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ALTERNATIVE NONNESTED SPECIFICATION TESTS OF TIME SERIES INVESTMENT MODELS

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ABSTRACT

This paper develops and compares nonnested hypothesis tests for linear regression models with first-order serially correlated errors. It extends the nonnested testing procedures of Pesaran, Fisher and McAleer, and Davidson and MacKinnon, and compares their performance on four conventional models of aggregate investment demand using quarterly U.S. investment data from 1951:I to 1983:IV. The data and the nonnested hypothesis tests initially indicate that no model is correctly specified, and that the tests are occasionally intransitive in their assessments. Before rejecting these conventional models of investment demand, we go on to investigate the small sample properties of these different nonnested test procedures through a series of monte carlo studies. These investigations demonstrate that when there is significant serial correlation, there are systematic finite sample biases in the nominal size and power of these test statistics. The direction of the bias is toward rejection of the null model, although it varies considerably by the type of test and estimation technique. After revising our critical levels for this finite sample bias, we conclude that the accelerator model of equipment investment cannot be rejected by any of the other alternatives.

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1. Introduction

In applied econometric work it is not uncommon to have several substantially different models that can claim success as empirical explanations of a particular economic phenomenon. A leading example, if not *the* leading example of this, is the assortment of models that explain the time series demand for business fixed investment (hereafter, simply "investment"). Although there may be no "true" investment specification that explains actual investment patterns, it can be agreed that the absence of a consensus empirical model not only inhibits investment policy formulation and complicates investment forecasts, but also adversely affects the usefulness of theoretical and empirical macroeconomic models whose interpretations hinge on the precise form of the equation(s) explaining net capital formation (cf. Feldstein (1982)).

Recent dramatic swings in aggregate investment expenditures, and in the underlying microeconomic and macroeconomic conditions that determine the rate of investment, have provided empirical investigators with a unique opportunity to test and cross-validate conventional models of investment. Several recent studies (e.g. Kopcke (1977) and Clark (1979)) have developed rankings of investment models using goodness-of-fit or statistical prediction error criteria similar to those used in earlier comparisons of investment models.[1] This work has generally concluded that there is modest support for investment models that use output or sales variables, and that there is little support for models that employ the user cost of capital or q. Few of these studies have, however, provided formal justifications or motivations as to why goodness-of-fit or forecast errors are appropriate comparative criteria. Further, the small sample size and power properties of these ranking procedures are rarely, if ever, considered.

Several recent refinements in the theory of nonnested specification tests suggest that in addition to or in place of the discrimination criteria used before, specifications should be compared on the criterion of whether they can show that other candidate models are misspecified (see Pagan (1981) and the excellent review by McAleer (1984)). Although

[1] Early comparative studies of investment include Kuh (1963), Jorgenson and Siebert (1968), Jorgenson, Hunter, and Nadiri (1970a, 1970b), and Bischoff (1971a).

several studies have used these tests to compare different macroeconomic time series models, there are at least three reasons why these nonnested testing principles have not been more widely applied to macroeconomic time series such as investment.[2] First, nonnested tests have not been developed for general time series models that possess mixtures of serial correlation, lagged dependent variables, and endogenous variables.[3] Second, there is some evidence that the asymptotic critical values of existing nonnested test procedures tend to reject the null hypothesis too often in finite samples.[4] Third, in more complicated practical applications, especially when one wishes to implement nonnested tests based upon maximum likelihood techniques, these tests can be quite burdensome computationally.

This paper explores the usefulness of nonnested testing procedures for evaluating linear regression models when the disturbances of the candidate models are first-order serially correlated. Specifically, we examine the usefulness of nonnested tests as a means for detecting whether serial correlation in time series residuals reflects specification error. The paper begins by extending and discussing existing nonnested test criteria of Pesaran (1974), Fisher and McAleer (1981), and Davidson and MacKinnon (1981) to situations involving first-order serially correlated errors. A number of asymptotically equivalent nonnested test statistics are derived, and the tradeoff between their respective computational complexities and their finite sample properties is evaluated. We then compare the finite sample performance of these alternative tests on four conventional nonnested models of macroeconomic investment demand. The data we use are quarterly U.S. business investment for structures and equipment from 1951:I to 1983:IV. We initially find that none of the traditional models is satisfactory. Before concluding that these conventional models of investment demand are inadequate, we investigate the hypothesis that there is a finite sample bias in the size of the test. After adjusting for finite sample biases, we find that we are able to accept the

[2] Two notable applications of nonnested tests in the investment literature are Bean (1981) and Poterba and Summers (1984). These two studies, however, use nonnested specification test principles developed for the classical linear model with spherical normal disturbances.

[3] For exceptions see Pesaran (1974), MacKinnon, White and Davidson (1983), and Ericsson (1983).

[4] This tendency was noted by Pesaran (1974) for small sample sizes and has been confirmed by other investigators for different statistics and different problems (e.g. Ericsson (1983)) .

equipment accelerator model. We conclude by discussing different conditions under which the significance levels of these different statistics are likely to be overstated.

2. Nonnested Testing Procedures with Serially Correlated Errors

Pesaran (1974), MacKinnon (1983), McAleer (1984), and the *Journal of Econometrics* special issue (1983) on nonnested hypothesis tests provide excellent introductions to the theory and application of nonnested tests. Currently, applied investigators can choose among several different nonnested procedures. These procedures differ not only in their computational requirements, but also in their generality and the statistical assumptions they require. A number of monte carlo studies have also shown that there are important computational and statistical tradeoffs among these alternative test statistics (cf. Pesaran and Deaton (1978), Davidson and MacKinnon (1981), and Pesaran and Godfrey (1983)).[5] Moreover, a fundamental complication that arises in attempts to evaluate these different nonnested tests is the multiplicity of ways in which each of these different approaches can be implemented. This multiplicity occurs both because practically there are many asymptotically equivalent ways of writing down any one nonnested hypothesis test and because of conceptual ambiguity in defining the family of alternative models.[6] At present, there appears to be little consensus on whether any one approach is generally more preferred than another.

One practical situation where specification tests would appear invaluable occurs when a time series model exhibits highly serially correlated disturbances.[7] For example, Granger

[5] Most monte carlo studies of the finite sample performance of these tests are based on bivariate regression models. There have been relatively few practical applications, and to our knowledge no published applications, of these tests that have used monte carlo or bootstrap techniques to evaluate the performance on actual data. Indeed, these tests are routinely applied and evaluated on the basis of asymptotic critical values despite warnings that may or may not have been adjusted for known biases in the nominal finite sample size of the test (see especially the warnings of Davidson and MacKinnon (1981), Godfrey and Pesaran (1982), and Pesaran (1982)). Finally, the power of these tests against particular alternative specifications rarely receives consideration.

[6] Pesaran (1982) examines these issues in comparing the approaches of Fisher and McAleer (1981), Davidson and MacKinnon (1981), and others. See also the discussion in McAleer (1984).

[7] Except perhaps if the investigator has a theory that predicts that the disturbances are

and Newbold (1974) argue that serial correlation in the residuals of time series models often reflects specification errors due to omitted or incorrectly included variables. Most econometrics texts take a similar, albeit more prescriptive position on serial correlation.[8] After warning that serial correlation may reflect different forms of specification error in time series models, they typically develop efficient generalized least square formulae for linear models with serially correlated random errors. In practice, applied investigators have used these generalized least squares methods because it is known that serial correlation of random disturbances will bias the ordinary least squares estimates of the coefficient standard errors. However, for most applications, this statistical response of estimating a serial correlation parameter is at odds with the prior acknowledgment that the *estimated* disturbances may contain omitted variables. Further, this response is even less appropriate if the investigator has several alternative models under consideration, as all but one (or all) of the models are known to be misspecified.

One response to the presence of serial correlation among several nonnested models might be to use conventional F or chi-square tests of exclusion restrictions to see if variables from other models might belong in a particular specification. Although such tests are feasible as long as the explanatory variables are not perfectly collinear, it is unclear whether these tests have high power in applications where the candidate variables are highly correlated.[9] Apart from these computational and power considerations, it is also important to note that a crucial feature of these conventional tests is that they are only a *partial* check for misspecification. To see why, note that the above tests are partial (or uni-directional)

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serially correlated. For example, certain transformations of an original model (such as the Koyck transformation) may produce serial correlation.

[8] For instance, Johnston (1972) in his econometrics textbook states

> In general, we include only certain important variables in the specified relation, and the disturbance term must then represent the represent the influence of omitted variables ... If the serial correlation in the omitted variables is pervasive and if the omitted variables tend to move in phase, then there is a real possibility of an autocorrelated error term. A disturbance term may also contain a component due to measurement error in the 'explained' variable. This too may be the source of serial correlation in the composite disturbance. [page 244]

[9] For some comparative evidence on this issue see Pesaran (1974) and Ericsson (1982).

tests in that they typically test whether the serial correlation in the null model contains a specification error that is related to variables in the alternative models.[10] That is, these tests only ask whether variables in the other models are capable of rationalizing the null model's serially correlated disturbances as misspecification errors. However, it is important to recognize that if the null model is the true model, then it should also be able to explain the serial correlation present in the residuals of the candidate models. Thus, for example if we made the null model (which is true) the alternative model, and made one of the false alternatives the null hypothesis, we should be able to conclude the serial correlation in this new null reflects omitted variables contained in the (true) alternative model. In short, if one model is true, then we ought to be able to rationalize the serial correlation in the other models as omitted variable specification error.

Most nonnested test procedures are based upon this practice of interchanging the null and alternative models as a check for specification error (cf. McAleer (1984)). The application of this interchanging principle would seem therefore to have a straightforward interpretation for macroeconomic time series models with serially correlated disturbances, apart from the need to work out the specific form of the test. There is, however, one potential complication that is created by the use of generalized least squares techniques to estimate and then separately compare both the null and alternative time series models. Use of generalized least squares on a false model (by simply 'tacking on' a serial correlation process for the disturbances) may remove some of the specification error serial correlation that one is attempting to diagnose with these tests. If this "masking" can occur, then it could be extremely difficult to uncover misspecification errors when generalized least squares techniques are used to estimate the candidate models. We shall return to this point below in the analysis of the investment models. First, however, we shall develop several nonnested testing procedures for time series models with first-order serially correlated errors.

