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A Simple Parametric Model Selection Test

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ABSTRACT

We propose a simple model selection test for choosing among two parametric likelihoods, which can be applied in the most general setting without any assumptions on the relation between the candidate models and the true distribution. That is, both, one or neither is allowed to be correctly specified or misspecified, they may be nested, nonnested, strictly nonnested, or overlapping. Unlike in previous testing approaches, no pretesting is needed, since in each case, the same test statistic together with a standard normal critical value can be used. The new procedure controls asymptotic size uniformly over a large class of data-generating processes. We demonstrate its finite sample properties in a Monte Carlo experiment and its practical relevance in an empirical application comparing Keynesian versus new classical macroeconomic models. Supplementary materials for this article are available online.

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One-step test; Sample-splitting; Uniform size control; Vuong test

1. Introduction

Model selection is an important step in most empirical work and, accordingly, there exists a vast literature devoted to this issue (Cox 1961, 1962; Atkinson 1970; Zellner 1971; Leamer 1983; Mizon and Richard 1986; Smith 1992; Gourieroux and Monfort 1994; Sin and White 1996; Andrews 1997, 1999; Chesher and Smith 1997; Smith 1997; Andrews and Lu 2001; Ramalho and Smith 2002; Hong, Preston, and Shum 2003; Kitamura 2003). Since Akaike (1973, 1974), the Kullback–Leibler (KL) information criterion has become a popular measure for descriminating among models taking the form of parametric likelihoods, especially in the context of nested generalized linear models ("analysis of deviance;" e.g., Nelder and Wedderburn 1972; McCullagh and Nelder 1989). One strand of the literature (Nishii 1988; Vuong 1989; Sin and White 1996; Inoue and Kilian 2006 among others) uses this criterion together with earlier ideas about embedding the model selection problem into a classical hypothesis testing framework (e.g., Hotelling 1940; Chow 1980). In essence, this approach uses the maximum of the likelihood function as a goodness-of-fit measure. If model A is found to have a statistically significantly larger maximum likelihood than model B, then model A is to be preferred.

In an influential article, Vuong (1989) established that, unfortunately, the difference between the KL information criterion (KLIC) of two competing models exhibits a wide variety of limiting distributions (normal, χ^2, or even mixtures of χ^2), depending on whether the two models are overlapping or not, or whether one of the models is correctly specified or not. As a result, using the KLIC typically requires pretesting to establish which distribution to use for the computation of critical values for the tests. There are two reasons why the resulting two-step model selection test exhibits nonuniform behavior under the null and thus may suffer from size distortions: first, the existence of different asymptotic distributions of the test statistic implies that size distortions can occur when models are nonnested but "close" to each other. Second, the use of a pretest induces the well-known nonuniformity of two-step testing procedures (Leeb and Pötscher 2005) that may also lead to size distortions. Shi (2015) sought to address this issue by proposing a modified Vuong test for nonnested models that uniformly controls size but involves solving potentially high-dimensional optimization problems to find the appropriate critical values from a nonstandard limiting distribution.

In this article, we instead propose a simple method that delivers a model selection criterion based on the KL discrepancy and yet only involves a test statistic that is asymptotically $N(0, 1)$-distributed in all cases (nested, nonnested, or overlapping), under the null that the two models fit the data equally well. Therefore, no pretesting is required, complicated limiting distributions are entirely avoided, the test uniformly controls size, and we show in simulations that it may be significantly more powerful than Vuong's test. In fact, we provide simulation results in which the Vuong test's power is close to the test's nominal size while our test has power close to one. These advantages do come at the expense of some power loss relative to Vuong's test when the models are nested. However, our simulations suggest that this effect is small and therefore insufficient to offset the advantages of the method. In addition, our simulations suggest that neither Shi's nor our test generally dominates the other in terms of power or its ability to control size.

We test the hypothesis that two models have the same KL discrepancy to the true distribution versus one of them being

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Supplementary materials for this article are available online. Please go to www.tandfonline.com/r/JASA.

smaller. In case of a rejection, the model with the smaller discrepancy is retained, otherwise the criterion suggests both models fit the data equally well. Our approach remains valid even if both models are misspecified and enables the selection of the least misspecified of the two, that is, the model with the smallest KL discrepancy from the truth. This capability fits nicely within the context of valid likelihood inference under potential model misspecification (White 1982). We handle the possibility of overlapping models by devising an estimator of the KLIC that smoothly interpolates between a conventional sample-splitting scheme (e.g., Yatchew 1992; Whang and Andrews 1993) when the competing models overlap and a conventional full-sample estimator when the models do not overlap. In this fashion, the statistic of interest is never degenerate. The relative weights of the split-sample and the full-sample statistics are governed by a regularization parameter that we choose so as to trade off power and size of the test. The optimal regularization parameter requires only estimates of variance terms and therefore is very easy to compute from a given sample. In this fashion, we avoid having to consider higher-order terms of the test's asymptotic expansion (as in Vuong 1989, or, in a different hypothesis testing context, Fan and Li 1996). Although higher-order expansions (such as Edgeworth expansions) can, in principle, be used to address the degeneracy problem, such an approach may pose significant practical problems. For instance, a higher-order analysis of likelihood functions may involve quantities that are difficult to calculate for complex forms of likelihood functions (such as when it is obtained via numerical methods and/or simulations).

Besides deriving the local asymptotic power of our test we also show that it is of correct asymptotic level uniformly over a large class of data-generating processes. This is a very desirable property of a test, particularly in the model selection context, as it may be difficult to judge a priori whether competing models are "close" to each other—a case in which the Vuong test exhibits potentially very large finite sample distortions due to its nonuniform behavior under the null. We also demonstrate our procedure's small sample properties in a Monte Carlo study and illustrate its practical usefulness in testing Keynesian versus new classical macroeconomic models. Finally, we discuss how our approach may be extended in various directions such as time series data or models defined by moment conditions. Importantly, we can also apply our sample-splitting idea to tests comparing the accuracy of forecasts (such as those made popular by Diebold and Mariano 1995) to gain asymptotic uniform size control.

Model selection is an important step in empirical research as indicated by its vast coverage in standard statistical textbooks and in statistics courses, and the large citation count of seminal articles such as Vuong (1989). In many applications such as the one we discuss in Section 9, testing which of two models possesses a smaller KL discrepancy to the truth may be of direct interest. This is, for example, the case when the two models are observable implications from competing economic theories and the model selection test then speaks to the question which of the two theories (jointly with some distributional assumptions) is a better description of the economy. Another example is that of distinguishing different theories of voter behavior as described by Shi (2015). The outcome of Diebold and Mariano (1995)-type tests of which forecasting model is more accurate is also of direct interest. In all of these examples, the model selection step is not necessarily followed by another estimation or inference step.

The proofs of all results in this article can be found in the supplementary materials.

2. Setup

In this article, we define a model to consist of a set of probability distributions over the sample space of observed variables, indexed by a finite-dimensional parameter. For example, we subsequently use models A and B defined as

$$\mathcal{P}_A := \{P_{\theta_A} \in \mathbf{P} : \ \theta_A \in \Theta_A\},$$
$$\mathcal{P}_B := \{P_{\theta_B} \in \mathbf{P} : \ \theta_B \in \Theta_B\},$$

where $\mathbf{P}$ denotes the set of all probability measures and Θ_A and Θ_B are some finite-dimensional parameter sets. Such a set of distributions could, for example, be the set of all normal distributions indexed by their means and variances. An integral part in any model selection procedure consists of choosing a criterion that measures "closeness" of two models. We consider the KLIC here because it has a variety of convenient properties one of which being that maximum likelihood estimators of θ_A in model A, say, are known to minimize the KL distance (even though the KL discrepancy is not a distance metric, we will use the two terms interchangeably) between model A and the true data-generating process (White 1982). Consequently, the so-called pseudo-true parameter value θ_A^* that maximizes the population likelihood of model A delivers a distribution $P_{\theta_A^*}$ equal to the true distribution P_0 if model A is correctly specified, and can be interpreted as the best approximating model (in terms of KL distance) in the case that model A is misspecified.

