TESTING INEQUALITY RESTRICTIONS IMPLIED BY CONDITIONAL ASSET PRICING MODELS

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Abstract

This paper develops tests of inequality restrictions implied by conditional asset pricing models. The methodology is easy to implement, requires little knowledge of the conditional distribution of asset returns, and is valid under fairly weak assumptions. We provide several examples of asset pricing models in which inequality constraints play a central role, documenting results which are in contrast to recent empirical work in these areas. Specifically, we document reliable evidence that (i) the ex-ante risk premium is not always positive, (ii) the size effect does not hold conditionally, and (iii) term premiums may be monotonically increasing in maturity.

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1 Introduction

Financial asset pricing theory for the most part relates conditional expected returns to observable economic variables. These asset pricing models often lead to restrictions on the signs of unobservable parameters but not their magnitude. For example, the conditional version of the capital asset pricing model (CAPM), as well as less restrictive models, implies that the conditional expected return on the market should exceed the risk-free rate. In addition, seasonal models of stock returns suggest higher conditional returns on a particular day or month. With respect to term structure models, the liquidity preference hypothesis implies that term premiums should be increasing in remaining time to maturity. While a number of these and other implications from financial models have widespread support, very little in the form of direct testing has taken place.

There are two main reasons for this lack of hypothesis testing. First, these models imply multiple inequality restrictions on the parameters to be estimated. For example, with respect to the liquidity preference hypothesis, the (j)-month term premium exceeds the $(j-1)$-month term premium for all values of the horizon length j. Only recently has an econometric literature developed for testing these types of restrictions.[1] The statistical tests developed in this literature, although different in terms of implementation, are conceptually quite similar to those found in the more standard framework in which the econometrician tests equality restrictions.

A second more acute problem is that conditional expected returns are unobservable to the econometrician. This is unfortunate because most of the theories are expressed in terms of a conditioning set of information available to economic agents (a subset of which is available to the econometrician). However, for many applications, the model can be reduced to an unconditional specification, which can be estimated fairly easily using sample data. For example, Richardson, Richardson and Smith (1992) test the liquidity preference hypothesis by estimating unconditional term premiums and simultaneously checking for monotonicity. The theory's implications are, however, much stronger in that the monotonicity condition depends on the current and past states of the economy which are ignored in unconditional tests. The problem, therefore, is that these unconditional tests lack power (i.e. they throw away available information) and have little practical value in terms of the financial model's implications with respect to current information.

[1]See Yancey, Judge and Bock (1981), Gourieroux, Holly and Monfort (1982), Kodde and Palm (1986) and Wolak (1989a,1989b,1991) for recent examples of this literature and for references to earlier work in this area. For a recent application in finance, see Richardson, Richardson and Smith (1992).

With respect to testing equality restrictions, there is a substantive literature which does use conditional moments to test various asset pricing model models. As a representative sample of this literature, Hansen and Singleton (1982,1983) test the consumption based asset pricing model, Gibbons and Ferson (1985) investigate the CAPM, and Keim and Stambaugh (1986) look at whether expected returns time-vary, and the list goes on. Although the methodology in these papers is not particularly suited to testing asset pricing models in the presence of inequality restrictions, the conceptual approach behind using past information can be used in the inequality testing framework. In particular, the econometrician can condition on particular events (e.g. certain levels of instrumental variables) and iterate down to a testable unconditional form.

This paper combines the multivariate inequality constraints literature and conditional asset pricing econometric techniques to formulate a test of inequality restrictions implied by conditional asset pricing models. This approach has several advantages. First, the econometric theory requires only weak restrictions on the underlying processes. Second, the econometrician does not need to know how conditional moments evolve through time. Third, there are potentially widespread applications of this approach to the financial econometrics literature.

As such, we provide several examples of pricing models in which inequality restrictions play a central role. The first example looks at whether the ex-ante risk premium is always nonnegative. While we can derive theoretical models which produce negative risk premiums, it is an empirical question whether this is actually the case in the data. In the second example, the liquidity preference hypothesis is studied. Of interest here, we provide a formal test of this hypothesis by conditioning on information currently used in term structure tests. The third example provides a formal test of the size effect (i.e. the inverse relation between firm size and expected returns) which does not assume any functional form for expected returns on the size portfolios.

The paper is organized as follows. Section 2 describes the test methodology and corresponding test statistics. Section 3 considers some examples of restrictions implied by conditional asset pricing models. As an illustration of the techniques, these models are tested using the inequality testing procedure described in Section 2. Section 4 concludes with a discussion of some possible directions of future research.

2 Test Methodology

Consider a model which implies the following restriction:

$$E_t[X_{t+1}] \geq \mu_t, \tag{1}$$

where X_{t+1} and μ_t are observable random variables.[2]

If the model in (1) were an equality, i.e. $E_t[X_{t+1}] = \mu_t$, the standard approach to testing this model would be to regress $X_{t+1} - \mu_t$ on some predetermined variables (known at time t) and test whether the coefficients are nonzero. In terms of testing the inequality restriction in (1), however, it is unclear how this approach can generate an ex-ante test.

For example, let X_{t+1} be the stock return from time t to $t+1$ and μ_t the risk-free rate earned between time t and $t+1$. Suppose we wish to test the null hypothesis that the expected risk-premium is a constant, i.e. $E_t[X_{t+1}] = \mu_t + k$. As mentioned above, one approach would be to regress the risk premium on past variables and test whether the risk premium is in fact predictable. Documenting time variation in the premium, however, does not necessarily imply that it will be nonnegative. Ex-post analysis of the estimated ex-ante risk premium will generally be inconclusive regarding the inequality restriction in model (1). This is discussed in more detail later in the paper.