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[10] There are several possible interpretations of why all the models under consideration could have serially correlated errors. First, there may be one model that is correct and has "true" serial correlation. The serial correlation in the other models could then then be attributed to be a combination of the original serial correlation and serial correlation of the specification error. On the other hand, the serial correlation may be evidence that all models are misspecified and that none of the alternatives is correct.

2.1 The Cox-Pesaran Statistic

To date, only Pesaran (1974) has explicitly treated serial correlation in the context of nonnested hypothesis tests.[11] Pesaran's approach assumes that ***both*** models are linear and have first-order serially correlated errors of the form

$$\begin{aligned} H_o: &\quad y = X\beta + u_o \qquad u_{ot} = \rho_o u_{ot-1} + v_{ot} \\ H_1: &\quad y = Z\gamma + u_1 \qquad u_{1t} = \rho_1 u_{1t-1} + v_{1t}. \end{aligned} \tag{1}$$

The regressors X and Z are matrices of known constants and it is assumed that the disturbances, u_o and u_1, are normally distributed with mean zero and variance-covariance matrices

$$E(u_o u_o') = \frac{\sigma_o^2}{1-\rho_o^2}\begin{bmatrix} 1 & \rho_o & \rho_o^2 & \cdots & \rho_o^{T-1} \\ \rho_o & 1 & \rho_o & \cdots & \rho_o^{T-2} \\ \vdots & \vdots & \vdots & \ddots & \vdots \\ \rho_o^{T-1} & \rho_o^{T-2} & \cdots & \cdots & \end{bmatrix} = \sigma_o^2 R_o^{-1} = \Omega_o$$

$$E(u_1 u_1') = \frac{\sigma_1^2}{1-\rho_1^2}\begin{bmatrix} 1 & \rho_1 & \rho_1^2 & \cdots & \rho_1^{T-1} \\ \rho_1 & 1 & \rho_1 & \cdots & \rho_1^{T-2} \\ \vdots & \vdots & \vdots & \ddots & \vdots \\ \rho_1^{T-1} & \rho_1^{T-2} & \cdots & \cdots & \end{bmatrix} = \sigma_1^2 R_1^{-1} = \Omega_1. \tag{2}$$

From this framework, Pesaran derived Cox's nonnested likelihood ratio statistic (denoted here by $C0$) that tests whether H_o is the correct model. Its form is (his equations (S.4) and (S.21))

$$C1 = \{C0\}\ (\text{Var } C0)^{-\frac{1}{2}} = \left\{\frac{T}{2} ln\frac{\sigma_1^2}{\sigma_{10}^2} + \frac{1}{2} ln\frac{1-\rho_o^2}{1-\rho_1^2}\right\} \left(\frac{\sigma_o}{\sigma_{10}^2}(\epsilon' D\epsilon)^{\frac{1}{2}}\right)^{-1}. \tag{3}$$

where $\epsilon = X\beta - Z\gamma = (y - Z\gamma) - (y - X\beta) = u_1 - u_o$, σ_i^2 is the variance of v_{it} (i = 0, 1), σ_{10}^2 is the probability limit of σ_1^2 under the assumption H_o is true, and $D = R_{10}[R_o^{-1} - X(X'R_oX)^{-1}X']R_{10}$. This statistic is asymptotically distributed as a standard normal random variable if the null hypothesis is true (see Pesaran (1974)).

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[11] Walker (1967) has also constructed Cox tests for moving average and autoregressive time series models.

From a practical standpoint, this statistic is cumbersome computationally, because it can be implemented only after a system of nonlinear equations has been solved.[12] This computational drawback appears to account for its not being used. At present, we also do not have any information on the finite sample properties of this test. It is possible, however, to simplify Pesaran's statistic by examining its asymptotic properties.

2.2 Simplifications of the Cox-Pesaran Statistic

To simplify C1, note that the asymptotic distribution of this statistic depends only upon the first term (as can be seen by appropriately standardizing $C0$). Appendix B shows that by taking a Taylor series expansion of the first term about zero, one can obtain the following two asymptotically equivalent statistics:

$$\text{C2} = \frac{T}{2}\left(\frac{\sigma_1^2 - \sigma_{10}^2}{\sigma_o(\epsilon' D\epsilon)^{\frac{1}{2}}}\right), \tag{4}$$

and

$$\text{C3} = \left(\frac{\epsilon' R_{10} u_o}{\sigma_0(\epsilon' D\epsilon)^{\frac{1}{2}}}\right). \tag{5}$$

The first statistic is comparable to the computationally simple Cox likelihood ratio statistic discussed in Atkinson (1970), Pesaran and Deaton (1978), and Fisher and McAleer (1981). In this particular application, however, it is not that much more computationally convenient because it still requires the solution of the nonlinear maximum likelihood estimating equations. The second asymptotic form of the Cox Neyman-Pearson test does not look that much simpler, yet it actually is related to another nonnested testing procedure, the P-test proposed by Davidson and MacKinnon (1981).

2.3 Comparisons to the Davidson and MacKinnon Tests

The comparability to the Davidson and MacKinnon P-test can be seen by considering an auxiliary regression of the form:

$$y - X\hat{\beta} = X\pi + \hat{R}_o^{-1}\hat{R}_1(Z\hat{\gamma} - X\hat{\beta})\lambda + \eta, \tag{6}$$

[12] Another potential problem is that it has not been shown that these equations have a unique solution. Further, there is no guarantee that in small samples successive substitution methods will necessarily find a fixed point that corresponds to the maximum of the likelihood function.

or in more suggestive notation,

$$\hat{u}_o = X\pi - \hat{R}_o^{-1}\hat{R}_1\hat{\epsilon}\lambda + \eta \tag{7}$$

where the "hats" ($\hat{}$'s) denote maximum likelihood estimators. Applying generalized least squares to this equation assuming the null hypothesis applies and $E(\eta\eta') = E(u_o u_o') = \Omega_o$, yields an estimate of λ

$$\begin{aligned} \hat{\lambda} &= -(\hat{\epsilon}'\hat{R}_1(\hat{R}_o^{-1} - X(X'\hat{R}_oX)^{-1}X')\hat{R}_1\hat{\epsilon})^{-1}\hat{\epsilon}'\hat{R}_1(I - X(X'\hat{R}_oX)^{-1}X'\hat{R}_o)\hat{u}_o \\ &= (\hat{\epsilon}'\hat{D}\hat{\epsilon})^{-1}\hat{\epsilon}\hat{R}_1\hat{u}_o \end{aligned} \tag{8}$$

where $\hat{D} = \hat{R}_1[\hat{R}_o^{-1} - X(X'\hat{R}_oX)^{-1}X']\hat{R}_1$. This estimate of λ can be thought of as an auxiliary mixing parameter that measures departures from the null model. To test the null hypothesis that H_o is the true model, we test the null hypothesis that this auxiliary parameter is zero. This test can be carried out in a t-ratio form once we have an expression for the asymptotic standard deviation of $\hat{\lambda}$. It can be shown that if the errors satisfy regularity conditions given in White (1984, Chapter 1), then a consistent estimator of the asymptotic variance of $\hat{\lambda}$ is

$$\text{Asy Var }(\hat{\lambda}) = \sigma_o^2(\hat{\epsilon}'\hat{D}\hat{\epsilon})^{-1}. \tag{9}$$

Computing the t-ratio for λ we obtain

$$\text{P1} = -\left(\frac{\hat{\epsilon}'\hat{R}_1\hat{u}_o}{\hat{\sigma}_0(\hat{\epsilon}'\hat{D}\hat{\epsilon})^{\frac{1}{2}}}\right). \tag{10}$$

That is, the Davidson and MacKinnon P-test regression given by equation (6) yields a t-statistic that is asymptotically equivalent to C3. (Note, however, that maximum likelihood estimators have been substituted for the unknowns and the sign of the statistic is reversed.)

The derivation of the P-test regression illustrates that in practice there are many estimators of the alternative parameters that could be used to construct nonnested tests. For example, for $\hat{\epsilon}$ we could use either $X\hat{\beta} - Z\hat{\gamma}$, which is the form recommended by Davidson and MacKinnon (1981), or $X\hat{\beta} - Z\hat{\gamma}_{10}$, which is the form recommended by Fisher and McAleer (1981) (cf. Pesaran (1982) for a related discussion of these different forms of the test).[13] The first of these tests we have labelled P1. The second, with $\hat{R}_{10}$ substituted in place of $\hat{R}_1$, will be referred to as P2. The choice of P1 or P2 for any particular application should be guided by some consideration of their finite sample properties and computational difficulties. On the former issue we have no prior information. In general, P1 will be easier to implement because it does not require R_{10} or γ_{10}. As equation (10) stands, however, there is not that much of a computational savings because of the complexity of the auxiliary matrices $R_o^{-1}R_1$ and $R_o^{-1}R_{10}$.

2.4 Comparisons to Cox's Exponentially-Mixed Likelihood Function Test

As an alternative approach to generating a simple nonnested test for this particular application, we can adopt Cox's exponential mixing of likelihood functions principle (see also Pesaran (1982) and Davidson and MacKinnon (1983)).[14] Following Pesaran (1982), we can create the combined likelihood function

$$L(\beta, \gamma, \rho_o, \rho_1, \lambda) = \frac{L_o^{1-\lambda} L_1^{\lambda}}{\int_{R_y} L_o^{1-\lambda} L_1^{\lambda} \, dy} \tag{11}$$

where L_o and L_1 are the likelihood functions of u_o and u_1 under the two respective alternatives, H_o and H_1, R_y is the T-dimensional domain of y, and λ is a mixing parameter. Under multivariate normality on both errors, this composite likelihood function is

$$L(\beta, \gamma, \rho_o, \rho_1, \lambda) = 2\pi^{-\frac{T}{2}} |\Omega|^{-\frac{1}{2}} \exp \left\{ -\frac{1}{2} u'\Omega^{-1} u \right\} \tag{12}$$

where

$$u = y \; - \; (1-\lambda)\, \Omega\Omega_o^{-1}\, X\beta \; - \; \lambda\Omega\Omega_1^{-1}\, Z\gamma \tag{13}$$

and

$$\Omega^{-1} = (1-\lambda)\frac{R_o}{\sigma_0^2} + \lambda\frac{R_1}{\sigma_1^2}. \tag{14}$$

That is, the exponential mixing approach leads to a regression model

$$y = (1-\lambda)\Omega\Omega_o^{-1}\, X\beta + \lambda\Omega\Omega_1^{-1}\, Z\gamma + u \tag{15}$$

[13] Here, the estimators γ_{10} and R_{10} are the maximum likelihood estimators of γ and R assuming the null hypothesis is correct.