More formally, define the KL distance between two distributions P and Q, or if they possess densities p and q, respectively, as

$$K(P : Q) := \int \ln\left(\frac{dP}{dQ}\right) dP = E_P\left[\ln\left(\frac{p(X)}{q(X)}\right)\right].$$

Here we assume that P is absolutely continuous with respect to Q. Otherwise, we define the KL distance to equal $+\infty$. The pseudo-true value θ_A^* of a model A is then defined as the one that minimizes the KL distance between model A and the true distribution P_0, viz., $\theta_A^* := \arg\min_{\theta_A \in \Theta_A} K(P_0 : P_{\theta_A})$, and similarly for model B, $\theta_B^* := \arg\min_{\theta_B \in \Theta_B} K(P_0 : P_{\theta_B})$. Under standard conditions, (quasi-) maximum likelihood estimators consistently estimate this parameter (Akaike 1973; Sawa 1978). If model A is correctly specified, defined as $P_0 \in \mathcal{P}_A$, then there is a true parameter $\theta_0 \in \Theta_A$ such that $P_0 = P_{\theta_A^*} = P_{\theta_0}$. We call model B nested in model A if $\mathcal{P}_B \subset \mathcal{P}_A$, nonnested if neither model is nested in the other, overlapping if $\mathcal{P}_B \cap \mathcal{P}_A \neq \emptyset$ and nonoverlapping (or strictly nonnested) otherwise.

The goal of this article is to propose a model selection test for determining the model that fits the data "better." We define a model to be better if it is closer to the true distribution in the KL sense. $P_{\theta_A^*}$ and $P_{\theta_B^*}$ are the distributions in $\mathcal{P}_A$ and $\mathcal{P}_B$, which are closest to the truth, P_0, respectively. Formally, model A is defined to be better than model B if model A's KL distance to the truth is smaller than that of model B, that is, $K(P_0 : P_{\theta_A^*}) < K(P_0 : P_{\theta_B^*})$.

If the two KL distances are equal, then we say models A and B are equivalent. The procedure proposed in the next two sections selects the better model based on performing a test of

$$H_0 : K(P_0 : P_{\theta_A^*}) = K(P_0 : P_{\theta_B^*}),$$

that is, models A and B are equivalent, against model A is better, $H_A : K(P_0 : P_{\theta_A^*}) < K(P_0 : P_{\theta_B^*})$, or model B is better, $H_B : K(P_0 : P_{\theta_A^*}) > K(P_0 : P_{\theta_B^*})$.

Before proceeding to the actual model selection test, we conclude this section with the collection of a few formal definitions. To that end, let $X_i : \Omega \mapsto \mathcal{X}$, $i = 1, 2, \ldots$, be random vectors on the probability space $(\Omega, \mathcal{F}, Q_0)$ with $\mathcal{F}$ a σ-algebra and Q_0 a probability measure on Ω. Further, suppose $\mathcal{X}$ is a Polish space $\mathcal{X}$, that is, a complete separable metric space, and $\mathcal{B}_x$ the Borel σ-algebra on $\mathcal{X}$. Denote by μ some underlying σ-finite measure on $(\mathcal{X}, \mathcal{B}_x)$, for example, the Lebesgue measure on $\mathcal{X} = \mathbb{R}^k$. Finally, let $\mathbf{P}$ be the set of all distributions on $\mathcal{X}$, which have a measurable density with respect to μ.

3. The Test Statistic

To motivate our proposed test statistic, we first briefly describe the so-called degeneracy problem that complicates the use of existing test statistics.

Let $\theta := (\theta_A', \theta_B')' \in \Theta := \Theta_A \times \Theta_B \subset \mathbb{R}^p$, let ∇_{θ_k} denote gradient vectors with respect to θ_k, $k = A, B$, and define the moment conditions

$$E_{P_0}\, g(X;\theta) := E_{P_0}\left[\begin{pmatrix}\nabla_{\theta_A} \ln f_A(X;\theta_A)\\ \nabla_{\theta_B} \ln f_B(X;\theta_B)\end{pmatrix}\right] = 0, \qquad (1)$$

which are satisfied by the pseudo-true value $\theta^* := (\theta_A^{*\prime}, \theta_B^{*\prime})'$. Let $d^* := E_{P_0}[\ln f_A(X, \theta_A^*) - \ln f_B(X, \theta_B^*)]$ be the pseudo-true log-likelihood ratio of the two models. Assume that we have an iid sample $X_1, \ldots, X_n$ from P_0 and let $\hat{g}(\theta) := \sum_{i=1}^n g(X_i;\theta)/n$. We assume that $\hat{\theta} := (\hat{\theta}_A', \hat{\theta}_B')'$ is the maximum likelihood estimator of θ^*, but in principle one could use any estimator that solves the empirical analog of (1), that is, $\hat{g}(\hat{\theta}) = o_p(1)$, typically called a "Z-estimator." (See van der Vaart 1998 chap. 5 for an introduction.) GMM and GEL estimators of θ^* are examples of such estimators.

For $k = A, B$, define the variances $\sigma_k^2 := \mathrm{var}_{P_0}(\ln f_k(X;\theta_k^*))$, the covariance $\sigma_{AB} := \mathrm{cov}_{P_0}(\ln f_A(X;\theta_A^*), \ln f_B(X;\theta_B^*))$, and the variance of the likelihood ratio $\sigma^2 := \sigma_A^2 - 2\sigma_{AB} + \sigma_B^2$. Let $\hat{d}$ be the empirical log-likelihood ratio $\hat{d} := n^{-1}\sum_{i=1}^n \ln(f_A(X_i;\hat{\theta}_A)/f_B(X_i;\hat{\theta}_B))$ and define the sample variance estimators $\hat{\sigma}_k^2$ of σ_k^2, $k = A, B$, and the covariance estimator $\hat{\sigma}_{AB}$ of σ_{AB}, that is, $\hat{\sigma}_k^2 := n^{-1}\sum_{i=1}^n (\ln f_k(X_i;\hat{\theta}_k) - \overline{\ln f_k})^2$ where $\overline{\ln f_k} := n^{-1}\sum_{i=1}^n \ln f_k(X_i;\hat{\theta}_k)$ and similarly for $\hat{\sigma}_{AB}$. The variance of the likelihood ratio, σ^2, we then estimate by $\hat{\sigma}^2 := \hat{\sigma}_A^2 - 2\hat{\sigma}_{AB} + \hat{\sigma}_B^2$.

Define t_n to be the t-statistic for testing $H_0 : d^* = 0$, that is, $t_n := \sqrt{n}\hat{d}/\hat{\sigma}$. This statistic is equivalent to the one Vuong (1989) proposed when the two candidate models are known to be nonnested. The t-statistic possesses a standard normal limiting distribution if $\sigma^2 > 0$. The type of degeneracy ruled out by this assumption, however, poses a standard challenge encountered in parametric model selection testing. It requires that the variance of the log-likelihood ratio evaluated at the pseudo-true values is nonzero. This condition is violated when both models A and B are observationally equivalent, that is, when both are correctly specified, which implies that (i) they must be overlapping (including the nested case) and (ii) the truth must be an element of their intersection. Then the pseudo-true densities are identical, $f_A(\cdot;\theta_A^*) \equiv f_B(\cdot;\theta_B^*)$, which in turn implies that the variance σ^2 is zero.

The common solution in the literature has been to either assume this case away or develop a pretest for testing whether degeneracy holds or not. See Vuong (1989), Kitamura (2000), and Kitamura (2003) for a discussion of issues related to degeneracy and pretests that have been suggested.

We now propose a modified version of the t-statistic that preserves the standard normal limiting distribution even when the models are observationally equivalent. There are several ways one could think of regularizing the model selection problem. The approach we present here is based on reweighting the individual log-likelihoods, which is very simple to implement and results in desirable properties of the resulting test (see Section 5). Furthermore, the efficiency loss in the "nondegenerate" observationally distinct case seems to be small in finite samples and is, in fact, asymptotically negligible under simple conditions.

For simplicity of exposition assume that the sample size n is an even number. We propose to reweight the individual log-likelihoods

$$\hat{\tilde{d}} := \frac{1}{n}\sum_{i=1}^n \Big(\omega_i(\hat{\varepsilon}_n)\ln f_A(X_i;\hat{\theta}_A) - \omega_{i+1}(\hat{\varepsilon}_n)\ln f_B(X_i;\hat{\theta}_B)\Big)$$

with the weights

$$\omega_k(\hat{\varepsilon}_n) := \begin{cases}1, & k \text{ odd}\\ 1+\hat{\varepsilon}_n, & k \text{ even}\end{cases}, \quad k = 1, \ldots, n+1 \qquad (2)$$

that depend on a possibly data-dependent, real-valued regularization parameter $\hat{\varepsilon}_n$. Straightforward algebra shows that the asymptotic variance of $\sqrt{n}\hat{\tilde{d}}$ can be estimated by $\hat{\tilde{\sigma}}^2$, where

$$\hat{\tilde{\sigma}}^2 := \left(1+\hat{\varepsilon}_n\right)\hat{\sigma}^2 + \frac{\hat{\varepsilon}_n^2}{2}\left(\hat{\sigma}_A^2 + \hat{\sigma}_B^2\right).$$