Below, we deduce testable restrictions from model (1) which do not rely on a model for conditional expected returns. Moreover, these restrictions, as we shall see, lead to easy-to-calculate test statistics.

2.1 Inequality Restrictions from Conditional Models

The conditional model in (1) implies that the difference between the conditional expectation of X_{t+1} and the currently observed μ_t is greater than zero. That is,

$$E_t[X_{t+1} - \mu_t] \equiv D_t \geq 0. \tag{2}$$

where D_t is defined as this difference.

In the example regarding the risk-premium, equation (2) simply states that the ex-ante risk premium is positive. Model (2) is a fairly general restriction. The premium may depend on a variety of state variables in agents' information sets and, therefore, be

[2]More complicated frameworks are discussed in Section 4.

predictable. Further, realizations of the market return may fall below the risk-free rate, but agent's expectations never do under the null.

With respect to deriving testable restrictions from model (2), note that the econometrician has much less information available to him than do the agents. Let us restrict ourselves to information available to the econometrician at time t which is also nonnegative for all t (denote z_t^+) for reasons which will become clear shortly. For example, in the risk-premium example, one feasible instrument in the above class is past squared values of the risk-premium $(X_t - \mu_{t-1})^2$. This instrument is strictly positive and, in all probability, provides some information about the ex-ante risk-premium if it in fact time-varies. In general, the econometrician's choice of instruments should be directed by the underlying economic theory and intuition of the model, and by interesting alternative models.

Because the set of instruments z_t^+ are nonnegative, multiplying both sides of equation (2) will not change the sign. Therefore,

$$E_t[(X_{t+1} - \mu_t) \otimes z_t^+] = D_t \otimes z_t^+ \geq 0 \tag{3}$$

Rearranging (3), and applying the law of iterated expectations,

$$E[(X_{t+1} - \mu_t) \otimes z_t^+ - \theta_{Dz^+}] = 0 \tag{4}$$

$$\text{where} \qquad \theta_{Dz^+} = E[D_t \otimes z_t^+] \geq 0. \tag{5}$$

Equation (4) provides a set of moment conditions in which the econometrician needs to estimate the vector of parameters, θ_{Dz^+}. It does not matter that D_t is unobservable because the vector of observables (X_t, μ_t, z_t^+) are enough to identify θ_{Dz^+}. Of particular interest, under the null hypothesis given in model (1), $\theta_{Dz^+} \geq 0$. Therefore, model (1) implies inequality restrictions on the sample means of the vector $(X_{t+1} - \mu_t) \otimes z_t^+$. Rejection of this restriction necessarily means rejection of model (1).

This approach has several attractive features. First, the econometrician does not require a model for conditional expectations. This is especially important because, for many asset pricing theories, conditional expectations are not explicitly modeled.[3] As it

[3]Suppose conditional expectations were modeled; for example, it is common practice to assume that returns are linear in the information set of variables. In this case, the method is to regress the variable $X_{t+1} - \mu_t$ on a set of predetermined variables — the fitted values being the estimate of the conditional values. Similar to equations (4) and (5), what are the corresponding restrictions in this linear expectations framework? Specifically, the inequality restriction can be tested using the following equations:

$$E[((X_{t+1} - \mu_t) - \beta' z_t) \otimes z_t] = 0$$
$$E[(\beta' z_t) \otimes z_t^+ - \theta_{Dz^+}] = 0$$

turns out, all that needs to be satisfied are some stationarity and ergodicity assumptions regarding the observable variables. Second, the econometrician can often point to the interesting instrumental variables, but may not know how they enter the model. Here, there is no assumed functional form so this is not a potential problem. Third, the restrictions given in (5) can be tested using the technology developed recently in the inequality testing literature. Of particular importance, these restrictions can be tested jointly and therefore will take into account any correlation across the mean estimators, $\hat{\theta}_{Dz^+}$. For example, in evaluating the significance of the estimators, the relevant factors are not only the magnitudes of the estimates but also whether these magnitudes are consistent with the covariance matrix of $\hat{\theta}_{Dz^+}$. The interpretation the econometrician can give is very similar to that of individual t-tests versus an F-test.

2.2 Test Statistic

In this section, we describe a statistic for testing inequality restrictions implied by the null model in (1), i.e. $\theta_{Dz^+} \geq 0 \quad \forall \quad z^+$. With respect to testing inequality constraints, our description most closely follows Wolak (1989a) and Kodde and Palm (1986). Because these papers provide an excellent description of the inequality testing methodology, we provide just a brief discussion as it applies to our problem.

In particular, suppose we have T observations on $X_{t+1} - \mu_t$ and the N-vector z_t^+. Assume these random variables are stationary and ergodic, with finite variances. Let the sample moment vector's, $\frac{1}{T}\sum_{t=1}^{T}[(X_{t+1} - \mu_t) \otimes z_t^+]$, variance-covariance matrix be defined as Ω. This matrix can take quite general forms. In brief, the matrix can account for crosscorrelation, autocovariances or heteroskedasticity in the series.

The restriction given in (4) and (5) can be written as a system of N-moment conditions:

$$E[(X_{t+1} - \mu_t)z_{1t}^+] = \theta_{Dz_1^+}$$

$$\vdots$$

$$E[(X_{t+1} - \mu_t)z_{Nt}^+] = \theta_{Dz_N^+}$$

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where $\theta_{Dz^+} = E[D_t \otimes z_t^+] \geq 0$

$z_t =$ set of predetermined variables chosen by econometrician.

