt}$ that we shall subsequently specify to be the residuals $\hat{e}_t$ from the regression in (9). Analogously to the definition of x, let $y := [y_1, \dots, y_T]'$ and $z := [z_0, \dots, z_T]'$.

Theorem 3. Let $\tilde{e}_{Tt}$ $(t = 1, \dots, T)$ be scalar measurable functions of x, y, z and such that $\sum_{t=1}^{\lfloor Tr \rfloor} \tilde{e}_{Tt}^2 \xrightarrow{p} \int_0^r m^2(s)ds$ for $r \in [0, 1]$, where m is a square-integrable real function on $[0, 1]$. Introduce $\tilde{\epsilon}_{tb} := w_t\tilde{e}_{Tt}$ $(t = 1, \dots, T)$, and $\tilde{B}_\eta(r) := \int_0^r m(s)d\tilde{B}_1(s)$, $r \in [0, 1]$, where $\tilde{B}_1$ is a standard Brownian motion independent of B. Under Assumption 2, the following converge jointly as $T \to \infty$:

$$\left(T^{-1/2}\sum_{t=1}^{\lfloor Tr \rfloor}\epsilon_t, T^{-1}\sum_{t=1}^{T}\sum_{s=1}^{t-1}\epsilon_{xs}[\epsilon_{yt}, \epsilon_{zt}]\right)\Bigg| x$$

$$\xrightarrow{w} \left(M_\eta(r), \int_0^1 M_{\eta x}(s)d[M_{\eta y}(s), M_{\eta z}(s)]\right)\Bigg| B_1,$$

$r \in [0, 1]$, in the sense of weak convergence of random measures on $\mathcal{D}^3 \times \mathbb{R}^2$, and

$$\left(T^{-1/2}\sum_{t=1}^{\lfloor Tr \rfloor}[e_{1t}, \tilde{\epsilon}_{tb}], T^{-1}\sum_{t=1}^{T}\sum_{s=1}^{t-1}\epsilon_{xs}\tilde{\epsilon}_{tb}\right)\Bigg| x, y, z$$

$$\xrightarrow{w} \left(B_1(r), \tilde{B}_\eta(r), \int_0^1 M_{\eta x}(s)d\tilde{B}_\eta(s)\right)\Bigg| B_1,$$

$r \in [0, 1]$, in the sense of weak convergence of random measures on $\mathcal{D}^2 \times \mathbb{R}$.

Remark 15. Let $E_x(\cdot) := E(\cdot|x)$ and $E^*(\cdot) := E(\cdot|x, y, z)$. The convergence concept used in Theorem 3 is defined as follows. Let ζ, ζ_T and ξ, ξ_T $(T \in \mathbb{N})$ be random elements of the metric spaces $\mathcal{S}$ and $\mathcal{T}$, respectively, such that ζ, ξ and B_1 are defined on the same probability space, and similarly for ζ_T, ξ_T and x, y, z. We say that $\zeta_T|x \xrightarrow{w} \zeta|B_1$ and $\xi_T|x, y, z \xrightarrow{w} \xi|B_1$ jointly in the sense of weak convergence of random measures on $\mathcal{S}$ and $\mathcal{T}$ if for all bounded continuous functions $f : \mathcal{S} \to \mathbb{R}$ and $g : \mathcal{T} \to \mathbb{R}$ it holds that

$$\left[E_x(f(\zeta_T)), E^*(g(\xi_T))\right]' \xrightarrow{w} \left[E\left(f\left(\zeta\right)|B_1\right), E(g(\xi)|B_1)\right]'$$

as $T \to \infty$, in the sense of standard weak convergence of random vectors in $\mathbb{R}^2$.

We are already in a position to establish in Theorem 4 the large-sample behavior of S conditional on x, and of S^*, its bootstrap analog from Algorithm 1, conditional on the data. These two limiting distributions will be seen to coincide under the null hypothesis.