With the modified estimator of d^* and its variance estimator, we can construct a new t-statistic $\tilde{t}_n$ defined as

$$\tilde{t}_n := \frac{\sqrt{n}\hat{\tilde{d}}}{\hat{\tilde{\sigma}}}.$$

If $\hat{\varepsilon}_n = 0$, then $\hat{\tilde{\sigma}} = \hat{\sigma}$ and $\hat{\tilde{d}} = \hat{d}$, and the modified and unmodified t-statistics are equivalent, that is, $\tilde{t}_n = t_n$. Now, suppose $\hat{\varepsilon}_n \neq 0$. In the observationally distinct models case, the two statistics differ only in that some observations are weighted by $1 + \hat{\varepsilon}_n$ rather than by one. To understand how the weights $\omega_k(\hat{\varepsilon}_n)$ regularize the t-statistic in the equivalent models case, rewrite the new statistic as

$$\tilde{t}_n = \frac{\sqrt{n}(\hat{d} + \hat{\varepsilon}_n\hat{d}_{\text{split}})}{\hat{\tilde{\sigma}}}$$

with

$$\hat{d}_{\text{split}} := \frac{1}{n}\sum_{i=1}^{n/2}(\ln f_A(X_{2i};\hat{\theta}_A) - \ln f_B(X_{2i-1};\hat{\theta}_B)).$$

This representation shows that the numerator of $\tilde{t}_n$ is equal to a weighted sum of the conventional full-sample log-likelihood ratio $\hat{d}$ and the split-sample log-likelihood ratio $\hat{d}_{\text{split}}$, which computes the log-likelihood of model A from the odd observations and that of model B from the even observations. As the data are assumed to be iid, the variance of the split-sample statistic is always nonzero regardless of whether the models are observationally distinct or equivalent. The parameter $\hat{\varepsilon}_n$ determines how much of the split-sample statistic should be added to the full-sample counterpart. Equivalent models lead to identical densities, that is, $\ln f_A(\cdot;\theta_A^*) \equiv \ln f_B(\cdot;\theta_B^*)$ and, therefore, t_n has a degenerate distribution. The new statistic $\tilde{t}_n$, however, continues to be nondegenerate because of the split-sample term. When $\hat{\varepsilon}_n \to_p 0$ at a suitable rate (notice that the assumptions of Theorem 1 do not actually require the regularization parameter to vanish with the sample size. We only need it to be bounded in probability), the net effect of the proposed regularization approach is to reduce to a sample splitting device in the observationally equivalent models case, while smoothly reverting to the conventional full-sample expression as the models move away from perfect overlap.

There are multiple alternative ways one could modify the full-sample likelihood ratio statistic so as to obtain some of the desirable properties of our proposed test such as uniform asymptotic size control and the avoidance of a pretest. For example, one could define a t-statistic based only on the split sample statistic $\hat{d}_{\text{split}}$, as sample-splitting is a known and effective way to address degeneracy issues in test statistics (e.g., Yatchew 1992; Whang and Andrews 1993). However, when models are nonnested such a statistic may suffer from poor power as it ignores half of the sample whereas our proposed statistic does not because it asymptotically equals the full-sample likelihood ratio statistic in that case.

Another simple alternative that may at first appear attractive would be to simply pretest whether σ^2 is significantly different from zero and accordingly use a full sample or a split sample Vuong statistic based on the result of the pretest. While we leave the derivation of its theoretical properties for future research, we conjecture that such a two-step testing procedure is likely to suffer from similar lack of uniformity and power loss as the two-step Vuong test.

In general, two-step approaches with a discontinuous change in the second step's test statistic likely possess poor uniformity properties. A practical consequence of this problem is that practitioners could often be in the situation that very small changes to the data could yield dramatic changes in the test's p-value, which would make it hard to access the level of confidence that the chosen model is the correct one. A smooth transition between sample splitting and no sample splitting elegantly avoids this theoretical and practical problem.

The benefit of our regularization scheme is that the strong nonsingularity condition $\sigma^2 > 0$ can be replaced by the following very weak condition.

Assumption 1. For $k = A, B$, $\sigma_k^2 > 0$, $\text{var}_{P_0}((\ln f_k(X;\theta_k^*))^2) > 0$, and $\text{var}_{P_0}(\nabla_{\theta_k} \ln f_k(X;\theta_k^*))$ is nonsingular.

We also need standard conditions for Z-estimators to be consistent and asymptotically normal. They can be weakened substantially, but serve as a simple basis to discuss the relevant issues in our model selection framework.

Assumption 2. $\Theta \subset \mathbb{R}^{d_\theta}$ is compact and $\ln f_k(x;\cdot)$, $k = A, B$, are twice continuously differentiable.

For $k = A, B$, let $\nabla^2_{\theta_k}$ denote the Hessian matrix of a function of θ_k, containing derivatives with respect to elements of θ_k.

Assumption 3. (i) $X_1, \ldots, X_n$ is an iid sequence of random variables with common distribution $P_0 \in \mathbf{P}$. (ii) There is a unique $\theta^* \in \text{int}(\Theta)$ so that $E_{P_0} g(X;\theta^*) = 0$. (iii) $E_{P_0}[\nabla^2_{\theta_k} \ln f_k(X;\theta_k^*)]$, $k = A, B$, are invertible.

Assumption 3(ii) can be overly restrictive because likelihoods with a unique global maximizer may possess more than one root of the corresponding first-order conditions. This means Θ has to be chosen sufficiently small so as to exclude roots not corresponding to the global maximum. The assumption is made here to simplify the exposition. In practice, however, one may simply estimate θ_A and θ_B separately by standard maximum likelihood assuming that there is a unique global maximizer.

The remainder of Assumption 3, Assumptions 1 and 2 are not very restrictive and could be termed standard regularity conditions. We also impose some moment existence conditions on the individual likelihoods and their derivatives:

Assumption 4. (i) $E_{P_0}[\|\nabla_{\theta_k} \ln f_k(X,\theta_k^*)\|^{2+\delta}] < \infty$ and $E_{P_0}[|\ln f_k(X,\theta_k^*)|^{4+\delta}] < \infty$ for $k = A, B$ and some $\delta > 0$. (ii) There exists a function $\bar{F}_1(x)$ such that $E_{P_0}\bar{F}_1(X) < \infty$ and, for $j, k = A, B$, for all $\theta = (\theta_A', \theta_B')' \in \Theta$, for all $x \in \mathcal{X}$, and for $h(x;\theta)$ being any of the functions $\ln f_k(x;\theta_k)$, $\text{vec}(\nabla^2_{\theta_k} \ln f_k(x;\theta_k))$ and $\ln f_k(x;\theta_k)\nabla_{\theta_j} \ln f_j(x;\theta_j)$, we have $\|h(x;\theta)\| \le \bar{F}_1(x)$. (iii) There exists a function $\bar{F}_2(x)$ such that $E_{P_0}[|\bar{F}_2(X)|^{2+\delta}] < \infty$ and $\|\nabla_{\theta_k} \ln f_k(x;\theta_k)\| \le \bar{F}_2(x)$ for all $x \in \mathcal{X}$ and $k = A, B$.

Finally, we place restrictions on the regularization parameter. First, we define the set of positive sequences that are $O(1)$ but converge to zero only at a rate slower than $n^{-1/4}$.

Definition 1. Let $\mathcal{E}$ be the set of sequences $\{\varepsilon_n\}$ in $\mathbb{R}$ such that $\varepsilon_n > 0$ for all $n \ge 1$, $n^{1/4}\varepsilon_n \to \infty$, and $\varepsilon := \lim_{n\to\infty} \varepsilon_n < \infty$.

Assumption 5. $\hat{\varepsilon}_n$ is a sequence of real-valued, measurable functions of $X_1, \ldots, X_n$ such that there exists a sequence $\{\varepsilon_n\} \in \mathcal{E}$ with $|\hat{\varepsilon}_n - \varepsilon_n| = O_{P_0}(n^{-1/2})$.

Notice that this assumption allows for constant ($\hat{\varepsilon}_n \equiv \varepsilon \neq 0$), deterministic and random sequences of regularization parameters $\{\hat{\varepsilon}_n\}$ as long as they do not vanish too quickly and $\{\hat{\varepsilon}_n\}$ lies in the $n^{-1/2}$-neighborhood of some deterministic sequence $\{\varepsilon_n\}$ in $\mathcal{E}$. Intuitively, we need the condition $n^{1/4}\varepsilon_n \to \infty$ to make sure that the regularization parameter does not tend to zero too quickly, otherwise it would not have any regularizing effect (at least asymptotically).

The following theorem establishes that the regularized t-statistic is asymptotically standard normal regardless of whether the two models are observationally equivalent or not.

Theorem 1. If Assumptions 1–5 hold, then, under H_0, $\tilde{t}_n \to_d N(0,1)$ and, under $H_A \cup H_B$, $|\tilde{t}_n| \to_p \infty$.