Two problems arise here. First, this procedure works if conditional expectations are linear in the set of variables z_t *chosen* by the econometrician. In most frameworks, there is no reason to believe this is the case. Second, there are parameters β not specified by the null inequality restriction. This can lead to problems in deriving globally exact test statistics. The tests, however, can be viewed as local around the true parameters β with a corresponding large sample justification (see Wolak (1989a,1991)).

$$H_0 : \theta_{Dz_i^+} \geq 0 \quad \forall i = 1, \ldots, N \tag{6}$$

versus

$$H_A : \theta_{Dz_i^+} \in R^N.$$

With respect to testing the hypothesis in (6), the first step is to estimate the sample means of the product of the observable variables.[4] In particular,

$$\hat{\theta}_{Dz_i^+} = \frac{1}{T} \sum_{t=1}^{T} [(X_{t+1} - \mu_t) z_{it}^+] \qquad \forall i = 1, \ldots, N.$$

There is no restriction on the sign of these estimates; that is, they may be negative because either the null is false or sampling error is present. Of importance to the distributional results to follow, the vector $\hat{\theta}_{Dz^+}$ is asymptotically normal with mean θ_{Dz^+} and variance-covariance matrix Ω. The econometrician does not need to know Ω; all that is required is a consistent estimate, denote $\hat{\Omega}$.[5]

Under the null restriction in (6), the parameter estimates must be nonnegative. Following Perlman (1969) and Wolak (1989a), we can derive estimates under the restriction, by minimizing deviations from the unrestricted model;

$$\min_{\theta_{Dz^+}} \quad (\hat{\theta}_{Dz^+} - \theta_{Dz^+})' \hat{\Omega}^{-1} (\hat{\theta}_{Dz^+} - \theta_{Dz^+}),$$

$$\text{subject to } \theta_{Dz^+} \geq 0.$$

Let $\hat{\theta}^R_{Dz^+}$ be the solution to this quadratic program.

Within this framework, a natural statistic for testing the hypothesis in (6) is to test how close the restricted estimates $\hat{\theta}^R_{Dz^+}$ are to the unrestricted estimates $\hat{\theta}_{Dz^+}$. Under the null, the difference should be small. In particular, the test statistic is given by

$$W \equiv T(\hat{\theta}^R_{Dz^+} - \hat{\theta}_{Dz^+})' \hat{\Omega}^{-1} (\hat{\theta}^R_{Dz^+} - \hat{\theta}_{Dz^+}). \tag{7}$$

The final step is to calculate W's asymptotic distribution. The econometrician can then calculate W for his particular estimation problem and then evaluate its value at the

[4] In this paper, we consider the analog to the Wald statistic for testing equality restrictions. Similar to the equality restrictions case, there are general results relating Wald, Likelihood Ratio and Lagrange Multiplier tests. For the weak distributional assumptions imposed in this paper, the Wald statistic is the most straightforward to compute.

[5] See Perlman (1969) or Wolak (1989a) for relevant proofs. Note that $\hat{\Omega}$ can be estimated using various techniques such as White (1980), Hansen (1982), and Newey and West (1987), among others.

appropriate level of significance. Note that the null hypothesis no longer implies a particular value for θ_{Dz+}. Nevertheless, using results in Perlman (1969), it is possible to calculate the distribution of the W statistic for the least favorable value of the null hypothesis and thus of any size test.[6] Unlike the standard statistics under equality constraints (such as the Wald and likelihood ratio statistic), these statistics will no longer have an asymptotic chi-squared distribution in the presence of inequality restrictions. Instead, the statistic is now distributed as a weighted sum of chi-squared variables with different degrees of freedom. Specifically, the asymptotic distribution of W is given by

$$\sum_{k=0}^{N} Pr[\chi_k^2 \geq c] \; w\left(N, N-k, \frac{\hat{\Omega}}{T}\right), \tag{8}$$

where $c \in R^+$ is the critical value for a given size and the weight $w\left(N, N-k, \frac{\hat{\Omega}}{T}\right)$ is the probability that $\hat{\theta}^R_{Dz+}$ has exactly $N-k$ positive elements.

To gain some intuition for this result, first note that the estimators $\hat{\theta}_{Dz+}$ have an asymptotic multivariate normal distribution. We are interested in testing the hypothesis $\theta_{Dz+} \geq 0$ versus the unrestricted alternative. For the univariate one-sided test, it is well known that the probability that the statistic exceeds some critical value is simply $\frac{1}{2}Prob(\chi_1^2 \geq c)$, where c is the critical value. This result comes from using the fact that $\hat{\theta}_{Dz+}$ is normally distributed and that zero is the least favorable value of the null. The bivariate one-sided test follows similarly; in particular, the probability that the statistic exceeds some critical value is the sum of two components: (i) the probability that only one estimate is negative times the probability a χ_1^2 exceeds the critical value, plus (ii) the probability that both estimates are negative times the probability a χ_2^2 exceeds the value. For any N-dimensional multivariate one-sided test, the probability that the W statistic exceeds some critical value is the sum of N components, each reflecting the probability of finding k estimates outside the null region times the probability a χ_k^2 is greater than the critical level. In the presence of cross-correlation between the estimators, calculating these probabilities can be nontrivial for large numbers of restrictions. Since these methods

[6]Note that, in general, Ω may depend upon the parameter vector θ_{Dz+}. As shown by example in Wolak (1991), when Ω depends on the parameter vector θ_{Dz+} even if $\theta_{Dz+} = 0$ is the unique value which satisfies all of the inequalities with equality (as in equation (6)), the vertex of the positive orthant may not be the least favorable value of θ_{Dz+} for the purposes of constructing the asymptotically exact critical value for the inequality constraints hypothesis test. It is important to point out that $\Omega's$ dependence on θ_{Dz+} (if it in fact exists) does not necessarily pose a problem. Without specifying the estimation further, however, it is not possible to determine the problem cases. In this instance, it may be appropriate to treat the inequality hypothesis test as local to the point $\theta_{Dz+} = 0$ in the manner described in Wolak (1989a).

are discussed in detail in the literature, Appendix A provides a brief description of the various techniques for calculating the weights on the χ^2 distributions.