Theorem 4. Under DGP (1)–(3) and Assumption 2, the following converge jointly as $T \to \infty$, in the sense of weak convergence of random measures on $\mathbb{R}$:

$$S|x \xrightarrow{w} \int_0^1 \{F(r, c_x) + g_z G(r, c_x, c_z)\}^2 dr\Bigg| B_1 \qquad (14)$$

$$S^*|x, y, z \xrightarrow{w} \int_0^1 F(r, c_x)^2 dr\Bigg| B_1, \qquad (15)$$

where the processes F and G are as defined in Theorem 2.

Remark 16. A comparison of (14) and (15) shows that the bootstrap statistic S^*, conditional on the data, and the original statistic S, conditional on x, converge jointly to the same random distribution when $g_z = 0$, that is, under the null hypothesis, H_u, H_x. An implication of this is that the bootstrap approximation is consistent in the sense that

$$\sup_{u\in\mathbb{R}} \left|P_x(S \le u) - P^*(S^* \le u)\right| \xrightarrow{p} 0, \qquad (16)$$

given that the random cdf of $\int_0^1 F(r, c_x)^2 dr|B_1$ is sample-path continuous. Here P_x and P^* denote probability conditional on x and on all the data, respectively. Thus, the distribution of the "fixed-regressor bootstrap" statistic S^* conditional on the data consistently estimates the large-sample distribution of the original statistic S conditional on the "fixed regressor" x. This result differs from the usual formulation of bootstrap validity, where two cdfs with a common nonrandom limit are compared; here, in contrast, $P_x(S \le u) \xrightarrow{w} P(\int_0^1 F(r, c_x)^2 dr \le u|B_1)$, $u \in \mathbb{R}$, with a nondegenerate random limit.

In Corollary 1, we formulate the conclusion of asymptotic validity of the bootstrap test based on S and S^* in terms of the bootstrap p-values.

Corollary 1. Let $P_T^* := P^*(S^* > S)$. Under H_u, H_x and Assumption 2, $P_T^*|x \xrightarrow{w}_p U[0, 1]$ and $P_T^* \xrightarrow{w} U[0, 1]$.

An implication of Corollary 1 is that comparison of the statistic S with a ξ level bootstrap critical value (approximated by the upper tail ξ percentile from the order statistic formed from B independent simulated bootstrap S^* statistics, which we will denote by $\text{cv}_{\xi,B}$) results in a bootstrap test with correct asymptotic size (ξ) under H_u, H_x, conditionally on x and unconditionally. In what follows we denote by S_B the fixed regressor wild bootstrap procedure outlined in Algorithm 1, whereby S is compared to the critical value $\text{cv}_{\xi,B}$. The asymptotic local power of S_B under H_z, H_{xz} depends on the parameter g_z.

Remark 17. For the bootstrap statistic, S^*, the same limiting distribution is obtained in (15) under the alternative hypothesis, H_z, H_{xz}, as under the null hypothesis. In contrast, in the case of S, a stochastic offset, arising from the term $g_z G(r, c_x, c_z)$, is seen in the limiting distributions (in (14) conditionally on x, and in (11) unconditionally). Although, for a given alternative, the asymptotic local power is different for the bootstrap test based on S^* and an (infeasible) test based on the unconditional limit of S and knowledge of the parameter c_x (the former power is a random variable depending on B_1 and the latter power is a number), we comment in Remark 18 on some qualitative similarities.