Remark 1. Conditional densities can be accommodated just as described by Vuong (1989).

Remark 2. The requirement $\varepsilon_n \neq 0$ (but possibly $\varepsilon_n \to 0$) is necessary only for the limiting distribution of $\tilde{t}_n$ to be nondegenerate in the observationally equivalent case. Therefore, if it is known a priori that the two models A and B are observationally distinct (e.g., strictly nonnested), $\varepsilon_n \equiv 0$ is permitted. However, Section 5 shows that tests based on sequences that do satisfy the requirements of $\mathcal{E}$ uniformly control size. Since observationally distinct models can be "close" to observationally equivalent in finite samples, one may want to employ nonzero sequences $\{\hat{\varepsilon}_n\}$ even in such cases.

Remark 3. The functional form of the weights $\omega_k(\varepsilon)$ in (2) can be seen as a normalization in the following sense. In Section 6, we provide a data-driven choice of $\hat{\varepsilon}_n$ that optimizes a particular power and size trade-off given the functional form of $1 + \hat{\varepsilon}_n$ for weighting the even observations. For any other functional form of the weight, say $\bar{w}_k(\hat{\varepsilon}_n)$, the optimal $\hat{\varepsilon}_n$ would then be such that $\bar{w}_k(\hat{\varepsilon}_n) = 1 + \hat{\varepsilon}_n$ as long as the range of the function $\bar{w}_k$ is large enough. On the other hand, consider choosing some constant, say c, other than 1 for weighting the odd group together with the appropriate adjustment to the standard deviation in the denominator of $\tilde{t}_n$. This modified test statistic is numerically equivalent to our test statistic when the optimal epsilon, now $c(1 + \hat{\varepsilon}_n) - 1$ with $\hat{\varepsilon}_n$ the optimal choice under $c = 1$, is employed.

Our test statistic relies on assigning individual observations to two groups. Clearly, the test statistic is invariant to sample reorderings that permute observations within the two groups, but do not reassign observations across the two groups. In the remainder of this section, we discuss in what sense our statistic is asymptotically invariant under reassignment of observations across groups and the impact of such reassignments in finite samples.

We introduced our test statistic by splitting the sample into odd and even observations, which was purely for concreteness and ease of presentation. As Theorem 1 shows, the limiting distribution of our test statistic does not depend on the definition of the two groups. In fact, any other partition of the sample into two groups yields the same asymptotic distribution. In this sense, reordering has no effect on the test statistic in large samples. The supplement of this article shows that not only does every partition of the sample into two groups lead to the same asymptotic distribution, but also the random difference between two test statistics based on different assignment rules is negligible in large samples. This result requires that one partition into two groups can be constructed from the other partition by $o(n)$ reassignments of observations across groups.

Even though this result provides a sense in which our test statistic is asymptotically invariant to reassignment of observations across groups, one may be concerned that, in a finite sample, the invariance may not hold. One should realize, however, that our critical values account for fluctuations due to different sample orderings, so one would have to try about 100 different reassignments of observations across groups before finding one leading to a false rejection of the null at the 99% level (and this is assuming that reassignment is the only source of noise, which is not the case, so, in reality, even more permutations than this would have to be tried to stumble on a permutation yielding a false rejection). The fact that our critical values account for the reassignment noise is an automatic consequence of the fact that they account for the usual sampling noise. Indeed, a reordered sample is just another possible random draw from the population distribution.

To check robustness of the model selection results in finite samples, the user of our test may want to report summary statistics of covariates in the two groups. Balance of such summary statistics across the two groups ensures that estimates and test results are not driven by significant (observable) differences across the two groups. In fact, one could randomly assign observations to two groups to guarantee balance not only on observable, but also on unobservable characteristics.

Splitting samples of observations into two groups is common practice in randomized control trials, and the effect of randomization, stratification, and possible imbalance on estimators and test statistics is well-understood in that literature. The same advantages and disadvantages carry over to our context of model specification tests.

4. The Model Selection Test

The results of the previous section suggest a very simple model selection procedure based on a two-sided (alternatively, one could use a one-sided t-test with obvious modifications to the procedure) t-test: Given a nominal level $\alpha \in (0, 1)$ and some finite $\hat{\varepsilon}_n$ such as the optimal choice proposed in Section 6, we compute the test statistic $\tilde{t}_n$ and compare its absolute value to the $(1 - \alpha/2)$-quantile $z_{1-\alpha/2}$ from the $N(0, 1)$ distribution. If $|\tilde{t}_n| > z_{1-\alpha/2}$, then reject the null that model A and B are equally close to the truth. The rejection is in favor of model A if $\tilde{t}_n > z_{1-\alpha/2}$ and in favor of model B if $\tilde{t}_n < -z_{1-\alpha/2}$. No pretesting is necessary and, in contrast to available methods, no complicated asymptotic distributions ever need to be used.

The simulation of critical values from the mixture of χ^2 distributions in Vuong's (1989) test requires the estimation of eigenvalues of a potentially large matrix, which are then to be used as the mixture weights. Such estimators may be quite imprecise in small samples and can induce further distortions. Shi's (2015) test, on the other hand, requires some conservative critical value because the exact limiting critical value cannot be estimated consistently. The conservative critical value is then determined as the supremum over a potentially very large space of nuisance parameters, which can be an expensive numerical task.

Interestingly, conditional on a given selected model, asymptotically valid confidence regions for its parameters can be readily obtained by using the first-order conditions of its likelihood maximization problem. This scheme automatically recovers the well-known "sandwich" formula for misspecification-robust estimation of the asymptotic variance (White 1982; Owen 2001). Of course, model estimation following a model selection procedure always carries the risk that the model selection step may influence the significance levels of subsequent tests. As our approach selects the best model of the two with probability approaching one, the model selection step has, asymptotically, no effect on further pointwise inference. Remark 4 discusses uniformity properties of our procedure.

In the presence of a priori information justifying the exclusion of the observationally equivalent models case, the same test can be performed using the test statistic t_n instead of $\tilde{t}_n$. In certain modeling situations, it might be straightforward to check whether the condition $\sigma^2 > 0$ is satisfied. For example, one might have reasons to believe that both models are only crude approximations to the truth so that both are misspecified. If, in addition, it can be established analytically that the models do not overlap, then $\sigma^2 > 0$ holds and the test without regularization can be used.

5. Large Sample Properties of the Test

5.1. Uniformity

In this section, we define a set $\mathcal{P}$ that contains all distributions under which the moment conditions and some regularity conditions similar to those in the previous section hold. Then we show that our regularized test controls size uniformly over those distributions in $\mathcal{P}$ that also satisfy the null hypothesis.

In view of the impossibility result by Bahadur and Savage (1956) and its extensions in Romano (2004), we cannot hope to gain uniform size control over general nonparametric classes of distributions. It has been recognized before (see sec. 11.4.2 in Lehmann and Romano 2005, for instance) that Lyapounov's condition (see eq. (23.35) in Davidson 1994, for example). places sufficient restrictions on the space of distributions so that one can establish uniformity for t-statistics. The following definition of the set of distributions $\mathcal{P}$ follows that route and ensures that the Lyapounov condition holds for several components of our test statistic. This can be seen as a strengthening of the assumptions in Section 3 to allow for asymptotic theory under sequences of data-generating processes.

Subsequently, we need to be more specific about under which distribution P certain quantities are computed. Define $\theta^*(P) := (\theta_A^*(P)', \theta_B^*(P)')'$ to be the parameter value that satisfies $E_P g(X_i; \theta^*(P)) = 0$ and $d^*(P) := E_P[\ln f_A(X; \theta_A^*(P)) - \ln f_B(X; \theta_B^*(P))]$. Let $\sigma_k^2(P) := \mathrm{var}_P(\ln f_k(X; \theta_k^*(P))$, $\tilde{\sigma}^2(\theta, P, \varepsilon) := (1+\varepsilon)\sigma^2(P) + \varepsilon^2(\sigma_A^2(P) + \sigma_B^2(P))/2$, abbreviate $\tilde{\sigma}^2(\theta^*(P), P, \varepsilon)$ by $\tilde{\sigma}^2(P, \varepsilon)$, and $H_k(P) := E_P[\nabla_{\theta_k}^2 \ln f_k(X; \theta_k^*(P))]$ for $k = A, B$.