2.3 Further Discussion

The inequality restrictions in (6) are generally weaker than the more standard equality restrictions usually tested in finance. This point is not a valid criticism, however, of the testing approach described in this paper. If the model imposes inequality restrictions, and we wish to test these implications of the model, then we must rely on an inequality testing framework. Alternative inequality-based testing methods such as Bonferroni-type procedures are inferior. In particular, the correlation across the estimators (and therefore the individual statistics) is ignored in the Bonferroni framework (see Wolak (1989b) for a relevant discussion).

To the extent that we perform an inequality restrictions test and wish to determine where the true parameters θ lie, Gourieroux, Holly and Monfort (1982) derive tests for the case of testing a null $\theta = 0$ versus the alternative $\theta \geq 0$. This second test is often applied when the inequality null is accepted and helps determine the source of the acceptance. Resorting back to an equality testing framework here is not appropriate. In terms of comparing the power of the Gourieoux et. al. one-sided test of equality versus the usual two-sided tests, the power is greater with the one-sided test (see Yancey, Bohrer and Judge (1982)).

Finally, note that the test statistic depends upon our choice of instruments which is at the obvious discretion of the econometrician. This is *true of all* conditional asset pricing tests involving instrumental variables — it is not unique to this paper. In the context of current empirical work in finance, our procedure shows how to apply existing econometric method to testing inequality restrictions implied by conditional asset pricing models. Just as with tests for equality, therefore, the choice of the best instruments ultimately depends on the underlying economic model. With this in mind, we hope in future research to consider how the "quality" and number of instruments can affect the size and power of the test statistic in (7).

3 Examples

3.1 Is the Ex Ante Risk Premium Positive?

Is the expected return on the market greater than the risk-free rate? Even though this result is central to many of our asset pricing theories, there has been almost no direct testing of this hypothesis. In fact, in a prelude to much of the time-varying asset return literature, Merton (1980) states

> *In estimating models of the expected market return, the non-negativity restriction of the expected excess return should be explicitly included as part of the specification.*

From a theoretical standpoint, Merton (1982) provides justification for this result in both static and dynamic settings.[7]

One of the best known models, the conditional version of the CAPM, implies this nonnegativity restriction although for the most part it has been ignored in testing. For example, Gibbons and Ferson (1985) test implications derived from the linear relation between expected returns and their market beta.[8] Using international data, and in a slightly expanded framework, Harvey (1991) tests the conditional CAPM and again concentrates on the linearity relation. The problems with these and similar approaches are that they require some knowledge (which the econometrician does not have) about the true time-varying movements in expected returns, covariances and variances of the underlying assets.

Outside the CAPM framework, under what circumstance should we accept the fact that the nominal risk premium on holding risky assets can be negative? It is well known that in the absence of arbitrage there exists a nonnegative operator n_{t+1} (the nominal marginal rate of substitution) such that $\mathrm{E}_t[n_{t+1}R_{it+1}] = 1$ for all the returns on assets in the economy. Now $\mathrm{cov}_t(n_{t+1}, R_{mt+1} - R_{ft}) = \mathrm{cov}_t(n_{t+1}, R_{mt+1}) = \mathrm{E}_t[n_{t+1}R_{mt+1}] - \mathrm{E}_t[n_{t+1}]\mathrm{E}_t[R_{mt+1}] = 1 - \mathrm{E}_t[R_{mt+1}]/R_{ft}$, where R_{mt+1} and R_{ft} are the the returns from t to t+1 on the market and the risk-free asset, respectively. The ex ante risk premium is negative therefore if and only if the conditional covariance between n_{t+1} and $R_{mt+1} - R_{ft}$ is positive. While it is theoretically possible to obtain this condition in some states of the world, there is some debate regarding its plausibility using reasonable parameter values

[7]For a more extensive discussion of several sufficient equilibrium conditions, also see Merton (1990).
[8]For additional examples of similar tests, see Campbell (1987) and Ferson, Foerster and Keim (1991).

(see Tauchen and Hussey (1991) for a discussion of this in the context of Lucas' (1978) asset pricing model).

Nevertheless, it remains an empirical question as to whether this occurs in the actual data. Given the extensive literature on the risk premium, it may seem surprising then that no formal tests of the nonnegativity restriction have taken place. Because the restriction implies a set of conditional inequality constraints, however, the test requires the methodology introduced in this paper.

The restriction on the risk-premium can be written as

$$E_t[R_{mt+1}] \geq R_{ft}.$$

Substituting the above model into the framework in Section 2, yields

$$E[(R_{mt+1} - R_{ft}) \otimes z_t^+ - \theta_{Dz^+}] = 0, \tag{9}$$

where a nonnegative risk-premium implies $\theta_{Dz^+} \geq 0$. This restriction can then be tested using the methodology introduced in Section 2.2.