Remark 18. The limiting functional for S in (11) and (14) is dominated in probability (both unconditionally and conditionally on B_1) by $g_z^2 \int_0^1 G(r, c_x, c_z)^2 dr$ for large g_z and, as a result, asymptotic local power approaches 1 as g_z diverges. Nonetheless, asymptotic local power is not monotone in $|g_z|$. For example, in the case $c_x = c_z$, the null component $F(r, c_x)$ in (11) and (14) involves a term in $h_{32}B_{\eta 2}(r)$, while the alternative component $g_z G(r, c_x, c_z)$ involves a term in $g_z \int_0^r \bar{B}_{\eta c,2}$ (see Remark 7). Because $B_{\eta 2}(r)$ and $\int_0^r \bar{B}_{\eta c,2}$ are positively correlated, it can be shown that $E\{\int_0^1 F(r, c_x)G(r, c_x, c_z)dr\} \neq 0$ for $h_{32} \neq 0$, and similarly for the conditional expectation given B_1, a.s. As a result, when $h_{32} \neq 0$, there exist values of g_z (dependent on B_1 in the conditional case) which render the expectations of the limits in (11) and (14) (respectively, unconditional and conditional on B_1), smaller than their expectations under the null hypothesis. For such g_z the limit distribution under the alternative does not first-order stochastically dominate the limit distribution under the null, translating into power being less than size for some size levels.

4.4 Asymptotic Local Power of Stationarity Tests Under H_z

We now consider the asymptotic local power of S and S_B, the latter on average over B_1. We use the same set of homoscedastic simulation models as for the size of t_u and Q in Figure 1, so we overlay this information on them. For the asymptotic power of S under H_z, we use the limit expression (11), having first obtained 0.10-level critical values from simulating (11) under $g_z = 0$. Since these critical values depend on knowledge of c_x, S here is an infeasible test against which to benchmark the power of S_B. The asymptotic power of S_B is also based on the limit distribution of S under H_z but compared against a simulated limit bootstrap critical value $\text{cv}_{\xi,B}$ with $\xi = 0.10$. For each replication, this critical value is obtained by simulating the limit (15) using $B = 2000$ replications, conditioning on the simulated B_1 for that Monte Carlo replication.

When $c = 0$, we see the power of S rising rapidly with departures from $g_z = 0$. For $g_z = 50$, its power is very close to 1. Turning attention to S_B, it has a very similar power profile to that of S; indeed, its power marginally exceeds that of S. It is of course anticipated from Remark 17 that S_B does not have the same asymptotic local power function as S, but the fact that its power exceeds that of S is a welcome finding as S_B, unlike S, is a feasible procedure. When $c = 10$ the powers of S and S_B are near identical, but at a lower level than when $c = 0$. There is also a nonmonotonicity in the power profiles of S and S_B, anticipated from Remark 18, for $\sigma_{zy} = -0.70$ when g_z is small, with power dipping below size. However, for large enough g_z, this anomaly disappears. (We note that S is not LBI when we allow correlation between ϵ_{yt} and ϵ_{zt} so this anomalous behavior is perhaps not entirely surprising.)

The important comparison here is between the power of S_B (restricting attention to the feasible procedure) and the size of t_u and Q (as their size profiles are similar we only refer to t_u). When $c = 0$, the power of S_B exceeds the size of t_u, hence the invalidity of the PR is detected with greater frequency than t_u spuriously rejects in favor of predictability of y_t by x_{t-1}. This demonstrates the capability of S_B to detect PR invalidity in cases where the important size problems associated with t_u exist. That the power of S_B exceeds the size of t_u under H_z is possibly to be expected, because S is designed to detect departures from the null of $g_z = 0$ whereas such departures simply represent model misspecification in the context of the PR test t_u. With $c = 10$, we again see that the power of S_B generally out-strips the sizes of t_u, with the size/power differences appearing even more marked than for $c = 0$. Again, the only exception to this is for $\sigma_{zy} = -0.7$ when g_z is small.