Definition 2. For some fixed $\delta, \kappa > 0$, $0 < \underline{M} \leq \overline{M} < \infty$, and an increasing, continuous function $\epsilon : (0, \infty) \to (0, \infty)$ with $\epsilon(0) = 0$, let $\mathcal{P}$ be the set of distributions P on $\mathcal{X}$ that satisfy the following conditions for $X \sim P$: (i) There exists a unique $\theta^*(P) \in \Theta$ such that $E_P g(X; \theta^*(P)) = 0$, for all $\mu > 0$, $\inf_{\theta:\|\theta-\theta^*(P)\|\geq\mu} \|E_P g(X;\theta)\| > \epsilon(\mu)$, and $B_\kappa(\theta^*(P)) \subseteq \Theta$, where $B_\kappa(\theta)$ denotes a ball in $\mathbb{R}^{d_\theta}$ with radius κ around θ. (ii) There exists a function $D(x)$ such that $E_P[|D(X)|^{2+\delta}] \leq \overline{M}$ and, for all $x \in \mathcal{X}$,

$$\left|\ln f_A(x; \theta_A^*(P)) - \ln f_B(x; \theta_B^*(P))\right| \leq D(x)\left(E_P\left[\left|\ln f_A(X; \theta_A^*(P)) - \ln f_B(X; \theta_B^*(P))\right|^2\right]\right)^{1/2}, \quad (3)$$

where $\theta^*(P) := (\theta_A^*(P)', \theta_B^*(P)')'$. Further, we have $E_P[|\ln f_k(X; \theta_k^*(P))|^{4+\delta}] \leq \overline{M}$ and, similarly, $E_P[\|\nabla_{\theta_k} \ln f_k(X; \theta_k^*(P))\|^{2+\delta}] \leq \overline{M}$ for $k = A, B$. (iii) There exists a function $\bar{F}(x)$ such that $E_P \bar{F}(X) \leq \overline{M}$ and, for $j, k = A, B$, for all $\theta = (\theta_A', \theta_B')' \in \Theta$, for all $x \in \mathcal{X}$, and for $h(x;\theta)$ being any of the functions $\ln f_k(X;\theta_k)$, $\nabla_{\theta_k} \ln f_k(X;\theta_k)$, $\mathrm{vec}(\nabla_{\theta_k}^2 \ln f_k(x;\theta_k))$ and $\ln f_k(x;\theta_k)\nabla_{\theta_j} \ln f_j(x;\theta_j)$, we have $\|h(x;\theta)\| \leq \bar{F}(x)$. (iv) For $k = A, B$, we have $\underline{M} \leq \lambda_{\min}(H_k(P))$ and $\lambda_{\max}(H_k(P)) \leq \overline{M}$, where $\lambda_{\min}(A)$ and $\lambda_{\max}(A)$, respectively, denote the smallest and largest eigenvalue of a matrix A. Furthermore, for $h(x;\theta)$ being any of the functions $\log f_k(x;\theta_k)$, $(\log f_k(x;\theta_k))^2$, and $\nabla_{\theta_k} \log f_k(x;\theta_k)$, $k = A, B$, $\theta := (\theta_A', \theta_B')'$, we have $\underline{M} \leq \lambda_{\min}(\mathrm{var}(h(X;\theta^*(P))) \leq \lambda_{\max}(\mathrm{var}(h(X;\theta^*(P))) \leq \overline{M}$.

Before stating the uniformity theorem, we slightly modify Assumption 5 to hold under sequences of distributions.

Assumption 6. Let $\hat{\varepsilon}_n$ be a sequence of real-valued, measurable functions of $X_1, \ldots, X_n$ such that, for every sequence $\{P_n\}$ in $\mathcal{P}$, there exists a sequence $\{\varepsilon_n\} \in \mathcal{E}$ with $|\hat{\varepsilon}_n - \varepsilon_n| = O_{P_n}(n^{-1/2})$.

In Section 6, we verify Assumption 6 for our proposed data-driven regularization parameter selection rule.

Theorem 2. Suppose Assumptions 2 and 6 hold. Let $\mathcal{P}_0 := \{P \in \mathcal{P} : d^*(P) = 0\}$ be the subset of distributions in $\mathcal{P}$ that satisfy the null hypothesis. Then the regularized t-test of nominal level α is asymptotically of level α uniformly over $\mathcal{P}_0$, viz.,

$$\lim_{n\to\infty} \sup_{P\in\mathcal{P}_0} P\left(|\tilde{t}_n| > z_{1-\alpha/2}\right) = \alpha.$$

To the best of our knowledge, this uniformity property of our model selection test is the only result of this kind besides that by Shi (2015). If the test was only pointwise of correct asymptotic level, then it could be the case that for any sample size N there exists a sequence of distributions $P_n \in \mathcal{P}_0$ such that for any sample size $n \geq N$ the rejection probability under P_n is arbitrarily close to one. This possibility is ruled out when the test is uniformly of correct asymptotic level, which implies that for any $\epsilon > 0$ there is a sample size N such that, for all $n \geq N$, the rejection probability under any sequence $P_n \in \mathcal{P}_0$ is at most $\alpha + \epsilon$. Uniform control of the level over all distributions in $\mathcal{P}_0$ is both important and often difficult to establish because the distributions in the null hypothesis can be nested, nonnested, or overlapping. In tests such as the Vuong test, for example, these different cases give rise to different limiting distributions of the test statistic so that even, in, say, nonnested models that are "close" to overlapping, substantial finite sample size distortions can occur. The uniformity of the level over $\mathcal{P}_0$ guarantees that such distortions do not occur or, at least, vanish in large samples. In the model selection context, this uniformity property is particularly desirable as it may be difficult to judge a priori whether competing models are "close" to each other. When they are "close," a formal model selection test is arguably the most valuable as the two models may be difficult to distinguish on other, say, theoretical grounds.

Remark 4. Our model selection test avoids pretesting as is necessary in Vuong's two-step procedure and guarantees uniform asymptotic size control as shown in Theorem 2. However, the well-known nonuniform behavior of post-model selection inference persists so that researchers should exercise caution when using the selected model in subsequent estimation and inference steps. In finite samples, some effect of the model selection step cannot be completely excluded (see, e.g., White 2000; Leeb and

Pötscher 2005, 2008, and references therein, for a more detailed discussion). Fortunately, effective methods have been developed to quantify the effect (White 2000).

5.2. Local Power

Theorem 1 shows that the limiting distribution of our test statistic is independent of the regularization parameter $\hat{\varepsilon}_n$. Therefore, our test controls size (by Theorem 2 even uniformly) and is consistent against fixed alternatives, independently of the specific choice of the sequence $\{\hat{\varepsilon}_n\}$. However, as we show in this section, the local asymptotic power of our test depends on the probability limit of $\{\hat{\varepsilon}_n\}$.

We consider local alternatives $\delta \in \mathbb{R}$ so that $n^{1/2}d^*(P_n) \to \delta$. The set $\mathcal{P}_\delta$ contains all sequences of distributions that satisfy the assumptions placed on $\mathcal{P}$ and along which $n^{1/2}d^*(P_n)$ converges to δ.

Definition 3. For some $\delta \in \mathbb{R}$, let $\mathcal{P}_\delta$ be the set of sequences $\{P_n\}$ in $\mathcal{P}$ such that $n^{1/2}d^*(P_n) \to \delta$ and such that, for any $(\theta_{A,\infty}, \theta_{B,\infty}, \sigma_A^2, \sigma_B^2, \sigma_{AB}) \in \Theta_A \times \Theta_B \times \mathbb{R}_+ \times \mathbb{R}_+ \times \mathbb{R}$, $\theta_A^*(P_n) \to \theta_{A,\infty}$, $\theta_B^*(P_n) \to \theta_{B,\infty}$, $\sigma_A^2(P_n) \to \sigma_A^2$, $\sigma_B^2(P_n) \to \sigma_B^2$, and $\sigma_{AB}(P_n) \to \sigma_{AB}$, where $\sigma_A^2(P) := \text{var}_P(\ln f_A(X;\theta_A^*(P)))$, $\sigma_B^2(P) := \text{var}_P(\ln f_B(X;\theta_B^*(P)))$, and $\sigma_{AB}(P) := \text{cov}_P(\ln f_A(X;\theta_A^*(P)), \ln f_B(X;\theta_B^*(P)))$.

Importantly, alternatives in $\mathcal{P}_\delta$ are allowed to approach both, observationally equivalent ($\sigma^2 = 0$) or observationally distinct ($\sigma^2 \neq 0$) data-generating processes, in the null. The following theorem presents the power of our test against all local alternatives in $\mathcal{P}_\delta$.

Theorem 3. Suppose Assumptions 2 and 6 hold. Let $\{P_n\} \in \mathcal{P}_\delta$ for some localization parameter $\delta \in \mathbb{R}$. Denote by $\{\varepsilon_n\} \in \mathcal{E}$ a sequence such that $|\hat{\varepsilon}_n - \varepsilon_n| = O_{P_n}(n^{-1/2})$ and $\varepsilon := plim_{n\to\infty}\hat{\varepsilon}_n$ under P_n. Then, under P_n,

$$\tilde{t}_n \to_d N(\tilde{\lambda}, 1)$$

with mean

$$\tilde{\lambda} := \lim_{n\to\infty} \frac{\sqrt{n}d^*(P_n)(1+\varepsilon_n/2)}{\sqrt{(1+\varepsilon_n)\sigma^2(P_n) + \varepsilon_n^2(\sigma_A^2(P_n)+\sigma_B^2(P_n))/2}},$$

and $\sigma^2(P) = \sigma_A^2(P) - 2\sigma_{AB}(P) + \sigma_B^2(P)$.