Of particular interest to the conditional CAPM, the above test represents a possible solution to the critique most commonly associated with Roll (1976). In standard tests of the CAPM, identification of the market portfolio is necessary for an appropriate test. That is, the linear relation holds mathematically as an identity. The only empirical question is which portfolios satisfy this relation, and, with respect to the CAPM, is one of these portfolios the market portfolio? Latent variable approaches to the Roll critique which avoid identifying the market portfolio, such as Gibbons and Ferson (1985), have been largely unsuccessful (see Wheatley (1990)). In a related context, Kandel and Stambaugh (1987) and Shanken (1987) do manage to place bounds on the correlation of the market proxy and the unobservable market portfolio. However, these bounds are unconditional and are often quite restrictive.

In terms of the nonnegativity of the risk premium, the above inequality testing framework can address the issue underlying the Roll critique. Suppose the conditional CAPM is true, then the return on the market proxy R_{pt} must satisfy the following relation:

$$E_t[(R_{pt+1} - R_{ft})] = \frac{\mathrm{cov}_t[R_{pt+1}, R_{mt+1}]}{\mathrm{var}_t[R_{mt+1}]} E_t[(R_{mt+1} - R_{ft})].$$

Under the null, the true ex ante market risk premium must be positive. Therefore, if the conditional covariance between our market proxy and the unobservable market portfolio is positive, the ex ante risk-premium on our proxy must also be positive. Hence, rejection of this restriction necessarily implies a rejection of the CAPM. Since the proxy is generally a well-diversified portfolio of assets, the restriction that it must have positive correlation with the market seems weak. It is in stark contrast to the high correlation normally required from CAPM tests (see Kandel and Stambaugh (1987)). The drawback from this approach is that the nonnegativity restriction is not sufficient for the CAPM to be true. As mentioned earlier, this restriction may be valid in much less restrictive environments.

One area of the literature which the tests also address is the equity premium puzzle, first described by Mehra and Prescott (1982). In particular, given the sample mean of the risk premium over the 1890-1980 period, financial asset pricing models will have difficulty explaining the high expected return on equities relative to riskless bonds. In contrast, a recent paper by Siegel (1991) suggests that in an earlier period (1802-1890) the equity premium is much smaller than it was in the twentieth century. Note that for the choice of instruments, $z_t^+ = 1$, the problem reduces to estimation of the unconditional mean of the risk-premium. This is an example of equation (9) with other conditioning information omitted. Below, using two centuries of data, we use conditioning information to test whether the ex ante risk premium is positive.

3.1.1 Tests

For the nonnegativity of the risk-premium, Merton (1980) suggests some reasonable instruments. As Merton (1980) points out, one estimate of the expected return might be the sample average of realized returns on a market index. While this exceeds the average T-bill rate (i.e. treating the T-bill rate as the risk-free) over most sample periods, at any given time this estimate may be less than the T-bill rate when the T-bill rate is high. In this case, the sample average will be a poor estimate under the null. The idea is that rational agents incorporate high T-bill rates (either through high ex-ante real rates or high expected inflation) into their estimation of the market's expected return. One choice of instruments, therefore, is times when the risk-free rate is high. Since this must be a relative condition, we look at times when the T-bill rate exceeds rates on longer term bonds. Hence, we consider the $\max[0, R_{ft} - R_{lt}]$, where R_{lt} is the yield on longer term bonds. To the extent premiums are time-varying, choosing times with downward sloping yield curves has particular advantages. Expectations about inflation aside, it can capture anticipation of a recession and thus lower future real rates. This will impact the economy and therefore expected returns.

A second important factor, as pointed out by Merton (1980), is the level of risk associated with the market. Merton (1980) argues that it is reasonable (although not necessary) to expect that the return on the market will be an increasing function of risk. Merton uses a common measure of market risk, the variance of the market return, and establishes some evidence of this relation. In addition to this evidence, French, Schwert and Stambaugh (1987) show that the risk premium is positively related to predictable volatility although this is not universally accepted (see Pagan and Hong (1991)). Nevertheless, as a potential instrument, we choose the past level of the risk-premium squared. If this is positively correlated with future levels, then high past values may signal riskier times. To maximize power a priori, however, we wish to put weight on periods with relatively small risk, i.e. when volatility and therefore expected returns are low. Hence, we choose the inverse of the past squared risk-premium, i.e. $\frac{1}{(R_{mt}-R_{ft-1})^2}$.

Table 1 reports tests of restriction (9) using annual data (in percentage terms) from 1802-1990 taken from Siegel (1991). Siegel (1991) provides data on annual stock returns on a broad index, the short-term annual rate of interest, and the rate on longer term government bonds.[9] There are several reasons for using this data series. First, given the rarity of downward sloping yield curves, the long time-series hopefully allows us enough periods to extract information from the instruments. Second, given existing evidence in post World War II data for the relation between stock returns and term structure spreads, using a long series helps avoid data snooping biases. Third, given the paucity of cross-sectional asset returns data over this period, our procedure is also the only viable approach to testing the conditional CAPM over this long term. Fourth, we can extend Siegel's (1991) equity risk premium application to a conditional asset pricing framework.

For the overall period, the joint test statistic W equals 4.630. Since this value falls between the 5% upper and lower bounds, we can use the Kudo (1963) analytical weights to evaluate its significance.[10] In particular, the statistic lies above the 5% cut-off level (i.e. 3.829) — its asymptotic p-value is .034. We therefore reject the null, that the conditional

[9] Siegel (1991) collects the data from various sources. In brief, the stock return data is taken from Schwert (1990) and measures a broad based index for most of the period (the pre-civil war period, however, represents mostly financial and railroad firms). Data on long-term U.S. government bonds was available throughout much of the nineteenth century; Siegel (1991) collected bonds which had maturities as close to twenty years as possible. With respect to the short-term rate, due to the scarcity of available data Siegel (1991) constructs the series from various sources (e.g. UK short-term data under the gold standard and commercial paper rates in the U.S.). He finds that these constructed rates are close to available actual risk-free rates during this period.