[14] For a discussion of this mixing approach and alternative approaches to mixing the likelihood functions see Quandt (1974).

where the error structure depends on λ.

An obvious problem with trying to estimate directly the regression equation (15) is that not all parameters are identified by equation (15) alone.[15] In particular, it is crucial to note that the mixing parameter λ cannot be separately identified from the variances of the v_{it}. To see this, note

$$(1-\lambda)\Omega\Omega_0^{-1} = I - \lambda\Omega\Omega_1^{-1}, \tag{16}$$

where I is a TxT identity matrix. Substituting equation (16) into equation (15) gives

$$y = X\beta - \lambda\Omega\Omega_1^{-1}\,\epsilon + u. \tag{17}$$

Writing out $\lambda\Omega\Omega_1^{-1}$,

$$\lambda\Omega\Omega_1^{-1} = \left(I + \frac{(1-\lambda)\sigma_1^2}{\lambda\sigma_0^2} R_1^{-1} R_0\right)^{-1} \tag{18}$$

or

$$\lambda\Omega\Omega_1^{-1} = \left(I + \frac{1-\xi}{\xi} R_1^{-1} R_0\right)^{-1} \tag{19}$$

where

$$\xi = \frac{\lambda\sigma_0^2}{(\lambda\sigma_0^2 + (1-\lambda)\sigma_1^2)}.$$

Thus, equation (17) becomes

$$y = X\beta - \xi\,(\xi R_1 + (1-\xi)R_0)^{-1}\,R_1\,\epsilon + u. \tag{20}$$

Expanding the term in parentheses in a Taylor series about $\xi = 0$ we obtain,

$$y = X\beta - \xi\,R_0^{-1}\,R_1\,\epsilon + u + \nu \tag{21}$$

where ν represents the remainder term of the expansion. This regression equation is comparable to the Davidson and MacKinnon P1 test regression provided we interpret λ in (6) as being ξ in equation (21).

In practice, the exponential mixing likelihood function and equation (20) could be implemented using the same nonlinear methods used to identify the parameters in the

[15] This point is well known for the case of two nonnested univariate regressions with spherical normal errors and is due to Atkinson (1970) (see also Pesaran (1982)).

first Cox statistic (equation (3)). Some further computational savings can also be made in implementing this test by comparing the regression (21) to the Lagrange multiplier tests discussed in Breusch and Pagan (1980). That is, the test statistic from regression (21) can be reinterpreted as an auxiliary regression nR^2 test. Once again, however, it is unclear whether there are any finite sample advantages to be obtained by using this more complicated nonnested model misspecification test.

2.5 Other Forms of Nonnested Tests

Finally, as a practical and more simple computational alternative to these maximum likelihood and mixed regression procedures, one could abandon the strict mixing interpretation of λ and its appearance in the disturbances, so as to reduce the nonlinearities in equation (15). Once we abandon a strict mixed likelihood function approach to testing for misspecification, it is possible to obtain a variety of easily implemented nonnested specification tests. These tests can be derived directly by noting that if H_o is asymptotically correct, then almost any estimate of y that employs an estimate of $Z\gamma$ (such as the second right hand side term in equation (15)) can be used in a P-type specification test of H_o.[16] For example, an extremely simple nonnested test is motivated by what investigators have done previously in comparing time series models that exhibit serially correlated disturbances (Pesaran and Deaton (1978), Davidson and MacKinnon (1981), and Bean (1981)), which is to ignore serial correlation and to apply nonnested tests assuming classical (spherical) disturbances. Such an approach may be attractive because it avoids the possibility that the estimation of serial correlation process may mask the presence of omitted variables in the disturbances. For example, Davidson and MacKinnon (1981) compared alternative macroeconomic consumption models with serially correlated errors by applying ordinary

[16] This indeterminacy of nonnested specification mixing has drawn some criticism in the literature (e.g. Pesaran (1982)). Given the computational complexity of the nonnested procedures based upon (perhaps equally ad hoc) mixtures of likelihood functions, however, it is an open question as to whether the added computational complexity results in better finite sample properties of the test.

least squares to[17]

$$\tilde{u}_0 = X\pi - \lambda(X\tilde{\beta} - Z\tilde{\gamma}) + u_o \tag{22}$$

where the "tildes" ($\tilde{}$'s) denote ordinary least squares estimators. One could also consider applying unconstrained least squares to

$$\tilde{u}_o = X\pi + \lambda Z\tilde{\gamma} + u_o, \tag{23}$$

following the suggestions made in the paragraph above. Moreover, instead of ignoring the serial correlation of the residuals, one could also think of preserving the simplicity of the above tests by applying generalized least squares to equations (22) and (23) (using $\hat{\Omega}_o$), and testing whether λ is equal to zero using the t-test procedures described above.[18] These tests can all be made asymptotically valid t-tests of H_o because under the null hypothesis $y = X\beta + u_o$, and $Z\hat{\gamma}$ is extraneous to the asymptotic distribution of the test statistics. It is unclear, however, whether the choice of estimator for $Z\gamma$ can dramatically affect the finite sample properties of these statistics under the null hypothesis.[19]

The existence of a variety of asymptotically equivalent tests with substantially different computational costs naturally raises the issue of whether there are significant finite sample size and power differences among these alternative procedures. Indeed, in most practical applications it will be difficult, if not impossible, to tell whether a large number of observations guarantees that the asymptotic distribution of the different test statistics

[17] Although some of their consumption models were nonlinear, the generalization of this statement to nonlinear settings is straightforward. One could also consider applying generalized least squares to equations (22) and (23) to correct the standard errors. In the monte carlo experiments reported in the next section, we found that the two methods yielded essentially the same results.

[18] These are just a few of the many possible tests one could consider employing. Other tests could adjust $Z\gamma$ to take into account the serial correlation in the the alternative specification by estimating

$$\hat{u}_o = X\beta + \lambda[(Z_t - \hat{\rho}_1 Z_{t-1})\hat{\gamma} + \hat{\rho}_1 u_{1t-1}] + u$$

according to the test principles of the previous paragraph.

[19] Moreover, it is also important to consider how the choice of $Z\gamma$ affects the power of these test procedures given the interchange of null and alternative hypotheses.

is close to their finite sample distribution.[20] In many applications it may also be difficult to defend the normality assumption required by the maximum likelihood test procedures. Consequently, we propose evaluating the finite sample properties of these tests using monte carlo techniques. In particular, in section 5 we use monte carlo evaluations to gauge the size and the power of these tests against specific alternative investment models.

The next section describes the investment models used in our study; the following section briefly discusses the data; and the final section presents our evaluations of the investment models and the test statistics.

3. An Application: The Lack of a Consensus Model of Macroeconomic Investment Demand

A number researchers have attempted to form a consensus models of investment demand by evaluating empirically the performance of alternative theoretical and hodgepodge models on the same set of investment data. These studies have typically sought to measure and rank model performance on the basis of within- or out-of-sample goodness-of-fit criteria. Given the arbitrariness of these ranking criteria, however, it is not surprising that investigators can disagree on the merit of alternative models. Such a dilemma is apparent in Stephen Goldfeld's remarks on Peter Clark's (1979) investment specifications:

> "However, because these models [modified neoclassical and accelerator] are non-nested–that is, neither is a special case of the other–it is slightly problematic to choose between them. Standard errors within the sample favor the modified neoclassical model, but a forecasting criterion (measured either by anticipated forecast error or actual forecasts) reverses the ranking, leading Clark to prefer the accelerator model..." [p.118]

This section describes four traditional economic models of aggregate investment demand that we are interested in comparing.[21] These models are widely used (see for example

[20] The dependence of the different t-statistics on estimates of the two serial correlation parameters currently precludes the possibility of analytically deriving the finite sample distribution of λ, even under normality on the disturbances.

[21] By "traditional" we mean "a traditional model in existence prior to the Lucas (1976) critique." We do not consider post-critique models here primarily because these models usually do not fit into our linear regression format and they therefore would substantially increase our computational burden in the monte carlo section. It remains an open question

Clark (1979)) and our descriptions will therefore be brief. One major difference that should be noted at the outset, however, is that we will be comparing net investment equations (instead of the gross investment equations that are usually estimated). This change is made to avoid the problem of conducting the estimation and hypothesis test with what amounts to a (definitionally related) lagged dependent variable on the right hand side.[22] rates.

3.1 Accelerator Model

The generalized accelerator model is closely associated with the work of Chenery (1952) and Koyck (1954). This model assumes that the economy's "desired" level of capital stock, K_t^*, is proportional to the current level of output, Y_t; or, equivalently (assuming initial equilibrium), that the desired rate of net investment is proportional to the first difference of output. A strict application of the accelerator principle implies a greater volatility of investment spending than what has been observed. Hence, a costs-of-adjustment argument is usually invoked (see Clark) in support of the assumption that actual investment is linked to desired investment via a distributed lag. This leads to a specification for actual net investment, I_t, of the form

$$I_t = \alpha + \sum_{s=0}^{N} \beta_s \Delta Y_{t-s} + u_t \qquad (24)$$

where N is the lag length, α and the β_s are scalar parameters to be estimated, Δ is the first-difference operator, and u_t is an additive random disturbance. In estimating this model, as well as the neoclassical models (which are also of the basic accelerator form), we followed Clark in including a constant term. (See Clark's Appendix A.) Also following Clark's specification of accelerator and neoclassical models, we divided the dependent variable by a measure of potential output. We departed from Clark's specifications, however, in that we did not constrain the lag coefficients to lie on a particular polynomial. Instead, we chose to freely estimate eleven unconstrained lag coefficients on changes in output.

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as to how good these traditional models are when compared to more recent models. For recent an example of recent work on nonnested testing procedures applicable to these new models see Singleton (1984).

[22] In using net investment as our dependent variable we are implicitly ignoring that we have introduced specification error by using estimated depreciation rates. In both the actual estimation, and in some of trial monte carlo analyses, we considered the statistical consequences of using estimated depreciation rates and found them to be minimal.