Consider sequences $\{P_n\}$ that approach an observationally distinct models case in the null, that is, $\sigma^2(P_n) \to \sigma^2 > 0$. Then the noncentrality parameter becomes

$$\tilde{\lambda} = \frac{\delta(1+\varepsilon/2)}{\sqrt{(1+\varepsilon)\sigma^2 + \varepsilon^2(\sigma_A^2+\sigma_B^2)/2}}. \tag{4}$$

If $\{P_n\}$ approaches an equivalent models case in the null, that is, $\sigma^2(P_n) \to 0$, and $\varepsilon \neq 0$, then

$$\tilde{\lambda} = \frac{\delta(1+\varepsilon/2)}{\varepsilon\sqrt{(\sigma_A^2+\sigma_B^2)/2}}. \tag{5}$$

In the two cases of (4) and (5), $\tilde{\lambda}$ as functions of ε is maximized at $\varepsilon = 0$ or as ε approaches 0, respectively. On the other hand, when models overlap at the truth, we require a nonzero sequence of regularization parameters, possibly converging to zero, to guarantee a nondegenerate limiting distribution of our test statistic. In finite samples, we typically encounter an intermediate case: we would prefer not to regularize ($\hat{\varepsilon}_n = 0$) if we knew that the two candidate models are "sufficiently far apart" from each other, but we would choose a positive regularization parameter when the two candidate models are "close" to overlapping to minimize size distortions. (Notice that Theorem 2 only requires a *positive* value $\hat{\varepsilon}_n$ for uniform size control, but does *not* imply that larger values $\hat{\varepsilon}_n$ lead to "better" size control in any sense.) The next section formalizes the trade-off between power in the distinct models case and size control in the equivalent models case, and shows how this trade-off determines an optimal regularization parameter that can easily be estimated from the data.

6. Data-Driven Regularization Parameter

In this section, we provide a data-driven choice of $\hat{\varepsilon}_n$ that minimizes higher-order distortions to size and power of our test. Specifically, we balance the worse-case size distortion if the models were overlapping with the worst-case power loss if the models were not overlapping. The rationale for proceeding in this way is that, in our approach, size distortion only occurs for overlapping models while power loss only occurs when the models are not overlapping. Furthermore, in a finite sample, it may be difficult to accurately test whether the models are overlapping or not (this is the fundamental pretesting problem we wish to avoid) and hence it is natural to consider both possibilities simultaneously. Such an approach also considerably simplifies the implementation of the method.

In the supplement to this article, we derive an asymptotic expansion of the size of our test when the two models are overlapping, viz., for any distribution P_0 such that $d^*(P_0) = 0$ and $\sigma^2(P_0) = 0$,

$$P_0(|\tilde{t}_n| > z_{1-\alpha/2}) \leq \alpha + C_{SD}\varepsilon_n^{-1}n^{-1/2}\ln\ln n + \text{remainder}, \tag{6}$$

where C_{SD} is some constant. Similarly, we expand the power of our test when the models are nonnested, viz., we show that for sequences of local alternatives $\{P_n\}$ satisfying $d^*(P_n) = \delta n^{-1/2}$ for any given $\delta \in \mathbb{R}\setminus\{0\}$ and $\sigma^2 := \lim_{n\to\infty}\sigma^2(P_n) > 0$,

$$P_n\left(|\tilde{t}_n| > z_{1-\alpha/2}\right) = \Phi\left(z_{\alpha/2} + \frac{\delta}{\sigma}\right) + \Phi\left(z_{\alpha/2} - \frac{\delta}{\sigma}\right) - C_{PL}^*\varepsilon_n^2 + \text{remainder}, \tag{7}$$

where C_{PL}^* is some constant. Size distortion for overlapping models is decreasing in ε_n and power loss for distinct models is increasing in ε_n. Therefore, we propose a tuning parameter ε_n that balances the respective leading terms of the size distortion, that is, the term $C_{SD}\varepsilon_n^{-1}n^{-1/2}\ln\ln n$, and power loss, that is, the term $C_{PL}^*\varepsilon_n^2$. This tuning parameter choice can be estimated by

$$\hat{\varepsilon}_n = \left(\frac{\hat{C}_{SD}}{\hat{C}_{PL}^*}\right)^{1/3} n^{-1/6}(\ln\ln n)^{1/3} \tag{8}$$

with

$$\hat{C}^*_{\mathrm{PL}} := \phi\left(z_{\alpha/2} - \frac{\hat{\delta}^*}{\hat{\sigma}}\right)\frac{\hat{\delta}^*(\hat{\sigma}^2 - 2(\hat{\sigma}_A^2 + \hat{\sigma}_B^2))}{4\hat{\sigma}^3}$$

$$\hat{C}_{\mathrm{SD}} := 2\phi(z_{\alpha/2})\frac{\max\{|\mathrm{tr}(\hat{H}_A^{-1}\hat{V}_A)|, |\mathrm{tr}(\hat{H}_B^{-1}\hat{V}_B)|\}}{\sqrt{(\hat{\sigma}_A^2 + \hat{\sigma}_B^2)/2}}$$

estimating the constants C^*_{PL} and C_{SD}. In the expressions above, $\hat{\delta}^* := \hat{\sigma}/2(z_{\alpha/2} - \sqrt{4 + z^2_{\alpha/2}})$, $\hat{H}_k$ and $\hat{V}_k$, $k = A, B$, are estimates of $H_k := H_k(P_0)$ and $V_k := V_k(P_0)$ with $V_k(P) := E_P\left[\nabla_{\theta_k} \ln f_k\left(X_i, \theta_k^*(P)\right)\left(\nabla_{\theta_k} \ln f_k\left(X_i, \theta_k^*(P)\right)\right)'\right]$, obtained by replacing expectations by sample averages.

The proposed value of $\hat{\varepsilon}_n$ in (8) can easily be computed from the data as it requires only estimates of the matrices H_k and V_k, which have to be computed for the "sandwich" variance estimator for potentially misspecified models anyway, and the sample variances $\hat{\sigma}$, $\hat{\sigma}_A^2$, and $\hat{\sigma}_B^2$.

Remark 5. The tuning parameter $\hat{\varepsilon}_n$ in (8) depends on whether the models overlap or not via the dependence of C^*_{PL} on σ^2 and thus on σ_{AB}. In addition, some model-overlap-dependence is built into the test statistic itself. When the models are far from overlapping, $\hat{\varepsilon}_n$ is the prefactor of a higher-order term of the stochastic expansion of the test statistic. When models approach overlap, the leading term tends to zero and the term of next higher order (with $\hat{\varepsilon}_n$ prefactor) becomes dominant. As mentioned in Section 5.2 it is worth emphasizing that Theorem 2 only requires a *positive* value of $\hat{\varepsilon}_n$ for any fixed n, but does *not* imply that larger values of $\hat{\varepsilon}_n$ lead to "better" size control in any sense.

Remark 6. The choice $\hat{\varepsilon}_n$ in (8) is derived from a particular trade-off between the worst-case size distortion if the models were overlapping with the worst-case power loss if the models were not overlapping. In principle, it would be possible to derive data-driven choices of $\hat{\varepsilon}_n$ using other criteria, such as weighted size distortion and power loss or error in rejection probability (e.g., as in Calonico, Cattaneo, and Farrell 2016). One attractive feature of the trade-off presented here is the simplicity of the resulting choice in (8).

7. Extensions

To simplify the presentation of our basic model selection procedure, we restrict attention to a simple and stylized framework: we compare two fully specified parametric models based on the KL criterion, iid data and a t-statistic. In the supplement, we argue that our procedure applies much more generally and discuss some important, but mostly straightforward, extensions. First, one could use our test based on goodness-of-fit criteria other than KL distance. An important example would be comparing the accuracy of competing forecasts by Diebold and Mariano (1995). Second, the limiting distribution of our test statistic requires only asymptotic normality of certain sample averages, so extensions to stationary data are straightforward. Third, instead of Z-estimators one could readily extend our test statistic to the comparison of models defined by moment conditions that can be estimated by GMM. Fourth, we could use our test to rank more than two models by incorporating it into a multiple testing framework in the usual way (e.g., Lehmann and Romano 2005; Romano, Shaikh, and Wolf 2010). To see this, notice that our test for the comparison of two models is simply a t-test for whether a mean, that is, the KL discrepancy between the two models, is equal to zero or not. Ranking several models therefore requires testing whether multiple means, that is, the KL discrepancies between all possible pairs of models, are equal to zero or not. A simple procedure that accounts for the multiplicity of hypotheses by, say, controlling the family-wise error rate, is based on individual t-tests with adjusted critical values. Examples of adjustments are Bonferroni's and Holm (1979)'s procedures, but more sophisticated step-up or step-down procedures could be used. See, for instance, Lehmann and Romano (2005) and Romano, Shaikh, and Wolf (2010) for more details.