[10] In later subsections, when we have more restrictions, we need to use the simulation method described in the appendix. With respect to the routine, the multivariate normal is simulated using the IMSL subroutine DRNMVN. In calculating the weights, we perform 1000 replications.

risk premium is always nonnegative. At first glance, this result is surprising. It suggests there are times in which we should expect the market expected return to be less than the risk-free rate.

To understand this result better, consider the values of the unconstrained means associated with the term structure slope instrument, $\hat{\theta}_{Dz_1^+} = -1.799$, and associated with the risk premium instrument, $\hat{\theta}_{Dz_2^+} = 13.939$. Combined with their standard errors, 0.844 and 20.286 respectively, the evidence against the null is related primarily to the term structure instrument. That is, the risk premium tends to be negative, according to this test, when the term structure is downward sloping. This rejection is fairly robust, as indicated by the results when the sample period is broken down into two subperiods: 1802 to 1895, and 1896 to 1989. At the 5% level we reject only for the second subperiod, but the statistics (3.359 and 4.072 respectively) and their P-levels (.066 and .042) provide some confidence in the robustness of the result.[11]

One possible explanation for this result may be due to data errors, especially in the pre civil war period.[12] However, the above mentioned robustness to subperiods suggests this may not be the case. Keeping in mind the imprecise nature of the data, this rejection still presents a challenge to existing asset pricing models. Even if our proxy is a poor measure of the overall market index, it should be positively correlated with the market — thus, rejecting the conditional version of the CAPM. Why should agents view holding the market, a nominally risky asset, advantageous to holding a nominally riskless asset, and hence charge a *negative* risk premium? As mentioned earlier, one implication of the result is that the conditional covariance between the marginal rate of substitution and the (excess) return on the market is positive during certain periods, namely ones in which the term structure is downward sloping.

3.2 Are Term Premiums Monotonic?

The liquidity preference hypothesis implies that the expected return on Treasury securities increases monotonically with remaining time to maturity. Direct tests of this theory have been performed by Fama (1984), McCulloch (1987) and Richardson, Richardson and

[11] Please note that during the 1802-1895 and 1896-1989 subperiods respectively, there are 54 and 21 years in which the term structure is downward sloping.

[12] At least with the stock return data, however, we might expect the bias to actually go in the opposite direction. The pre civil war stock index has a survivorship bias to the extent it does not contain poorly performing stocks during this period.

Smith (1992). While there is some disagreement concerning the reliability of the data in the 1964-1972 period, the evidence seems to suggest that expected returns are monotonic. For example, Richardson, Richardson and Smith (1992) find that, when one correctly accounts for monotonicity using inequality constraints, there is little evidence against the liquidity preference theory. This evidence, however, depends on unconditional tests of the theory. The tests, therefore, may be expected to have low power. More interesting is the question of whether expected returns are monotonic conditional on all available information.

There is evidence to suggest that expected returns on bills are not constant.[13] Fama (1986) documents time-varying movements in term premiums which depend on the business cycle. With respect to the liquidity preference theory, Fama (1986) states

> *term premiums are generally interpreted as rewards for risk. In this view, the changes from upward sloping term structures of expected returns during good times to humped and inverted term structures of expected returns during recessions imply that the ordering of risks and rewards across maturities changes with the business cycle and is not always monotonic.*

However, it is difficult to interpret Fama's (1986) findings in terms of statistical significance. First, although the individual mean estimates of the premiums suggest expected returns are not monotonically increasing, there have been no joint tests of the implied inequality restriction. Given the high correlation across the premiums, the need for a joint test seems especially clear.[14] Second, Fama (1986) uses term structure shapes as his conditioning variable for the state of the economy. Given that these shapes may be correlated from month to month, the variance-covariance matrix of the estimators needs to be adjusted for serial correlation in the series. The problem of adjusting for serial correlation fits directly into the testing framework described in Section 2.2.

In a related setting, Stambaugh (1988) adds to Fama's (1986) evidence by showing that a two latent variable model of expected returns on T-bills produces similar results. In particular, he shows that expected returns exhibit variation with business cycles which is non-monotonic. However, while this paper certainly suggests non-monotonicity of term premiums, no direct tests of this hypothesis have taken place. As mentioned above, this

[13] For a review of this literature and other term structure related issues, see Shiller (1990).

[14] Richardson, Richardson and Smith (1992) do perform such a joint test; however, their approach is based on unconditional tests. Hence, in the framework of Section 2, their choice of instruments is confined to $z_t^+ = 1$ for all t.

requires tests of multiple inequality constraints and knowledge of the model for conditional expected returns (i.e. without the procedure in Section 2).

In particular, to coincide with Fama (1986), define $H_{\tau_{t,t+1}}$ as the continuously compounded return from t to $t+1$ on a bill with maturity τ at date t. The term premium in the return on a bill with maturity τ is defined as

$$P_{\tau_{t,t+1}} \equiv E_t[H_{\tau_{t,t+1}} - H_{1_{t,t+1}}],$$

where $P_{(N+1)_{t+1}} \geq P_{N_{t+1}} \geq \cdots \geq P_{2_{t+1}}$ under the null model.