3.2 The Neoclassical Model

Jorgenson's neoclassical model was the second model chosen for comparison. It incorporates capital costs into an accelerator-type specification. Assuming that the aggregate production function is Cobb-Douglas, and defining the desired capital stock K_t^* as the level at which the marginal product of capital services equals their rental price, one can write the desired capital stock, K_t^*, as

$$K_t^* = \frac{\gamma p_t Y_t}{c_t} \qquad (25)$$

where γ is the Cobb-Douglas capital-share parameter, p_t is the price of output, and c_t is the rental price of capital services (discussed further in the data section). Imbedding (2) in the generalized accelerator specification yields

$$I_t = \alpha + \sum_{s=0}^{N} \beta_s \Delta(\frac{pY}{c})_{t-s} + u_t. \qquad (26)$$

The distributed lag on the first difference of the desired capital stock terms was left unconstrained and estimated over eleven quarters.

3.3 The Modified Neoclassical Model

The modified neoclassical model is a variant of the neoclassical model due to Bischoff (1971a, 1971b). Unlike the standard neoclassical approach, Bischoff's model allows for "putty-clay" capital. That is, he acknowledges the possibility that it may be easier to modify factor proportions and thus the capital-output ratio *ex ante*. Bischoff shows (see the references just noted) that a simple version of the putty-clay hypothesis implies the formulation

$$I_t = \alpha + \sum_{s=0}^{n} \beta_{1s}[\frac{p_{t-s-1} Y_{t-s}}{c_{t-s-1}}] + \sum_{s=0}^{n} \beta_{2s}[\frac{p_{t-s-1} Y_{t-s-1}}{c_{t-s-1}}] + u_t. \qquad (27)$$

In estimating equation (26), we again truncated the distributed lag at eleven quarters.

3.4 The Securities-Value or q Model

The securities-value or "Tobin's q" model of investment posits that the rate of net investment should depend on the ratio of the market value of capital to its replacement cost (Tobin's q). Although a strict interpretation of the theory underlying this principle suggests that current investment should depend only on beginning of period values of q, it is well-known that investment is related to lagged q as well. Thus the standard empirical specification is

$$I_t = \alpha + \sum_{s=0}^{N} \beta_s q_{t-s} + u_t. \tag{28}$$

Clark (1979) and Summers (1981) normalized investment in the above regression by the capital stock; so as to have the same normalized investment series in each of our equations, we have instead deflated investment again by potential output. (Note that the q variable is a relative price and does not require normalization.)

In his study of the securities-value model, Clark used a quarterly q series constructed by von Furstenburg (1977). However, Summers (1981) has recently shown that adjustment of the q series to reflect corporate, dividend, and capital gains taxes improves the performance of the model in annual data. In our only significant departure from the empirical specifications used by Clark (other than replacing gross with net investment), we constructed a quarterly "tax-adjusted" q series to use instead of the von Furstenberg series (see Appendix A).

These four models were estimated on quarterly aggregate U.S. data. The sample period covered 1951:I to 1983:IV. The starting date was chosen so as to correspond to the starting date used by Clark. The ending date was determined by data availability. Separate equations were estimated for equipment and for structures. Except as specified, all data came from the national income and product accounts. Appendices A and C describe the data and the data sources in detail. In particular, Appendix C describes our development of a new q series.

4. Comparisons of the Investment Models

4.1 Estimation Results from the Investment Data

Table 1 presents the maximum likelihood estimates of the investment equations for the period 1951:I to 1983:IV assuming normal disturbances and first-order serial correlation. As in Clark, separate equations have been estimated for equipment and for structures.

The estimated coefficients and standard errors are qualitatively similar to those found in other studies and do not require extensive comment. Viewed in isolation, each of the models appears "successful": the estimated coefficients are universally of the right sign and magnitude.[23] (The effects of the various explanatory variables on investment are, however, typically smaller in magnitude and less persistent over time than those found by Clark.) Even though the distributed lags were estimated without constraints, the lag shapes are smooth and the implied dynamic response patterns are of a plausible shape. A less encouraging feature of the results is that the estimated serial correlation coefficients are high, typically over .95.[24] In the q model equation for equipment, for example, $\hat{\rho}$ is close to 1.0.

To compare these results with earlier work and to provide a benchmark with past specification tests, we calculated several conventional specification diagnostics that have been used in previous work. These diagnostics are reported below in tables 2 through 4. These conventional statistics provide a mixed assessment of these different models. Overall, they suggest that there is misspecification in each of the models.

Table 2 contains: (1) estimated standard errors for both the rho-transformed (row a) and the untransformed (row b) equations; (2) R^2 statistics, for the rho-transformed equations (row c), for the untransformed equations (row d), and for the untransformed equations; and (3) Durbin–Watson statistics, for the rho-transformed equations. These statistics are comparable to those presented by Clark and others.[25] Based on this table,

[23] The one exception is the coefficients of the q variable at some longer lags. Note also that the sign pattern for the modified neoclassical model conforms to what is predicted if investment depends strongly on the first difference of output. See Bischoff (1971b).

[24] That the serial correlation coefficients found here are higher than those estimated by Clark is probably due to his use of more lags of the explanatory variables, as well as to his inclusion of the lagged capital stock on the right-hand side of each equation.

[25] In particular, the R^2's presented by Clark, like ours in rows d and e, "give credit" to

one might be inclined to choose the modified neoclassical model as the best representation of the data. Its standard errors for both structures and equipment equations are either the lowest or close to the lowest attained for both the transformed and untransformed data. The various R^2 measures are also somewhat favorable to the MNC, and the Durbin–Watson statistic indicates little residual serial correlation after the initial correction. In contrast, the neoclassical model seems to be the worst by these same criteria. The other two models yield mixed inferences.

Table 3 provides further evidence on the specifications of the models based upon their out-of-sample forecast performance. The two columns of table 3 compare the within-sample simulation errors for the period 1951:I–1973:II to the out-of-sample forecast errors for the forecast period from 1973:III–1983:IV. (This sample break is just before the beginning of OPEC oil crisis and the 1973–75 recession. It also matches Clark's sample division.) In principle, for a model that is correctly specified and stable over time, simulation and forecast errors should be of roughly equal size. In fact, the forecast errors are several times bigger than the simulation errors for all of the models. There is a particularly notable deterioration out of sample in the accelerator and q equations for equipment, and in the modified neoclassical equation for structures. In absolute terms, however, the forecasting performance of the various models is not completely dismal; see figure 1 for a graphical comparison of the forecasts of the various models.

Finally, table 4 significantly weakens the tentative conclusion drawn from table 2 that the modified neoclassical model is the preferred model. That table presents test statistics for the hypothesis of no structural change before or after 1973:III. (Our test accounts for the non-spherical nature of the disturbances, and is therefore a chi–square test; Clark used an F–test.) The most striking result is that both the modified neoclassical model and of the accelerator model are unstable models of equipment. investment. This is fairly strong evidence of misspecification, which contrasts with the generally favorable goodness-of-fit statistics. for these two models.

We now turn to a more formal nonnested test comparison of these models.

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the models for the part of the dependent variable forecast by the term $\hat{\rho}\hat{\epsilon}_{t-1}$. Arguably, this overstates the success of the various basic models in fitting the data.

4.2 Test Results

To compare the finite sample properties of these alternative nonnested models of investment demand, we chose to compare five of the above nonnested tests:[26]

C1 Pesaran's (1974) adaptation of the Cox statistic;

C2 the linearized version of Pesaran's statistic given in equation (4);

P1 the auxiliary regression P-test;

P2 the P-test that employs estimators of the alternative model assuming the null is correct; and,

P3 the (GLS) t-statistic for λ in equation (22).

We have limited our attention to these statistics because they represent the range of computational difficulty and the range of estimators required to test these competing models.

A successive substitution algorithm was used to find the maximum likelihood estimates of the alternative model assuming that the null is true. (See Pesaran (1974), equations S.10 to S.12. Hereafter we shall refer to these estimates as the "Pesaran estimates".) This algorithm estimates an initial ρ_{10} from the initial maximum likelihood estimates of the slope coefficients and then determines new estimates of β_{10} and σ_{10}. This process is repeated until the estimates converge. In practice, when the algorithm converged it did so within 20 to 30 iterations, regardless of the starting values. There were several instances, however, where we had difficulties computing the maximum likelihood nonnested test statistics. First, when there were more than 12 to 14 lagged exogenous variables in some of the specifications, it was possible to find more than one solution to the nonlinear equations determining the Pesaran estimates. This often occurred for modified neoclassical models containing more than 14 lags, and appears to be related to the high correlation among the regressors. Second, when computing the initial Pesaran estimates of the highly serially correlated equipment q model, we found that there was no apparent maximum of the likelihood function for values of ρ_{10} less than .9999. As the likelihood function was increasing at this point, we constrained ρ_{10} to equal .9999.[27] Finally, there were a number of instances in which the successive

[26] To ensure comparability of the signs among the P and C statistics in the following tables, the signs of P1 and P2 were reversed.

[27] One could interpret the failure of the Pesaran estimates to have an admissible value

substitution algorithm failed to converge. For these cases we used a modified Fibonacci search procedure (on values of ρ_{10}) to find the maximum likelihood estimates. This modified procedure was substantially more time consuming, requiring 100-150 iterations on average (approximately 2.0 CPU minutes on a DEC-20) before successive values of ρ_{10} were the same to five decimal places.

The nonnested test statistics for equipment and structures are reported in table 5. Under the null hypothesis which we used to develop these statistics in section 2, each of these statistics is asymptotically distributed as a standard normal random variable. In comparing the equipment specification test results in table 5, it becomes clear that C1 and C2 are uniform in their conclusions, while P1 and P2 differ between themselves and are different from their maximum likelihood counterparts C1 and C2. For example, if we had only employed C1 or C2 to test these equipment investment models, we would have been led to conclude that none of the models is acceptable (at a 5% critical level). The accelerator model, however, is the closest to being acceptable in that it rejects all of the other models and only is rejected by the q model. If, on the other hand, we had employed only P1, we would also have concluded that none of the models is acceptable, but this time it is the modified neoclassical model that rejects the accelerator. Test P2 is the only test that indicates that the accelerator model cannot be rejected by the other models. When the accelerator model is made the alternative model, P2 also indicates that the accelerator is capable of rejecting all but the modified neoclassical model. Finally, P3 provides the most negative evidence on all the models, indicating that almost all are capable of rejecting each other.

The results of the tests for the nonresidential structures investment data are similar to the equipment results as far as the general comparability of the absolute magnitudes of the different statistics and their rejection of all four specifications. The q model, however, is now the model that appears to be the closest to being nominally acceptable.