The idea of altering a test statistic so that it preserves a normal distribution in all cases can be exploited in other contexts. In fact, since this article was first circulated, Hsu and Shi (2013) have considered the selection among conditional moment inequality models and argued that an effect similar to sample splitting can be accomplished by adding a generated independent normal noise to a nonnormal statistic, to obtain a test statistic that is always normally distributed.

8. Simulations

This section reports Monte Carlo simulation results for two pairs of models (additional models are considered in the supplementary materials).

All simulations are based on 1000 Monte Carlo samples. Our test based on the regularized statistic $\tilde{t}_n$ is compared to the two-step Vuong procedure (see p. 321 in Vuong 1989) and to Shi's (2015) modified Vuong test. (Shi (2015) also compared her test to ours but did not use the optimal regularization parameter selection rule described in the present version of the article.) We consider our test statistic for various choices of the regularization parameter: $\varepsilon_n = 0$ ("no reg"), $\varepsilon_n = 0.5$, $\varepsilon_n = 1$, and the optimal $\hat{\varepsilon}_n$ as defined in (8). The two-step Vuong procedure for a level-α test is implemented by setting the level equal to α in both individual steps.

Example 1 (Joint Normal Location). This example is similar to one by Shi's (2015) who constructed it to illustrate the potentially poor power of Vuong's test. We let $P_0 := N((0, \mu), (25, 1)I)$ where I is the identity matrix, $\mathcal{P}_A := \{N((\mu_A, 0), I) : \mu_A \in \Theta_A\}$, and $\mathcal{P}_B := \{N((0, \mu_B), I) : \mu_B \in \Theta_B\}$. The null and alternative models are generated by varying μ in $[0, 2.5]$. $\mu = 0$ corresponds to the null hypothesis ($d = 0$) and values in $(0, 2.5]$ to alternatives $d = \mu^2/2$. Notice that the two models are observationally equivalent under the null, but misspecified.

Example 2 (Nonnested Regressions). This example is similar to one by Shi's (2015) who constructed it to illustrate the potentially poor size control of Vuong's test. Let the random vector $(Y_i, W_{i1}, \ldots, W_{i10})$, $i = 1, \ldots, n$, satisfy the regression equation $Y_i = 1 + \frac{\tau}{\sqrt{9}}\sum_{k=1}^{9} W_{ik} + \tau W_{i10} + \varepsilon_i$, with $\varepsilon_i \sim N(0, 2^2)$ and $(W_{i1}, \ldots, W_{i14}) \sim N(0, I)$. Consider model A, $Y_i = \alpha_0 + \sum_{k=1}^{9} \alpha_k W_{ik} + \varepsilon_i$ with $\varepsilon_i \sim N(0, \sigma_A^2)$, and model B, $Y_i = \beta_0 + \beta_1 W_{i10} + \varepsilon_i$ with $\varepsilon_i \sim N(0, \sigma_B^2)$. For any value of $\tau \neq 0$, the two

Table 1. Null rejection probabilities (nominal size 0.05) for Example 1.

	Our test					
n	No reg	$\varepsilon_n = 0.5$	$\varepsilon_n = 1$	Optimal	Vuong	Shi
100	0.000	0.041	0.045	0.037	0.000	0.000
200	0.000	0.046	0.045	0.039	0.000	0.000
500	0.000	0.039	0.037	0.038	0.000	0.000

models have the same distance to the true model, but are both misspecified. We vary τ in $[0, 2]$.

In Example 1, we estimate means and variances with the sample means and variances and, in Example 2, we estimate the regressions by ordinary least squares. Notice that these estimators are just the maximum-likelihood estimators in the particular models considered here. In both examples, it is straightforward to verify the assumptions of our theoretical results in the preceding sections.

Table 1 reports the finite sample size of the different tests. In Example 2, we consider a family of null hypotheses whereas, in Example 1, we study the properties of our test as the true distance $|d^*|$ increases from zero (the null hypothesis) to a range of positive values (alternatives). Figure 1 shows the power curves for Example 1 in panels (a)–(c) and the null rejection probabilities for Example 2 in panel (d). In both examples, we report results for 5%-level tests. In addition, we also show power results at the 1% level in Example 1. The black horizontal lines in the power and size graphs mark the level of the tests. "no reg," "$\hat{\varepsilon}_n = 0.5$," "$\hat{\varepsilon}_n = 1$," and "optimal" refer to our test using $\hat{\varepsilon}_n = 0$, $\hat{\varepsilon}_n = 0.5$, $\hat{\varepsilon}_n = 1$, and the optimal epsilon defined in (8), respectively.

The two main findings from this simulation experiment can be summarized as follows. (i) In Table 1 and Figure 1(d), we see that all three tests control size well with our test having size very close to nominal size in most examples. Vuong's and Shi's test, on the other hand, more frequently have size well below nominal size. (ii) Our new test and Shi's test can have significantly higher power than Vuong's test. Since our test has size closer to nominal size than Shi's, ours possesses more power to detect alternatives close to the null, that is, models that are difficult to distinguish.

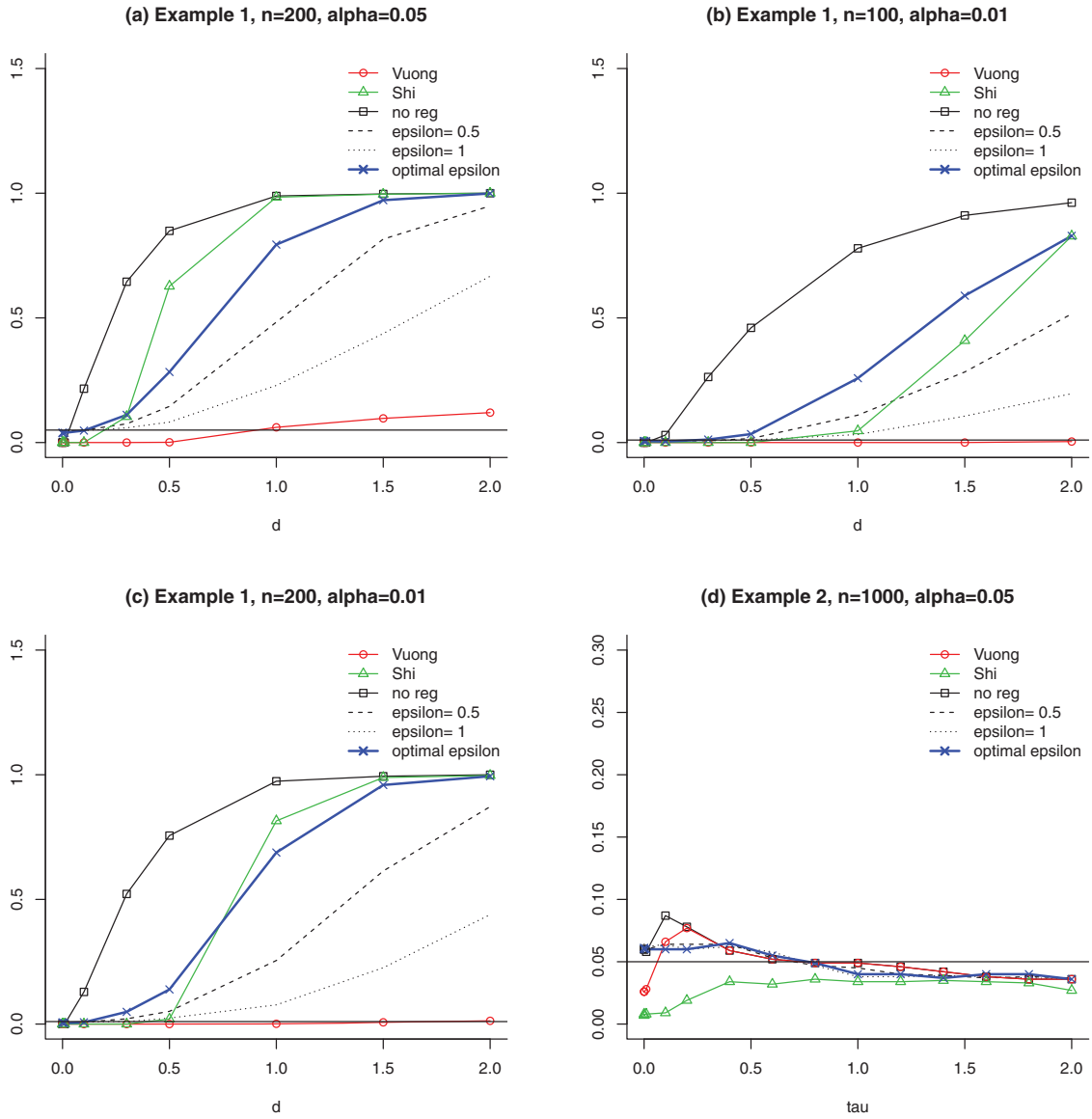

Figure 1. Comparison of the rejection frequencies of the different tests considered. For Example 1, panels (a)–(c) report power curves for different confidence levels α and sample sizes n as function of the alternative model, indexed by d. For Example 2, panel (d) reports the actual size for a family of model pairs (indexed by τ) satisfying the null hypothesis. On all graphs, the nominal level is marked by a black horizontal line.