3.2.1 Tests

We perform two tests of this model: (i) a generalization of the Richardson, Richardson and Smith (1992) test to conditional moments, and (ii) a replication of Fama's (1986) analysis. For case (i), we consider moment conditions implied by various maturities, $E[(H_{\tau_{t,t+1}} - H_{\tau-1_{t,t+1}}) \otimes z^+ - \theta_{\tau,Dz^+}] = 0$, with the restriction $\theta_{\tau,Dz^+} \geq 0$ under the liquidity preference hypothesis. With respect to case (ii), Fama (1986) looks at four holding period returns on bills — $B1/S0$, $B3/S1$, $B6/S3$ and $B12/S6$, where $B\tau/S\gamma$ means buying a τ-period bill and then selling it with γ periods to maturity. Under the null, the conditional expectations of $B\tau/S\gamma$ should be monotonic with maturity (i.e. $B12/S6 \geq B6/S3 \geq B3/S1 \geq B1/S0$). The estimation uses data from the Fama files for one to eleven month bills (in yearly percent) over the period 1974-1990.[15]

With respect to the available instruments, there are several natural choices. First, to the extent that there is some controversy regarding the unconditional term premium over the sample, we choose a constant instrument (e.g. $z_t^+ = 1$) to include this case in our joint test. Second, to coincide with Fama (1986), we consider times in which the yield curve is nonmonotonic. This should capture business cycle effects to the extent downward sloping yield curves may indicate that the economy is heading towards a recession (inflation aside). As an indicator of this variable, we choose the spread between the one-month and six-month bill at time t, $\max[0, H_{1_{t,t+1}} - H_{6_{t,t+6}}]$, as an instrumental variable. This variable puts its greatest weight on situations in which the term structure is sharply declining in the short term. The last, and a related instrument, is $\max[0, H_{6_{t,t+6}} - H_{11_{t,t+11}}]$, which

[15]We did not include the bill with twelve months to maturity for two reasons. First, there are a substantial number of missing observations at these maturities. Second, the twelve month bill is actually defined in the data to be bills of at least 11 months and ten days. As such, we considered its definition too unreliable for our analysis.

captures nonmonotonicity at longer horizons. This instrument, for example, captures humped yield curves as well as downward sloping curves.

Tables 2A reports results for a conditional test of the monotonicity of the term premium based on case (i). With ten term premiums and three instruments, the system imposes twenty-seven inequality restrictions. Note that the variance-covariance matrix of the estimators takes into account the correlation across the sample moment estimators, and also adjusts for serial correlation due to both time-varying behavior in the premiums and autocorrelation in the term structure shapes through time. (The variance-covariance matrix is estimated using the Newey and West (1987) procedure with eleven lags). The test statistic for a test of conditional monotonicity in the term premium equals 5.578, which falls between the 5% upper and lower bounds. Using the simulation method, however, its P-value is .812, which is well below the 5% cut-off level (i.e. 26.411). It is interesting to note that the majority of the unconstrained estimators are negative (19 out of 27). Similar to Fama's results for humped and inverted term structure shapes, the restrictions conditioned on the term structure instruments produce many (15 out of 19) of these negative means. While indeed negative, these means, however, provide little statistical evidence against the null hypothesis.

To coincide more closely with Fama (1986), Table 2B provides tests of the restrictions in case (ii). Using the same instruments $z_t^+ = (1, \max[0, H_{1_{t,t+1}} - H_{6_{t,t+6}}], \max[0, H_{6_{t,t+6}} - H_{11_{t,t+11}}])$, we find that four of the nine restrictions are negative. The negative estimators are associated with nonmonotonic yield curves; for example, $E[(B6/S3 - B3/S1) \times \max[0, H_{1_{t,t+1}} - H_{6_{t,t+6}}]$ equals -.03. The multivariate one-sided test statistic, however, is 2.037 which is less than the lower bound. Apparently, even with testing only a few restrictions, there is little evidence against the monotonicity of the term premium.

At the very least, these results show the different types of conclusions which can be reached by using tests for inequality restrictions. In particular, the commonly held belief that the liquidity preference hypothesis is violated once we take into account available information appears to be statistically unreliable. In conclusion, apparent nonmonotonicities in the data are consistent with sampling error. The low significance values suggest that existing stylized facts may need to be reevaluated.

3.3 Is the Size Effect Monotonic?

The size effect implies that the smaller a firm's size is, the larger its expected return. Empirical evidence suggests that in fact expected returns are monotonic in size. For example, Banz (1981) and Reinganum (1981) provide evidence that there has been a monotonic relation between firm size and mean stock returns over the past 60 years. These results, however, rely on unconditional tests and ignore other information available to the econometrician. Furthermore, many of the tests have been performed in a linear framework and therefore have not allowed for a possible nonlinear relation between size and returns.

There have been some investigations of the size effect using conditional moments. Keim and Stambaugh (1986) find that small firms tend to have higher estimated conditional mean returns than do large firms, but provide no formal test. Using a Kalman filter model, Brown, Kleidon and Marsh (1983) find evidence that the size effect, although on average monotonic, is reversed for some periods. Outside the framework of their particular model specification, however, it is less clear how to interpret their results within the inequality constraint framework. Below, we propose tests of the size effect using the methodology outlined in Section 2.

Given the preponderance of evidence for monthly returns, we choose instead to investigate the size effect using weekly data on portfolio returns from 1962-1991.[16] Most empirical work using weekly returns documents patterns in predictability (e.g. Lo and Mackinlay (1988,1990)). This evidence has led many researchers to propose a factor model for short-horizon asset returns (see, for example, Conrad, Kaul and Nimalendran (1992)). Moreover, these models have different implications for returns on size portfolios. It is of some interest, therefore, to test whether the size effect can be reconciled with time-varying expected returns.[17]

In particular, the size effect implies that

$$E_t[R_{i-1,t+1}] \geq E_t[R_{i,t+1}] \qquad \forall i,$$

where $\quad R_{i,t+1} =$ the return on firm size i from t to $t+1$.