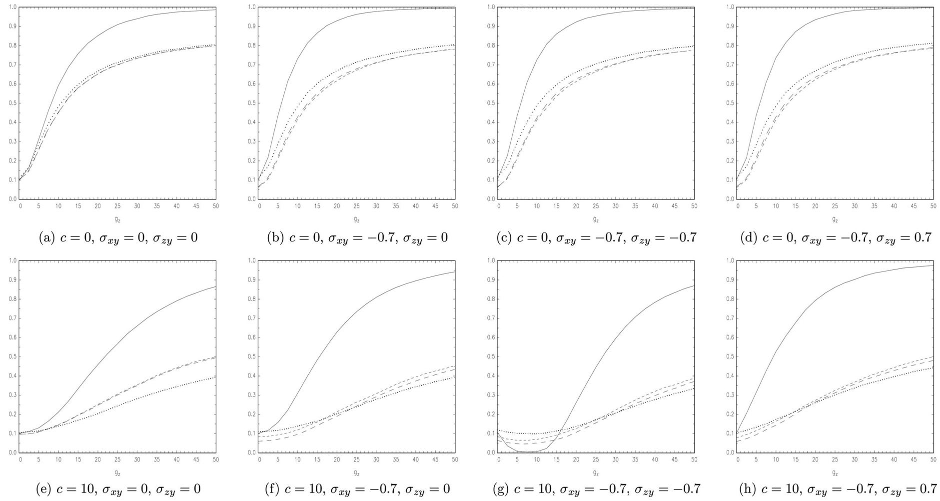


Figure 2. Finite sample rejection frequencies of S_B (power) and t_u, Q, IV_{comb} (size): $T = 200$, $g_x = 0$, $c_x = c_z = c$; S_B: — , t_u: - - - , Q: – – , IV_{comb}: · · ·

The supplementary appendix to this article contains asymptotic power simulation results for some additional parameter configurations (for which many possibilities exist). We consider the current setup with $c = 5$ and $c = 20$ and we find that the power of S_B with $c = 20$ is lower than for $c = 10$ due to a less persistent z_{t-1} lessening the impact of model misspecification. Other simulations where we allow c_z to be different to c_x confirm that the main driver of power for S_B is c_z and not c_x, as would be expected. We also consider $\sigma_{xz} \neq 0$ (with c_z and c_x equal or different; note that we reduce the magnitudes of σ_{xy} and σ_{zy} in some cases to ensure Ω remains positive definite). Here the interplay between S_B and t_u (Q) becomes rather more complex. For example, with $c_z = c_x$, setting $\sigma_{xz} = \pm 0.5$ causes the power of S_B to suffer while the frequency with which t_u rejects increases, while for $c_z \neq c_x$, only small changes are observed for $\sigma_{xz} \neq 0$ compared to $\sigma_{xz} = 0$.

5. FINITE SAMPLE SIZE AND POWER UNDER H_z

We now evaluate the finite sample size properties of the PR tests and the size and power of S_B. For the PR tests, we consider the feasible versions of t_u and Q, proposed by CES and CY, respectively, both of which rely on Bonferroni bounds to control size. (We are grateful to Campbell and Yogo for making their Gauss code available for these two procedures.) We also consider the IV-based test of BD that combines fractional and sine function instruments, denoted IV_{comb}, comparing this with its asymptotic $\chi^2(1)$ critical value. For S_B we use $B = 499$ replications.

To begin, we continue to abstract from heteroscedasticity and consider finite sample DGPs for the same settings as used in the main asymptotic simulations. Specifically, we simulate the DGP (1)–(3) for $T = 200$ with $\alpha_y = \alpha_x = \alpha_z = 0$, $g_x = 0$, $s_{x,0} = s_{z,0} = 0$, $d_{it} = 1$ $(i = 1, 2, 3)$, and $e_t \sim \text{IID}\, N(0, I_3)$. Figure 2 reports the finite sample analogs of Figure 1, that is, rejection frequencies of nominal 0.10-level (two-sided for t_u, Q, and IV_{comb}) tests under H_z. Simulations are again conducted using 10,000 Monte Carlo replications. On comparing Figure 2 with its large-sample counterpart Figures 1, it is clear that our asymptotic simulations provide a close approximation to the finite sample rejection frequencies of t_u, Q, and S_B, particularly in terms of the relative behavior of the tests, albeit in absolute terms the finite sample rejection frequencies tend to be slightly lower than their asymptotic counterparts. For t_u and Q, this is partly due to the feasible tests not having the same large-sample properties as the infeasible tests. The general observations made on the basis of the asymptotic simulations apply equally here; finite sample size of the PR tests increases with g_z, giving rise to an increasing likelihood of concluding spurious predictive ability. As anticipated in the discussion of Section 3.1, a similar pattern of rejections is found for IV_{comb}; its sizes are close to those of t_u and Q. As regards S_B, its finite sample power increases with g_z, with the invalidity of the PR generally being detected with greater frequency than the PR tests' spurious rejections. Hence, the ability of S_B to detect PR invalidity in cases where well-known PR tests suffer problematic over-size is displayed in finite samples also.