For alternatives further away from the null, neither test seems to dominate the other.

These simulations suggest that our test performs well in practice, with performance comparable and sometimes superior to existing methods. These results are especially encouraging in light of our method's conveniently straightforward implementation.

9. Empirical Application

A major part of the classic debate over (New) Keynesian versus (new) classical macroeconomic theory has focused on whether government policies, monetary or fiscal, can have any systematic impact on outcomes such as output or unemployment (Dadkhah (2009) gave a nice general overview of the literature and how it has evolved more recently). Under the new classical hypothesis of rational expectations ("RE") and natural rate of unemployment ("NR"), it has been shown (Sargent and Wallace 1975) that, under certain assumptions, there is no such effect. Consequently, a lot of effort has been devoted to testing the joint NR/RE hypothesis. In an influential article, Barro (1977) proposed such a test based on a two equation system, one for money growth ($\mathrm{DM_t}$),

$$\mathrm{DM_t} = \mathrm{Z_t'}\theta_1 + \varepsilon_{1t} \tag{9}$$

and one for unemployment ($\mathrm{UN_t}$),

$$\mathrm{UN_t} = \mathrm{X_t'}\theta_2 + \varepsilon_{2t}, \tag{10}$$

where X_t and Z_t are exogenous explanatory variables known at time $t-1$. Specifically, he suggested the covariates $Z_t := (1, \mathrm{DM_{t-1}}, \mathrm{DM_{t-2}}, \mathrm{FEDV_t}, \mathrm{UN_{t-1}})$ and $X_t := (1, \mathrm{DMR_t}, \mathrm{DMR_{t-1}}, \mathrm{DMR_{t-2}}, \mathrm{MIL_t}, \mathrm{MINW_t})$ with $\mathrm{FEDV_t}$ a measure of federal government expenditure, $\mathrm{DMR_t} := \varepsilon_{1t}$ the unanticipated part of $\mathrm{DM_t}$, $\mathrm{MIL_t}$ a measure of military conscription and $\mathrm{MINW_t}$ a minimum wage variable. (For exact definitions of the variables involved, see Barro (1977). He also studied output, but we confine our discussion here to unemployment as the outcome of interest.) The NR/RE hypothesis implies that unemployment deviates from its so-called natural level (here proxied by $\mathrm{MIL_t}$ and $\mathrm{MINW_t}$) only due to unanticipated changes in money growth ($\mathrm{DMR_t}$, $\mathrm{DMR_{t-1}}$, $\mathrm{DMR_{t-2}}$). Therefore, Equation (10) fitting the data well Barro interpreted as evidence supporting the NR/RE hypothesis.

Pesaran (1982) criticized this approach arguing that failing to reject the NR/RE hypothesis in a particular model is necessary, but not sufficient for failing to reject it against rival hypotheses. Therefore, he proposed to test it against "proper" or "genuine" alternatives, in particular against three different models with Keynesian features that satisfy (9) and (10) with the following set of covariates:

$$K1: \quad X_t := (1, \mathrm{DM_t}, \mathrm{DM_{t-1}}, \mathrm{DG_t}, \mathrm{MIL_t}, \mathrm{MINW_t}, \mathrm{t}),$$
$$K2: \quad X_t := (1, \mathrm{DM_t}, \mathrm{DM_{t-1}}, \mathrm{DM_{t-2}}, \mathrm{DG_t}, \mathrm{MIL_t}, \mathrm{MINW_t}, \mathrm{t}),$$
$$K3: \quad X_t := (1, \mathrm{DM_t}, \mathrm{DM_{t-1}}, \mathrm{DMR_t}, \mathrm{DG_t}, \mathrm{MIL_t}, \mathrm{MINW_t}, \mathrm{t}),$$

where $\mathrm{DG_t}$ is a measure of government spending. Subsequently, we test each of these models against Barro's new classical model and a slight variant with a time trend in the unemployment equation:

$$B1: \quad X_t := (1, \mathrm{DMR_t}, \mathrm{DMR_{t-1}}, \mathrm{DMR_{t-2}}, \mathrm{MIL_t}, \mathrm{MINW_t}),$$
$$B2: \quad X_t := (1, \mathrm{DMR_t}, \mathrm{DMR_{t-1}}, \mathrm{DMR_{t-2}}, \mathrm{MIL_t}, \mathrm{MINW_t}, \mathrm{t}).$$

We refer the reader to Pesaran (1982) for specifics about these five models and their theoretical foundations.

Based on Barro's (1977) annual data from 1946 to 1973, we estimate each of the models in two different ways. First, we estimate both Equations (9) and (10) jointly by full-information maximum likelihood (FIML) assuming that the errors in the two equations are jointly normal. Second, we estimated only the unemployment Equation (10) by maximum likelihood, again assuming normality of the errors and taking the estimated series $\{\mathrm{DMR_t}\}$ from Barro (1977) as given.

Table 2. Value of our regularized model selection test statistic $\tilde{t}_n$ based on the optimal $\hat{\varepsilon}_n$.

		K1	K2	K3
Both equations	B1	− 0.136	− 0.664	− 0.126
	B2	0.767	0.186	0.775
Only unemployment equation	B1	− 0.527	− 1.070	− 0.507
	B2	0.390	− 0.247	0.408

The results of the pairwise model selection tests of new classical models versus Keynesian models are reported in Table 2 and are based on the estimated optimal epsilon-parameters that ranged from 1.1 to 1.4 across the 12 pairs of models. As a sensitivity analysis we also performed our test for epsilon values in a range from 0.1 to 2.0 but the conclusions derived from the optimal epsilon do not change. When we compare Keynesian and new classical models based only on the unemployment equation, all three tests fail to reject the hypothesis that the models are equally distant from the truth. Even adding the money growth equation does not lead to rejections. The sign of our test static suggests that the Keynesian models are closer to the truth than the new classical model B1, but further away from the truth than B2. However, none of these statements is statistically significant at reasonable levels of confidence. Since, in the simulations, our new test tends to reject at a higher rate, both, under the null and under alternatives, with significantly higher power in some scenarios, the fact that our test fails to reject in all 12 model comparisons strengthens the findings of the Vuong test, which we found to also fail to reject in all 12 comparisons. The Vuong test's failure to distinguish the two theories is therefore less likely to be due to it under-rejecting under the null or to its potentially low power. In conclusion, we interpret the findings as there not being enough information in the present dataset to discriminate between the candidate new classical and Keynesian models. A larger sample or imposing more structure on the models might lead to different conclusions.

There are some interesting differences in these findings compared to the results reported in Pesaran (1982). He compared models based only on the unemployment equation employing an F-test as well as a Cox-type test for nonnested models. In the latter testing procedure, the null hypothesis is that model A is the true data-generating process to be tested against the alternative that model B is the truth. In terms of the F-test, no model in $\{B1, B2\}$ is found to be superior to any model in $\{K1, K2, K3\}$. His application of the Cox-type test, however, resulted in

any model in $\{B1, B2\}$ being rejected against any alternative in $\{K1, K2, K3\}$ and vice versa. The testing outcomes of the Cox-type procedure are not possible in our test because both models are treated symmetrically: As soon as our test rejects equivalence between any two models, the one with the smaller KL distance to the truth is concluded superior to the other. Even though the null hypothesis in our test does not assume correct specification of any model, we still do not reject any model combination. Small (1979) and Pesaran (1982) criticized Barro's specification of the model and argued that the estimates of the unemployment equation may be sensitive to variations in the specification of the money growth equation. Our test results show that, at least based on the present dataset, the inclusion the money growth equation has no implications on whether the new classical or the Keynesian theory is superior to the other.

Supplementary Materials

This supplement provides the proofs of all results in the main text, additional results referenced in the main text, and additional simulations.

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