What are we to conclude from this conflict among the test results? Aside from the obvious point that the ex ante choice of test may influence the outcome, it is important to note in table 5 that there are very substantial absolute differences in the magnitudes of

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of ρ_{10} as an indication of misspecification of the (equipment) q model. We, however, chose to proceed as though it was correct.

the test statistics. Given they all have the (same) asymptotic distribution under the null hypothesis, this suggests either that there are substantial differences in the finite sample distributions of these statistics (for these data and models) or that there can be substantial finite sample differences in the power of these alternative statistics.

4.3 Monte Carlo Evaluations

The above conflicts suggest that before accepting or rejecting nonnested alternatives that have test statistics close to the critical values, applied investigators should consider performing monte carlo or bootstrap evaluations of their test statistics. These evaluations, although somewhat specific to the particular application, could be used to reconcile any conflicts among the testing criteria and to evaluate empirically their power properties against any alternative that appears to be correctly specified. Below we illustrate how a monte carlo analysis could proceed using the equipment models. The equipment models were analyzed largely because of the marginal rejection of the accelerator model.

The monte carlo analysis was conducted by first generating normal errors whose first two moments matched those obtained for the original accelerator model.[28] Serially correlated errors were then generated using a serial correlation parameter equal to that estimated for the original accelerator model ($\rho_0 = .9674$). For comparison, we also retained the original errors (where implicitly $\rho_0 = 0$). Artificial equipment investment series were then generated using the estimated values of the accelerator regression coefficients reported in table 1. A final notable restriction placed on the monte carlo evaluations is that we limited our experiments to 100 replications in order to reduce the computational and programming complexity of the comparisons.[29] Further, the 100 sets of drawings of the error terms were constrained to be the same for each test statistic so that the results in tables 6 through 8

[28] There are two reasons for the normality assumption. First, normal errors serve as an important benchmark for the evaluation of the maximum likelihood tests. Second, inspection of the errors from the actual model could not reject the hypothesis that they were normally distributed, and indeed the bootstrap evaluations we did undertake did not produce substantially different results from the monte carlo techniques.

[29] Several cases were evaluated 500 times. There was no noticeable changes in the point estimates of the rejection probabilities for these cases. To obtain approximate standard errors for the size calculations reported in table 8, one can use the independence of the (binomial) trials to justify the formula $\sqrt{\hat{p}(1-\hat{p})/100}$ where $\hat{p}$ is the test size.

can be interpreted as 100 comparisons that are similar to those made in table 5 (where all tests were calculated using the same data). Further details on the monte carlo experiments are available from the authors.[30]

The test comparisons are in tables 6 through 8. Table 6 presents summary central tendency measures on the empirical distribution of the 100 replications of each test statistic when the accelerator is the true model. That is, this table attempts to evaluate the adequacy of the asymptotic normal approximation when the null model is the model that generated the data. Table 8 presents comparable tail probability estimates that are designed to evaluate the adequacy of the nominal (asymptotic) size of the four tests when the null model is true. For the remainder of this sub-section, we consider the results with $\rho = .9674$.

The relative inferences drawn in table 5 are consistent with the monte carlo results in tables 6 and 8. Tests C1 and C2 are similar in magnitude and lead us to similar inferences about the null. The same is also true for the relationship between inferences drawn with P1 and P2. However, although all of these tests are asymptotically equivalent, the P and C tests are not very highly correlated. For example, for the 100 trials where the (correct) accelerator was compared to the neoclassical model as the alternative, the pairwise correlations between C1 or C2 and P1 or P2 were below .10. This finding is consistent with differences observed among comparable statistics for the linear model with spherical errors (see Davidson and MacKinnon (1981).[31] As far as the adequacy of the asymptotic normal approximation to the finite sample distribution of the individual tests, we cannot reject the hypothesis that C1 and C2 have zero mean and unit variance. It is important to note, however, that the standard deviations are uniformly too large and there

[30] We have limited our analysis to the equipment models to conserve space and to limit the computational burden. (For example, over 1,800 sets of maximum likelihood estimates had to be computed to construct table 6.) We also experimented with bootstrap techniques and found the results to be similar in practice. However, there clearly are different computational and statistical tradeoffs involved with the bootstrap. (See Efron (1979) and Efron and Gong (1983).)

[31] The pairwise correlation of C1 and C2 was .99 and that for P1 and P2, .98. These correlations are virtually the same for the other comparisons when the null model is the accelerator. When the null model is incorrect and the accelerator is the alternative, the pairwise correlations among the C and P tests increase. (They range roughly between .2 and .5.)

is a tendency to observe too many extreme values in one tail. The first two moments of P1 and the P2 are less close to their asymptotic values, and one can usually reject the hypothesis that they have zero mean. Finally, the computationally simpler P3 statistic's empirical density function is furthest from the asymptotic approximation. Thus, on the basis of these results, we conclude that it may be well worthwhile to bear the increased computational complexity of C1, C2, P1, and P2.

Further investigation of the empirical density functions of all of these test statistics revealed that they are skewed in the direction of their mean. In most cases, this skewness is a consequence of a thick tail. This thick tail of the empirical density implies that the asymptotic critical levels will tend to understate the finite sample size of the test and therefore reject the null too often, a conclusion that is in agreement with evidence from several previous studies of the size of these tests when the errors are homoskcedastic and not serially correlated. Table 8 indicates that this bias in the size is more consequential for P1, P2, and P3, although not insubstantial for C1 and C2 given the length of our time series relative to the usual amount of time series data used in investment studies. Indeed, if we use the empirical distribution functions of the test statistics in table 8, the accelerator model is not rejected by C1, C2, or P1, or P2.

4.4 Serial Correlation as a Reflection of Omitted Variables

Table 7 reports evidence on the power of these tests against the (true) alternative accelerator model. The table is designed to indicate whether these tests can detect a misspecified null when there is possibility that the serial correlation correction could mask the misspecification due to the omitted accelerator variables. The major conclusion that can be drawn from this table is that the power of these different test statistics is uniformly high, even when one corrects for serial correlation. Further, the magnitudes of the test statistics can be used to explain the rather large values of the test statistics reported in table 5. In other words, the discrepancy of the test values in table 5 with the asymptotic normal distribution can be reconciled with the statistical properties of these statistics when the alternative is true.

5. Conclusion

This paper explored a number of alternative nonnested specification tests of time series investment models on quarterly U.S. business investment data from 1951:I to 1983:IV. We concluded that on the basis of various alternative nonnested test statistics (summarized in section 2), that no model was acceptable when compared against each of the other alternatives for either the equipment or structures data. Upon investigating the equipment model test results for bias in the nominal size of the test, we were able to conclude that the (pairwise) test results were *consistent* with the accelerator model being a correctly specified equipment model. Thus, future evaluations of investment models may wish to use the accelerator as a benchmark for comparison.

We also wish to emphasize several other features of our results. First, the extension of nonnested testing procedures of Pesaran, Fisher and McAleer, and Davidson and MacKinnon to time series models with first-order serially correlated errors is similar to principle to multivariate models. In practice, these nonnested tests appear to have desirable size and power properties even when a serial correlation nuisance parameter is estimated and the only serial correlation is due to specification error. We would recommend, however, that if the values of these nonnested tests are at all close to their asymptotic critical values that the investigator evaluate empirically the adequacy of the asymptotic distribution.

Second, in time series contexts of the type studied here, it appears as though the more complicated C1, C2, P1, and P2 statistics have better finite sample properties than their computationally simpler counterparts (e.g. P3). Clearly, however, this conclusion and our monte carlo results must be tempered by the observation that in practice investigators may wish to consider more general serial correlation or moving average error processes than the first-order processes. Future work might profitably extend the analysis in Walker (1967) and Pesaran (1974) to these more complicated situations, and provide comparative finite sample evidence such as that considered in this article.

Finally, we wish to emphasize that this application also illustrates how useful nonnested tests can be in detecting whether serial correlation reflects misspecification error. In particular, our experience with these investment data and models indicates that these tests perform quite well in a situation where the disturbances of each model are highly serially correlated and the regressors are temporally correlated within and across models.

APPENDIX A

The Data and a Description of the Variables

The Dependent Variables

The dependent variable I_t is net investment by nonfinancial corporations. Corporate investment was chosen because the securities-value, or q, model is more relevant to this component of total investment. Corporate investment was constructed separately for equipment and structures by the following procedure. First, annual nonfinancial corporate gross investment data (from the Bureau of Economic Analysis) were used to construct a quarterly gross investment series. Our procedure for interpolating the investment series is that due to Chow and Lin (1971), where we used a constant and a quarterly series on total nonfinancial business investment (from the *Survey of Current Business: National Income and Product Accounts*) as predictors. Next, the predicted quarterly gross investment series were cumulated to form gross investment capital stocks. As a benchmark, we used capital stock data for 1947:IV and 1982:IV in Musgrave (1981), and then set rates of depreciation (0.03784 for equipment, 0.01412 for structures) to interpolate these values. Finally, net investment was obtained by subtracting the product of the lagged capital stock and the relevant depreciation rate from the gross investment series.

In estimating the investment models described in section 3, the net investment series were divided by potential output, according to the convention of Clark. The potential output series was obtained from Gordon (1984) and updated assuming 3% growth.

The Independent Variables

Most of the independent variables used in this study also come from the *Survey of Current Business*. The output variable, Y_t, is the *NIPA* real gross domestic product of nonfinancial businesses. In the neoclassical model, p_t is the deflator corresponding to Y_t. The variable c_t is the rental price of capital and was derived according to Clark's (1979, appendix B) procedure using the formula

$$c = \frac{p_E(\delta_E + r) \cdot (1 - ITC_E - D \cdot ZE \cdot U \cdot ITC_E - ZE \cdot U)}{(1 - U)}$$

for equipment, and

$$c = \frac{p_S(\delta_S + r) \cdot (1 - ITC_S - ZS \cdot U)}{(1 - U)}$$

for structures. Here, δ_E and δ_S are the rates of depreciation derived above, p_E and p_S are the deflators for nonfinancial business investment (from NIPA), U is the corporate tax rate (we took the highest marginal rate on corporate income from Seater (1982)), and D is a dummy variable that was set equal to 1.0 when the Long amendment to the Revenue Act of 1962 was in effect. The discount rate r was constructed exactly as in Clark (1979, footnote 40). The present value of a dollar's worth of depreciation allowances (ZE and ZS) used the formulae given in Hall and Jorgenson (1967), where data on the average lifetime of investments from Jorgenson and Sullivan (1982) and the BAA bond rate (from Survey of Current Business) were used in the discounting. The rates adopted in the investment tax credit, ITC_E and ITC_S were also taken from Jorgenson and Sullivan (1982).