Finally, we examine the impact of unconditional heteroscedasticity in the DGP on the size of S_B and IV_{comb} when the error processes are subject to a single break in volatility. (We do not consider t_u and Q here since these procedures are not robust to heteroscedastic errors.) Specifically, we again simulate the DGP (1)–(3) for $T = 200$ with $g_x = g_z = 0$, $e_t \sim \text{IID}\, N(0, I_3)$, but setting $d_{it} = \mathbb{I}(t \leq \lfloor \tau T \rfloor) + \sigma_i \mathbb{I}(t > \lfloor \tau T \rfloor)$ for $i = 1, 3$. We set $\tau = \{0.3, 0.7\}$ thereby allowing for two (common) volatility break timings, and $\sigma_i = \{1, 4, \frac{1}{4}\}$ allowing for both upward and downward volatility shifts (these magnitudes being substantial for illustrative purposes). We consider $c_x = \{0, 5, 10\}$ and for simplification abstract from time-varying correlation between ϵ_{xt} and ϵ_{yt} by setting $h_{21} = h_{31} = h_{32} = 0$. Table 1 reports the results for nominal 0.10-level tests (two-sided for IV_{comb}). It is clear that the size of S_B is very well controlled across all the patterns of time-varying volatility of ϵ_{xt} and ϵ_{yt}. The wild bootstrap aspect of the bootstrap methods that we propose therefore works well in achieving size close to the nominal level even for

Table 1. Finite sample size of S_B and IV_{comb} under volatility shifts: $T = 200$, $g_x = g_z = 0$, $d_{it} = 1(t \le \lfloor \tau T \rfloor) + \sigma_i 1(t > \lfloor \tau T \rfloor)$, $i = 1, 3$

		$c_x = 0$				$c_x = 5$				$c_x = 10$			
		$\tau = 0.3$		$\tau = 0.7$		$\tau = 0.3$		$\tau = 0.7$		$\tau = 0.3$		$\tau = 0.7$	
σ_1	σ_3	S_B	IV_{comb}	S_B	IV_{comb}	S_B	IV_{comb}	S_B	IV_{comb}	S_B	IV_{comb}	S_B	IV_{comb}
1	1	0.098	0.110	0.098	0.110	0.103	0.104	0.103	0.104	0.102	0.105	0.102	0.105
	4	0.101	0.109	0.101	0.112	0.106	0.107	0.105	0.111	0.105	0.108	0.107	0.110
	$\frac{1}{4}$	0.102	0.112	0.098	0.104	0.104	0.105	0.099	0.105	0.104	0.106	0.102	0.105
4	1	0.100	0.109	0.102	0.113	0.103	0.107	0.104	0.112	0.104	0.108	0.104	0.113
	4	0.099	0.109	0.102	0.117	0.107	0.110	0.107	0.119	0.106	0.114	0.109	0.123
	$\frac{1}{4}$	0.101	0.107	0.099	0.099	0.104	0.102	0.102	0.100	0.106	0.102	0.102	0.103
$\frac{1}{4}$	1	0.102	0.114	0.099	0.111	0.102	0.108	0.105	0.107	0.104	0.109	0.110	0.106
	4	0.103	0.105	0.103	0.108	0.102	0.100	0.108	0.106	0.104	0.100	0.108	0.105
	$\frac{1}{4}$	0.103	0.117	0.098	0.108	0.105	0.112	0.101	0.108	0.106	0.113	0.101	0.110

the large volatility changes that we consider. (We also simulated the finite sample size of S_B under a variety of conditionally heteroscedastic specifications, including multivariate GARCH and EGARCH, the latter an example of an asymmetric GARCH process. The size of S_B was found to be well controlled, with only minor deviations from the nominal level.) The IV_{comb} test also displays a good degree of robustness to heteroscedasticity, although size can be a little inflated for some settings.

The supplementary appendix also contains results for the same settings as above but with $g_z = 25$ and $g_z = 50$, that is, power for S_B and size for IV_{comb}, with $c_z = c_x$ and additionally allowing for a volatility break in ϵ_{zt} via $d_{2t} = \mathbb{I}(t \le \lfloor \tau T \rfloor) + \sigma_2 \mathbb{I}(t > \lfloor \tau T \rfloor)$. It is clear that the presence of (unconditional) heteroscedasticity can have a substantial influence on the level of power attainable. Other things equal, a volatility increase in ϵ_{zt} (an increase in σ_2) leads to higher S_B power, with a volatility decrease in ϵ_{zt} having the opposite effect, while volatility changes in ϵ_{yt} have the reverse effect, with an increase (decrease) in σ_3 resulting in lower (higher) power for S_B. Volatility changes in ϵ_{xt} (changes in σ_1) appear to have relatively little effect. A similar pattern of rejection frequencies is also observed for the sizes of the IV_{comb} test under heteroscedasticity. In the same cases where S_B power is increased (decreased), so the oversize of IV_{comb} increases (decreases). It appears, therefore, that S_B has attractive size and power properties in finite samples as well as in the limit, and it is encouraging to see that for the most part these carry over to situations where the errors are unconditionally heteroscedastic.

6. AN EMPIRICAL APPLICATION TO U.S. EQUITY DATA

To illustrate how our proposed procedure may be used in practice, we reconsider the results from the empirical analysis investigating the predictability of excess returns using the U.S. equity data reported in CY. CY consider four different series of stock returns, dividend-price ratio, and earnings-price ratio. The first is annual S&P 500 index data over the period 1871–2002. The other three series are annual, quarterly, and monthly NYSE/AMEX value-weighted index data (1926–2002). Full data descriptions are provided in CY. The data can be obtained from *https://sites.google.com/site/motohiroyogo/home/research/*

CY analyze the time series behavior of these data and test for predictability in excess returns (relative to an appropriate risk free rate), using as putative predictors for a variety of sample windows: the dividend-price ratio, denoted $d - p$; the earnings-price ratio, denoted $e - p$; the three-month T-bill rate, denoted r_3, and a measure of the long-short yield spread, denoted $y - r_1$. Details on the construction of these variables can be found in CY; as is conventional, excess returns and the predictor variables appear in logs. CY argue that all of these possible predictors display high persistence with, in most cases, the 95% confidence interval for the largest autoregressive root containing unity. A priori then, bivariate tests of predictability would seem to be at potential risk from the spurious predictability problem.

Table 2 reports the application of a variety of statistics to the same sets of bivariate PRs as in Table 5 of CY. Here S is our PR invalidity statistic; KPSS is the KPSS for stationarity of the predictor appearing in that regression; IV_{comb} is the PR test of BD. The S statistic is implemented using BIC selection for the order of p in the fitted regression (12), starting from $p_{\max} = 12$, with an appropriate degrees of freedom adjustment made for s_y^2. (We have simulated this means of selection of p across a number of different stationary ARMA DGPs for ϵ_{xt} and it appears to control the size of S_B well.) For the KPSS statistic the long run variance estimate is based on the QS kernel with automatic bandwidth selection. For each test, a p-value is given. For S this relates to our fixed regressor wild bootstrap test, S_B using $B = 9999$ replications; for KPSS it is based on the wild bootstrap method of Cavaliere and Taylor (2005), again using $B = 9999$; for IV_{comb} it relates to a $\chi^2(1)$ distribution. Finally, under Q, an entry of $*$ (NS) denotes that CY's Q test rejects (does not reject) the null of no predictability at the 0.10 level.