The tax-adjusted q variable was constructed using the general form of Summers' (1981) q series,

$$q = \frac{1}{1-U} \cdot \left(\frac{V-B}{K} - 1.0 - b - ITC + U \cdot Z\right)$$

but with several modifications. The major deviation from Summers' formula is that we did not include a correction for future tax liabilities on dividends. A rationale for this omission can be found in Poterba and Summers (1983). The components of this equation are defined as follows.

The nominal market value of firms, V, is the ratio of dividends paid by the nonfinancial corporate sector (NIPA) to dividend yield (Standard & Poors 500). The nominal capital stock is $K = p_E K_E + p_s K_S + INV$, where p_E and p_S are the investment deflators, K_E and K_S are the stocks of real equipment and structures. The variable INV is nominal inventories of nonfinancial corporations. This series was also interpolated from annual data by the method of Chow and Lin. The interpolation formula used quarterly data on business inventories from *NIPA* and a constant term.

The investment credit ITC and the present value of a dollar's worth of depreciation allowances are investment-weighted averages of the relevant constituent variables. The variable B is the ratio of debt to capital (K), where the debt of nonfinancial corporation was derived as the ratio of net interest payments (*NIPA*) and the rate of interest (the *BAA* corporate rate, from ***Survey of Current Business***). To derive B, the present value of depreciation allowances of nonfinancial corporations, we defined the taxable capital stock, $KTAX$, as the capitalized difference of the value of total investment (equipment and structures) minus capital consumption allowances, CCA (excluding capital consumption adjustment, from NIPA). To reduce the effect of an inaccurate initial value, we set $KTAX$ equal to the actual capital stock in 1931:4 and started the capitalization from that date. B is then the present value of reduced taxes due to depreciation of the current taxable capital stock,

$$B = U \cdot \frac{\delta_T}{\delta_d + r_B(1-U)} \cdot KTAX,$$

where $r_B(1-U)$ is the quarterly, risk free, after-tax interest rate (on long term government bonds, Standard & Poors), and $\delta_d = \frac{CCA}{KTAX}$ is the rate of tax depreciation.

APPENDIX B

Simplification of the Pesaran/Cox Statistic for Autocorrelated Errors

This appendix outlines the asymptotic simplifications of Pesaran's Cox statistic. The numerator of the Cox statistic is given by Pesaran's equation (S.4)

$$C0 = \frac{T}{2} ln \frac{\sigma_1^2}{\sigma_{10}^2} + \frac{1}{2} ln \frac{1-\rho_o^2}{1-\rho_1^2}. \tag{A.1}$$

Taking a Taylor series expansion of this expression, and retaining all terms that do not vanish asymptotically when $C1$ is standardized by the square root of the sample size, yields

$$C2 = \frac{T}{2}\left(\frac{\sigma_1^2 - \sigma_{10}^2}{\sigma_{10}^2}\right). \tag{A.2}$$

It is easy to show that the asymptotic standard error of $C2$ is

$$\sqrt{\text{Var }(C2)} = \sqrt{\text{Var }(C1)} = \frac{\sigma_o}{\sigma_{10}^2}(\epsilon' D\epsilon)^{\frac{1}{2}} \tag{A.3}$$

where $\epsilon = X\beta - Z\gamma_{10} = (y - Z\gamma_{10}) - (y - X\beta) = u_1 - u_o$, $D = R_1^{-1}[R_o - X(X'R_o^{-1}X)^{-1} X']R_1^{-1}$, and $\sigma_o^2 R_o^{-1} = \Omega_o$ and $\sigma_1^2 R_1^{-1} = \Omega_1$. The asymptotic t-ratio for C2 is therefore

$$\frac{C2}{\text{Var }(C2)^{\frac{1}{2}}} = \frac{T}{2}\left(\frac{\sigma_1^2 - \sigma_{10}^2}{\sigma_0(\epsilon' D\epsilon)^{\frac{1}{2}}}\right). \tag{A.4}$$

This expression can be simplified by noting

$$\sigma_1^2 = \frac{u_1' R_1 u_1}{T} \tag{A.5}$$

and using Pesaran's estimating equation (S.6),

$$\sigma_{10}^2 = \frac{\epsilon' R_{10}\epsilon}{T} + \frac{u_o' R_{10} u_o}{T} \tag{A.6}$$

where R_{10} is the probability limit of R_1 assuming the null hypothesis is correct. Substituting equations (A.5) and (A.6) into (A.4)

$$\frac{C2}{\text{Var }(C2)^{\frac{1}{2}}} = \left(\frac{\frac{u_0'(R_1 - R_{10})u_0}{2} + \frac{\epsilon'(R_1 - R_{10})\epsilon}{2} + \epsilon' R_{10} u_o}{\sigma_0(\epsilon' D\epsilon)^{\frac{1}{2}}}\right). \tag{A.7}$$

Under H_o, ρ_1 converges to ρ_{10} and Slutsky's theorem can be used to show that the first two terms converge in probability to zero. Thus, asymptotically Pesaran's statistic is equivalent to

$$\left(\frac{\epsilon' R_{10} u_o}{\sigma_0 (\epsilon' D \epsilon)^{\frac{1}{2}}}\right). \tag{A.8}$$

APPENDIX C

The Quarterly, Tax-adjusted q-series

This appendix contains our q-series, as computed using the formula in appendix A, and compares it to that of Summers (1981).

A plot of both series for the years in which we have comparable data is provided in figure 2. The series move similarly over time and a have a high correlation (0.9618 for the comparable 31 values: 1948-1978). The differences between the two series are small. The main difference between the two series is in the levels. Apart from differences in data sources (in particular on lifetimes of investments), this can be explained by our decision not to over-adjust for dividend taxation. This adjustment increases Summers' values for q during this sample period because the factor $\frac{1-c}{1-\theta}$ in Summers' equation (A-13) is larger than unity.

The q values obtained from the procedure described in appendix A are:

YEAR	Q1	Q2	Q3	Q4
1947		.29304	.20494	.17301
1948	-.02211	.03242	.03353	-.09147
1949	-.18694	-.21096	-.27100	-.22595
1950	-.17527	.12042	.11087	.09067
1951	.01004	-.06918	-.21648	-.05107
1952	.04803	-.04260	.11115	.01932
1953	.26288	.00076	-.03113	-.08092
1954	.00608	-.00066	.04727	.23899
1955	.30523	.41870	.60327	.69960
1956	.68475	.64726	.50422	.35758
1957	.51130	.41282	.47948	.25969
1958	.15596	.31948	.47858	.61340

1959	.66751	.76526	.88520	.81454
1960	.87025	.64007	.70412	.66715
1961	.78022	.95603	.98640	1.0181
1962	1.1711	1.1954	.78334	.82370
1963	.91359	1.0969	1.2176	1.3046
1964	1.3366	1.3032	1.3952	1.4098
1965	1.4187	1.3145	1.2910	1.4601
1966	1.5800	1.3682	1.0920	.79381
1967	.75480	1.0410	1.1412	1.1478
1968	.94291	1.1026	1.2270	1.3367
1969	1.3085	1.1033	.88763	.68819
1970	.61683	.41176	.13344	.25157
1971	.41917	.60627	.51983	.50884
1972	.49333	.78818	.72924	.73995
1973	.81111	.70776	.59122	.52288
1974	.29029	.18521	.12560	-.26462
1975	-.32589	-.03713	.04003	-.09201
1976	-.00733	.13449	.25051	.25916f
1977	.26697	.08044	.05727	.02158
1978	.00895	-.08243	-.09559	.01047
1979	-.04132	-.09700	-.15138	-.18714
1980	-.26105	-.35382	-.23600	-.21014
1981	-.21476	-.20165	-.23772	-.30317
1982	-.30348	-.43277	-.43434	-.33526
1983	-.15509	-.02692	.09708	.06253
1984	.01456			

APPENDIX D

Computational Details

The calculations reported in this paper came from two sets of programs. An econometric program, PEC©, was used to obtain the initial maximum likelihood estimates and to generate the random numbers. (The random number generators were initialized with random seeds.) A fortran program was written to calculate the Pesaran estimates. Both of these programs were run on a DEC-20. Further details on the programs, computations and data are available upon request.

TABLE 1

RESULTS FOR 1951:I TO 1983:IV

Accelerator Model			Neoclassical Model			Q		
Variables	Equipment	Structures	Variables	Equipment	Structures	Variables	Equipment	Structures
YPI	3.497	4.696	YPI	6.406	3.069	Con	.01141	.00576
	(2.28)	(1.10)		(2.983)	(3.126)		(.0130)	(.0011)
DYY(0)	.0747	.0156	XNC(0)	.0053	.0006	Q(0)	.00240	-.00007
	(.014)	(.007)		(.002)	(.001)		(.0010)	(.0004)
DYY(-1)	.1223	.0306	XNC(-1)	.0118	.0015	Q(-1)	.00425	.00108
	(.015	(.007)		(.003)	(.001)		(.0011)	(.0004)
DYY(-2)	.1305	.0244	XNC(-2)	.0135	.0012	Q(-2)	.00280	.00141
	(.016)	(.008)		(.003)	(.001)		(.0011)	(.0004)
DYY(-3)	.1325	.0279	XNC(-3)	.0154	.0032	Q(-3)	.00276	.00521
	(.016)	(.008)		(.003)	(.001)		(.0011)	(.0004)
DYY(-4)	.1254	.0421	XNC(-4)	.0142	.0046	Q(-4)	.00164	.00051
	(.017)	(.008)		(.003)	(.001)		(.0010)	(.0004)
DYY(-5)	.1228	.0326	XNC(-5)	.0127	.0039	Q(-5)	.00128	.00129
	(.017)	(.008)		(.003)	(.001)		(.0010)	(.0004)
DYY(-6)	.1083	.0219	XNC(-6)	.0135	.0030	Q(-6)	.00045	-.00030
	(.017)	(.008)		(.003)	(.001)		(.0010)	(.0004)
DYY(-7)	.1039	.0148	XNC(-7)	.0117	.0014	Q(-7)	.00174	-.00010
	(.016)	(.008)		(.003)	(.001)		(.0010)	(.004)
DYY(-8)	.0488	.0162	XNC(-8)	.0090	.0016	Q(-8)	.00104	.00078
	(.015)	(.007)		(.003)	(.001)		(.0011)	(.0004)
DYY(-9)	.0592	.0174	XNC(-9)	.0076	.0020	Q(-9)	-.00093	-.00011
	(.015)	(.007)		(.003)	(.001)		(.0011)	(.0004)
DYY(-10)	.0335	.0149	XNC(-10)	.0044	.0021	Q(-10)	-.00063	-.00001
	(.014)	(.007)		(.003)	(.001)		(.0010)	(.0004)
DYY(-11)	.0551	.0006	XNC(-11)	.0046	.0017	Q(-11)	.00067	-.00072
	(.013)	(.006)		(.002)	(.001)		(.0010)	(.0004)
ρ_0	.967 (.02)	.967 (.02)	ρ_0	.969 (.02)	.995	ρ_0	.995 (.02)	.960 (.02)
σ_0	.13E-02	.61E-03	σ_0	.16E-02	.66E-03	σ_0	.15E-02	.59E-03
DW	2.05	1.77	DW	1.59	1.78	DW	1.89	1.89

TABLE 1
(continuation)

Modified Neoclassical Model

Variables	Equipment	Structures	Variables	Equipment	Structures
YPI	-2.025	3.991			
	(1.831)	(1.615)			
X1MNC(0)	.0152	.0024	X2MNC(0)	-.0128	-.0026
	(.0035)	(.0013)		(.0037)	(.0013)
X1MNC(-1)	.0259	.0048	X2MNC(-1)	-.0254	-.0045
	(.0037)	(.0013)		(.0039)	(.0014)
X1MNC(-2)	.0271	.0024	X2MNC(-2)	-.0268	-.0020
	(.0039)	(.0015)		(.0041)	(.0014)
X1MNC(-3)	.0301	.0042	X2MNC(-3)	-.0295	-.0035
	(.0041)	(.0015)		(.0042)	(.0015)
X1MNC(-4)	.0266	.0060	X2MNC(-4)	-.0272	-.0059
	(.0041)	(.0016)		(.0043)	(.0016)
X1MNC(-5)	.0276	.0054	X2MNC(-5)	-.0278	-.0050
	(.0042)	(.0015)		(.0043)	(.0016)
X1MNC(-6)	.0248	.0031	X2MNC(-6)	-.0247	-.0033
	(.0042)	(.0015)		(.0043)	(.0016)
X1MNC(-7)	.0242	.0022	X2MNC(-7)	-.0251	-.0022
	(.0040)	(.0015)		(.0042)	(.0015)
X1MNC(-8)	.0113	.0030	X2MNC(-8)	-.0105	-.0032
	(.0039)	(.0013)		(.0040)	(.0014)
X1MNC(-9)	.0134	.0033	X2MNC(-9)	-.0137	-.0030
	(.0038)	(.0013)		(.0039)	(.0014)
X1MNC(-10)	.0070	.0033	X2MNC(-10)	-.0066	-.0034
	(.0037)	(.0012)		(.0037)	(.0013)
X1MNC(-11)	.0157	.0022	X2MNC(-11)	-.0153	-.0008
	(.0036)	(.0012)		(.0036)	(.0012)
ρ_0	.899 (.02)	.952 (.03)			
σ_0	.13E-02	.60E-03			
DW	2.01	1.86			

Standard errors are in parentheses. The dependent variable is net investment divided by potential output. All equations are estimated by maximum likelihood. The standard error for the equation, σ_0, and the Durbin-Watson, DW, are for the differenced residuals.

The variables are defined as follows: YPI is the inverse of potential output (YPI = 1/YP); DYY(i) is the i-th lag change in output divided by potential output (DYY(i) = $\Delta Y_{t-i}/YP_{t-i}$); XNC(i) is the i-th lag on the first difference of desired capital stock (XNC(i) = $\Delta(pY/cYP)_{t-i}$); X1MNC(i) = $p_{t-1-i}Y_{t-1}/c_{t-i-1}$ and X2MNC(i) = $p_{t-i-1}Y_{t-i-1}/c_{t-i-1}$; Con is a constant term; and Q(i) is the beginning of period value of q. For a further description of the variables and the relevant sources, see Appendix A.

Table 2

Conventional Model Diagnostics
(Equipment/Structures)

		Accelerator	Neoclassical	MNC	q
Standard error × 10^{-2}	(a)	.13/.061	.16/.066	.13/.060	.15/.059
	(b)	.49/.22	.59/.41	.26/.17	1.19/.20
R^2	(c)	.53/.29	.24/.14	.56/.40	.34/.35
	(d)	.94/.95	.90/.95	.94/.96	.91/.96
	(e)	.93/.94	.89/.94	.93/.95	.89/.96
DW	(f)	2.05/1.77	1.59/1.78	2.01/1.86	1.89/1.89

<u>Notes</u>

(a), (c), (f) -- Calculated using quasi-differenced data
(b) -- Calculated using undifferenced data
(d) -- Calculated using undifferenced data, including forecastable part of residual in prediction of dependent variable; unadjusted for degrees of freedom
(e) Same as (d), adjusted for degrees of freedom

Table 3

Simulation and Forecast Errors
(Equipment/Structures)

		Accelerator	Neoclassical	MNC	q
Standard error × 10^{-2}	(a)	.12/.064	.17/.069	.12/.061	.15/.062
	(b)	.65/.18	.53/.26	.48/.24	.63/.17

(a) -- Within-sample root mean square simulation error, 1951:I-1973:II; quasi-differenced data
(b) -- Out-of-sample root mean square forecast error, 1973:III-1983:IV; quasi-differenced data

Table 4

Tests of Structural Stability
(Equipment/Structures)

	Accelerator	Neoclassical	MNC	q
χ^2	23.2**/12.6	11.1/10.8	49.1**/33.6	11.4/8.4
d.f.	13	13	25	13

The test is of the equality of the coefficients between the subsamples 1951:I-1973:II and 1973:III-1983:IV.

(*) denotes significance at .10 level
(**) denotes significance at .05 level

Table 5
Nonnested Test Statistic Values

Null Model	vs.	Alt. Model	Equipment Data C1	Equipment Data C2	Equipment Data P1	Equipment Data P2	Equipment Data P3	Structures Data C1	Structures Data C2	Structures Data P1	Structures Data P2	Structures Data P3
Acc		NEO	1.16	1.18	-.31	1.04	-.31	1.32	.97	-.39	1.35	-.24
Acc		MNC	1.35	1.77	-2.14	1.64	-4.23	-2.96	-2.73	-4.03	-.60	-4.36
Acc		Q	-2.08	-2.24	-.88	-1.16	-2.95	-6.59	-5.83	-4.63	-2.22	-5.30
Neo		Acc	-16.01	-12.05	-7.06	-5.52	-9.23	-7.34	-6.22	-2.87	-3.32	-4.85
Neo		MNC	7.10	8.58	-6.32	-5.78	-10.60	-8.39	-7.01	-2.51	-3.32	-6.87
Neo		Q	-8.28	-7.41	-2.97	-2.89	-5.90	-12.35	-10.15	-2.70	-3.13	-6.48
MNC		Acc	-6.73	-6.58	-3.17	-.38	-3.15	-5.64	-5.78	-.67	-.13	-.48
MNC		NEO	.92	.74	.76	2.06	.76	.07	-.43	.36	2.11	.57
MNC		Q	-2.53	-2.83	.27	-.05	-1.75	-6.51	-5.81	-4.07	-1.54	-4.44
Q		Acc	-8.16	-6.12	-10.66	-28.88	-8.80	-3.70	-3.50	-3.82	-.29	-4.29
Q		NEO	-3.61	-3.11	-5.93	-6.02	-3.71	.11*	-.33*	-1.90*	2.32*	-2.81
Q		MNC	-12.96	-9.84	-5.48	-9.51	-9.75	-2.69	-2.51	-4.79	-.63	-5.43

Each test statistic is asymptotically distributed as a standard normal random variable. The different test procedures are described in Section 4 of the text.

Table 6

Nonnested Test Values from 100 Monte Carlo Replications

True Model = Accelerator

			ρ = .9674					ρ = .0000				
Null Model	Alt. Model	Summary Statistic	C1	C2	P1	P2	P3	C1	C2	P1	P2	P3
ACC	NEO	Mean	-.15	-.10	.50	.61	-1.33	-.46	-.60	.25	-.14	-.40
		Median	-.01	-.06	.38	.59	-1.18	-.41	-.58	.22	-.23	-.44
		Mean Std. Err.	.12	.11	.13	.13	.10	.13	.13	.12	.15	.09
ACC	MNC	Mean	-.21	-.08	-.80	-1.42	-3.65	.22	.26	.01	.10	-2.48
		Median	-.19	-.10	-.73	-1.41	-3.55	.29	.29	.41	.09	-2.48
		Mean Std. Err.	.21	.20	.13	.30	.12	.15	.15	.52	.03	.11
ACC	Q	Mean	-.27	-.21	2.16	3.34	-1.60	-.61	-.78	3.25	5.21	-.40
		Median	-.25	-.18	1.98	3.25	-1.64	-.60	-.81	3.20	5.24	-.45
		Mean Std. Err.	.12	.12	.15	.15	.11	.14	.14	.14	.16	.09

Table 7

Nonnested Test Values from 100 Monte Carlo Replications

True Model = Accelerator

			ρ = .9674					ρ = .0000				
Alt. Model	Null Model	Summary Statistic	C1	C2	P1	P2	P3	C1	C2	P1	P2	P3
ACC	NEO	Mean	-15.3	-11.3	- 7.6	-7.9	- 9.3	-26.2	-16.5	-6.2	-8.0	-23.3
		Median	-15.1	-11.2	7.4	-7.8	- 9.3	-26.0	-16.4	-6.2	-7.7	-22.9
		Mean Std. Err.	.25	.14	.06	.11	.11	.30	.13	.04	.14	.27
ACC	MNC	Mean	-18.6	-13.3	- 8.5	-7.7	- .1	- 6.9	- 6.0	-6.1	-6.5	- .0
		Median	-18.8	-13.4	- 8.4	-7.7	- .0	- 7.0	- 6.1	-6.4	-6.0	- .1
		Mean Std. Err.	.33	.19	.10	.13	.10	.14	.11	.69	.69	.04
ACC	Q	Mean	-20.8	-14.9	-13.2	-8.9	-10.1	-30.7	-18.4	-4.8	-6.6	-28.8
		Median	-20.9	-15.0	-12.2	-8.9	-10.1	-29.9	-18.2	-4.81	-6.5	-28.9
		Mean Std. Err.	.37	.21	.37	.14	.13	.33	.14	.04	.15	.35