Notice first that the p-values for KPSS are relatively close to zero for most of the predictors. The KPSS test is known to reject the null of stationarity with high probability when a series displays local-to-unit root behavior (increasingly as the local-to-unity parameter approaches zero), so the p-value can be viewed as an indicator of the strength of persistence in a series (higher persistence associated with a lower p-value).

Table 2. Application to U.S. Equity Indices

Series	Obs.	Predictor	S	p-Val.	KPSS	p-Val.	IV_{comb}	p-Val.	Q
Panel A: S&P 1880–2002, CRSP 1926–2002									
S&P 500	123	$d-p$	0.358	0.057	0.669	0.043	0.187	0.426	NS
		$e-p$	1.111	0.000	0.449	0.087	1.087	0.139	∗
Annual	77	$d-p$	0.081	0.658	0.572	0.077	1.383	0.083	∗
		$e-p$	0.522	0.008	0.465	0.116	0.988	0.162	∗
Quarterly	305	$d-p$	0.531	0.017	1.201	0.007	0.474	0.319	NS
		$e-p$	1.302	0.000	0.889	0.026	0.624	0.267	∗
Monthly	913	$d-p$	1.449	0.000	2.588	0.000	− 0.423	0.337	NS
		$e-p$	1.522	0.000	1.938	0.001	− 0.139	0.445	∗
Panel B: S&P 1880–1994, CRSP 1926–1994									
S&P 500	115	$d-p$	0.346	0.081	0.495	0.028	0.388	0.350	NS
		$e-p$	1.207	0.000	0.251	0.146	1.600	0.054	∗
Annual	69	$d-p$	0.100	0.611	0.390	0.062	1.593	0.055	∗
		$e-p$	0.803	0.002	0.272	0.222	1.206	0.114	∗
Quarterly	273	$d-p$	0.894	0.001	0.753	0.009	0.451	0.327	NS
		$e-p$	2.028	0.000	0.420	0.114	0.711	0.239	∗
Monthly	817	$d-p$	1.626	0.000	1.473	0.000	− 0.598	0.276	NS
		$e-p$	2.434	0.000	0.839	0.021	− 0.164	0.435	∗
Panel C: CRSP 1952–2002									
Annual	51	$d-p$	0.368	0.051	0.351	0.210	1.286	0.099	NS
		$e-p$	0.058	0.675	0.244	0.270	0.979	0.163	NS
		r_3	0.071	0.726	0.269	0.151	− 1.391	0.082	NS
		$y-r_1$	0.085	0.657	0.626	0.014	0.472	0.381	NS
Quarterly	204	$d-p$	0.518	0.017	0.645	0.062	1.128	0.129	NS
		$e-p$	1.511	0.000	0.550	0.064	0.764	0.223	NS
		r_3	0.071	0.659	0.585	0.017	− 2.661	0.004	∗
		$y-r_1$	0.235	0.146	0.855	0.003	0.946	0.172	∗
Monthly	612	$d-p$	0.345	0.073	1.449	0.004	0.550	0.290	NS
		$e-p$	1.729	0.000	1.264	0.004	0.363	0.358	NS
		r_3	0.091	0.535	1.296	0.000	− 3.439	0.000	∗
		$y-r_1$	0.422	0.028	1.373	0.000	1.856	0.032	∗

NOTES: Returns are for the annual S&P 500 index and the annual, quarterly, and monthly CRSP value-weighted index. The predictor variables are the log dividend-price ratio $d-p$, the log earnings-price ratio $e-p$, the three-month T-bill rate r_3, and the long-short yield spread $y-r_1$. In the column headed Q, ∗ (NS) indicates those cases where the Q test of Campbell and Yogo (2006) rejects (does not reject) the null hypothesis of no predictability at the 10% level. The columns headed p-val. indicate the p-values of the tests in the preceding column calculated as detailed in the main text.