Table 8

Estimated Critical Values from Table 3 Replications

			ρ = .9674				ρ = .0000			
Null Model	Alt. Model	Statistic	Pr(T > 1.965) = .025	Pr(T < -1.965) = .025	Pr(T > 1.65) = .05	Pr(T < -1.65) = .05	Pr(T > 1.965) = .025	Pr(T < -1.96) = .025	Pr(T > 1.645) = .05	Pr(T < -1.645) = .05
ACC	NEO	T = C1	.03	.05	.04	.12*	.04	.13*	.05	.19*
		= C2	.03	.04	.06	.10*	.03	.14*	.05	.21*
		= P1	.14*	.03	.19*	.03	.08*	.01	.14*	.03
		= P2	.18*	.04	.21*	.04	.08*	.08*	.14*	.19*
		= P3	.00	.27*	.00*	.36*	.00	.07*	.00*	.09*
ACC	MNC	T = C1	.13*	.18*	.17*	.24*	.10*	.09*	.16*	.12*
		= C2	.15*	.15*	.19*	.20*	.11*	.09*	.16*	.12*
		= P1	.02	.20*	.04	.27*	.37*	.39*	.40*	.39*
		= P2	.00	.06*	.00*	.21*	.00	.00	.00*	.00*
		= P3	.00	.96*	.00*	1.00*	.00	.68*	.00*	.77*
ACC	Q	T = C1	.01	.09*	.04	.13*	.02	.12*	.03	.19*
		= C2	.02	.08*	.05	.12*	.02	.14*	.03	.23*
		= P1	.52*	.00	.61*	.00*	.83*	.00	.88*	.00*
		= P2	.85*	.00	.89*	.00*	1.00*	.00	1.00*	.00*
		= P3	.00	.40*	.00*	.50*	.01	.03	.02*	.09*

* The estimated probability of type I error differs significantly from its nominal value (.025 or .05).

Approximate standard errors for these critical values can be obtained from the formula $\sigma = \sqrt{\frac{P(1-P)}{n}}$, where n = 100 and p is the size of the test. For p = .05, σ = .021. For p = .025, σ = .016.

FIGURE 1:

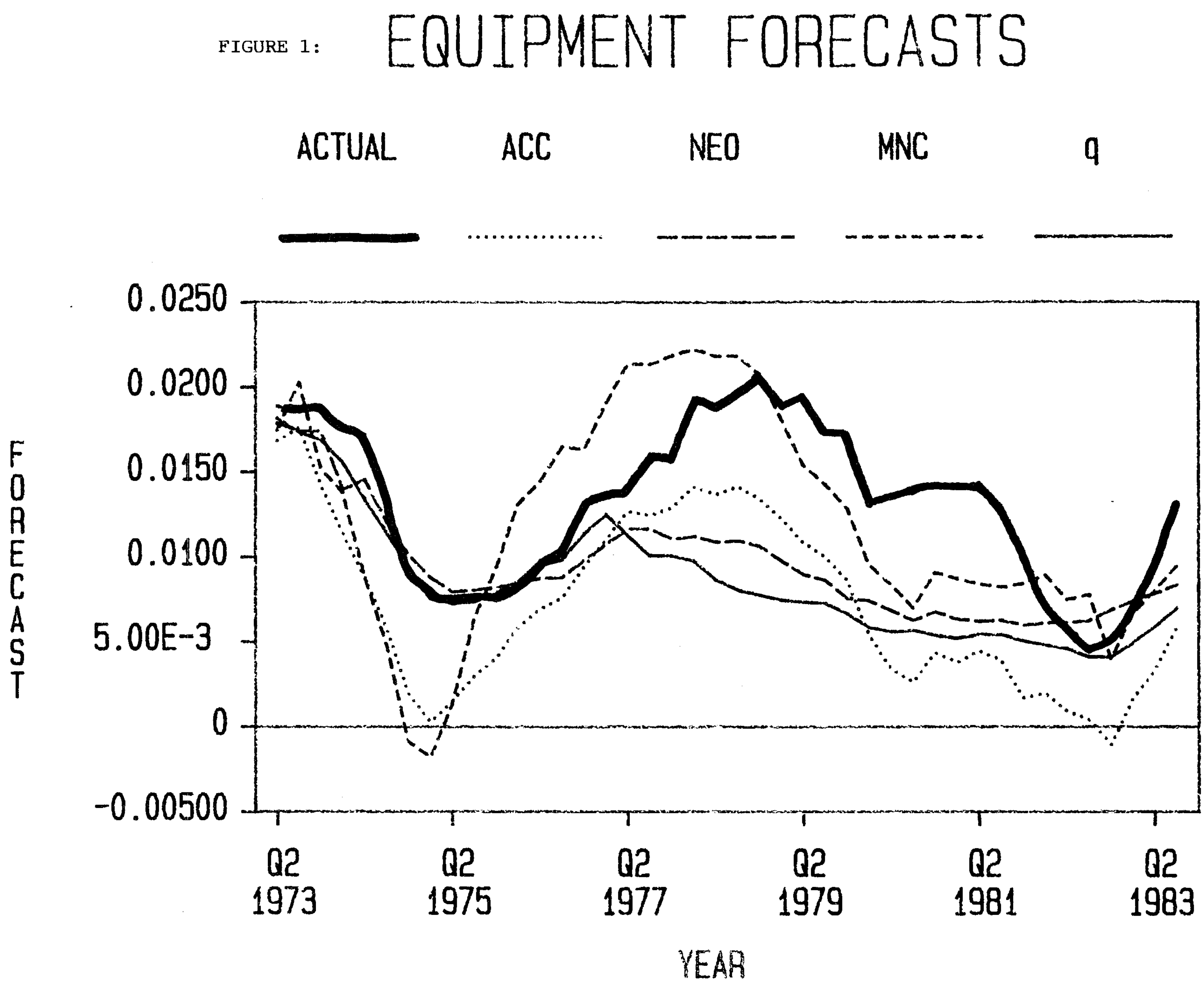
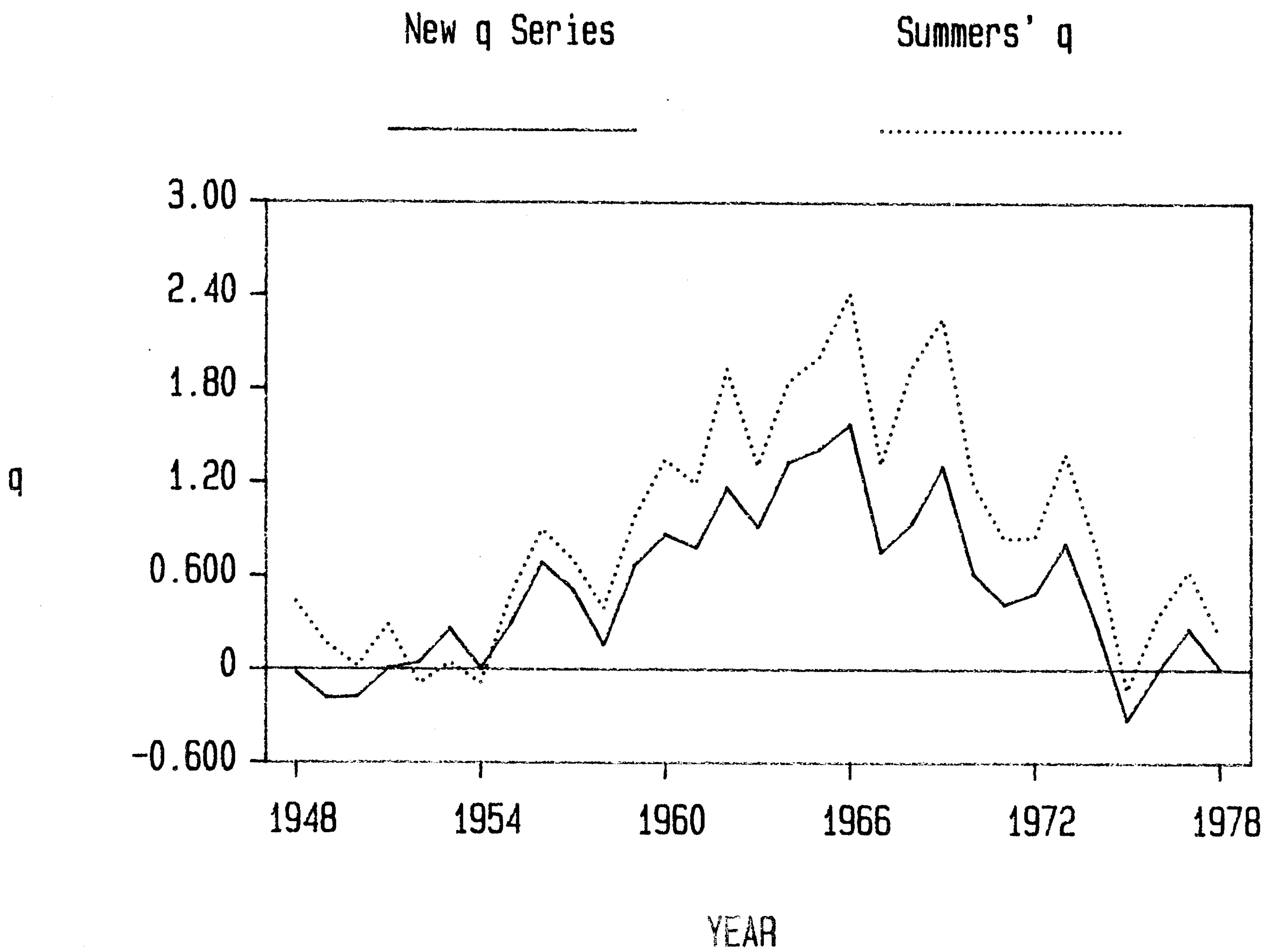

EQUIPMENT FORECASTS
ACTUAL
ACC
NEO
MNC
q
0.0250
0.0200
0.0150
0.0100
5.00E-3
0
-0.00500
FORECAST
Q2 1973
Q2 1975
Q2 1977
Q2 1979
Q2 1981
Q2 1983
YEAR

FIGURE 2: q SERIES



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