We conclude that, in accordance with the findings of CY and BD, these possible predictors all display (to differing degrees) strongly persistent behavior. The least persistent appears to be the annual log earnings-price ratio, $e-p$, regardless of which sample window is considered. Interestingly, while CY suggest that r_3 and $y-r_1$ are the least persistent variables, we find small p-values for these series in almost every case, suggesting they are strongly persistent.

For both the full sample results in Panel A and the sub-sample considered in Panel B, the Q test delivers rejections at the 0.10 level in the case of $e-p$, for all four of the data series considered. The Q test also rejects at the 0.10 level for $d-p$, but only for annual data. The IV_{comb} test also generally rejects with annual data. These results, when taken at face value, signal significant predictability of excess returns by $e-p$ in particular, but also by $d-p$ with annual data. However, in the case of $e-p$ any such conclusions of predictability are immediately thrown into serious question once we observe that S_B also rejects very strongly in all these cases, suggesting that such a PR model is potentially spurious, or at the very least, under-specified by some unincluded persistent process. Interestingly, in the annual data the S_B test for $d-p$ is highly insignificant in both Panels A and B suggesting no evidence that the significant outcome of the Q test is spurious here. So although the evidence from the Q tests alone suggests that $e-p$ has predictive power for excess returns with a less consistent body of evidence of predictability from $d-p$, a consideration of the Q tests in tandem with S_B suggests that the stronger evidence for genuine predictability may well lie with $d-p$; indeed the results are not inconsistent with $d-p$ being an omitted manifest persistent predictor when testing for predictability from $e-p$.

Turning to the results in Panel C, the Q test is seen to be significant at the 0.10 level only for r_3 and $y-r_1$ for quarterly and monthly, but not annual, data. Among these cases, only $y-r_1$ for monthly data is flagged up as potentially spurious by S_B. Consequently, with this exception, the rejections delivered by Q in Panel C do not appear problematic when judged by our PR validity test. For the IV_{comb} test in Panel C, significant predictability at the 0.10 level is again (as with Q) signaled for monthly r_3 and monthly $y - r_1$, but also signaled for annual $d - p$ and both annual and quarterly r_3. The results for S_B again suggest that most of these rejections do not appear to be obviously problematic, although S_B does reject at roughly the 0.05 level for annual $d - p$.

7. CONCLUSIONS

In this article, we have examined the issue of spurious predictability that can potentially arise with recently proposed tests for predictability. We have shown that the outcomes from these tests have considerable potential to spuriously signal that a putative predictor is a genuine predictor whenever unincluded persistent (manifest and/or latent) variables are present in the underlying data generation process. To guard against this possibility, we have proposed a diagnostic test for such PR invalidity based on a well-known stationarity testing approach. To again allow for an unknown degree of persistence in the putative (and latent) predictors, and to allow for both conditional and unconditional heteroscedasticity in the data, a fixed regressor wild bootstrap test procedure was proposed and its asymptotic validity established. Doing so required us to establish some novel asymptotic results pertaining to the use of the fixed regressor bootstrap with nonstationary regressors, which are likely to have important applications beyond the present context. Monte Carlo simulations were reported which suggested that our proposed methods work well in practice. A reconsideration of the empirical study of the predictability of U.S. stock returns reported in CY highlighted the potential value of our procedure in practice.

We have proposed what we believe to be the first serious diagnostic testing exercise in the context of fitted PRs, suggesting within-sample misspecification tests directed to have power to detect the presence of persistent variables in the underlying DGP but not included in the PR. We hope that this article encourages further research in this area, developing additional within- and out-of-sample diagnostic procedures for PRs.

SUPPLEMENTARY MATERIALS

This supplement contains the additional Monte Carlo simulation results described in sections 4.4 and 5, together with mathematical proofs for the large sample results given in sections 3 and 4 of the article.

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