[16] Over this period, we form five size portfolios sorted by market value of equity. Note that these portfolios are resorted every quarter.

[17] Since we are conditioning on previous empirical results, we are subject to data snooping. On the other hand, a large part of the literature in finance believes that short-term expected asset returns are not constant and that this cannot be explained by market microstructure biases. We do not take a view on this issue. In the context of this previous literature, we are simply testing its implications for the size effect.

Following Section 2.1, the restriction reduces to

$$E[(R_{i-1,t+1} - R_{i,t+1}) \otimes z_t^+ - \theta_{i,Dz^+}] = 0, \tag{10}$$

where $\theta_{i,Dz^+} \geq 0$ for all i and z^+.

3.3.1 Tests

Two stylized facts emerge from the finance literature regarding the predictability of short-term asset returns: (i) previous market returns are positively correlated with future returns, and (ii) this correlation is asymmetric with respect to returns on size portfolios (Lo and MacKinlay (1990)). Taking these empirical facts as representative of the true description of returns (as most of the current literature in finance does), a natural choice for an instrument is to condition on time-varying expected returns. In particular, we consider $z_t^+ = \max[0, -R_{ew,t}]$, where $R_{ew,t}$ is the previous return on the equal-weighted market index. Therefore we are conditioning on periods in which the previous market return was negative. If this return has information for future size portfolio returns and this information is asymmetric, then the usual ordering in conditional means across size portfolios may no longer be maintained.

Our other choice of instrument is a constant (e.g. 1), so that the restrictions reduce to an unconditional test. At present in the literature, there have been no joint tests of the monotonicity property across the sample means on the size portfolios. While substantial evidence against equality of these means has been documented, the additional evidence that small firms have higher estimated means has not transformed into a test across the size portfolios in general. In addition, in contrast to existing work, this test does not impose a linear relation between size and stock returns.

Table 3 reports tests of the size effect (equation (10)) using the above two instruments. (Note that the mean estimates are multiplied by 1000 in the table). Consistent with existing evidence, expected returns are unconditionally decreasing in size. For example, the difference in expected weekly returns between the smallest and second smallest portfolios is .194%. However, when we condition on previous weeks in which the equally-weighted market index has fallen, the size effect is reversed. In particular, for all five size portfolios, expected returns are now increasing in firm size. In terms of the multivariate inequality test of the size effect, the statistic's value is 34.48 which is well above the 5% upper bound. We can reject the size effect at any reasonable level of significance.

This paper provides some of the first evidence against the size effect holding conditionally. While the results may be due to nonsynchronous trading or some other microstructure bias, existing work suggests that this is not the case (e.g. Lo and MacKinlay (1988)). If the correlation patterns documented in Lo and MacKinlay (1988) and others are in fact *truth*, then the evidence here shows that their implications for the size effect are profound. Specifically, theories which treat size as an important factor in describing returns must also be consistent with implications from other "financial models". The results in Table 3 suggest that the size effect (at least conditionally) does not meet this requirement.

4 Conclusions and Future Research

Using recently developed techniques for testing inequality constraints, this paper provides new tests of restrictions implied by conditional asset pricing models. Of particular interest, this methodology is easy to implement, requires little knowledge of the conditional distribution of asset returns, and is valid under fairly weak assumptions. As applications of this method, we investigated some conditional pricing models (e.g. equity premium, liquidity preference theory and size effect) which have received support based on the unconditional distribution of returns. Our results cast doubts on the conditional versions of some of these models and are in contrast to recent empirical work in these areas. On a more cautionary note, the test methodology described in Section 2 provides the econometrician with much leeway in choosing instrumental variables in the information set. The choice of these variables should come from a priori theory or intuition and be picked on an ex ante basis. The tendency to pick "powerful" variables ex post can lead to incorrect application of the statistics. Nevertheless, the econometric techniques developed in the inequality constraints literature seem especially suited to tests of financial models. In particular:

Empirical Corporate

There are two standard empirical techniques employed in the corporate finance literature. The first is to condition on particular events of interest and test whether there is some abnormal mean or variance effect (possibly adjusted for market movements) as implied by the corporate theory. For example, conditional on there being an announcement of a seasoned equity issue, are returns on average negative on day 0, day 1, et cetera? The second method often involves a regression of these "abnormal" returns on explanatory variables identified by the theory. For example, the theory may suggest a set of factors which can explain return movements around the announcement date.

The need for inequality restrictions in this type of framework is very clear. Most of the corporate theories impose restrictions on the signs of the mean/variance effects rather than on the magnitude. While the magnitude of the effect has considerable economic importance, it is also important to guage the appropriate level of significance. Since the inequality restrictions on the parameters are more often multivariate in nature, the approach described in this paper should prove especially useful. It is important to point out that, even if the researcher's null is that there is no effect on the announcement date, the inequality framework needs to be used if the alternative theory suggests sets of inequality restrictions (see Section 2.3).

Market Microstructure Models

With the availability of transactions data, there has been a plethora of theoretical and empirical papers studying the microstructure of financial markets. With respect to empirical work, there are a variety of intraday patterns in stock returns (see, for example, Harris (1987)), leading to a number of competing theoretical explanations (see, for example, Admati and Pfleiderer (1988) versus Brock and Kleidon (1992)). As an illustration, consider the information based models of Admati and Pfleiderer (1988). The U-shaped pattern in variances arises from different flows of information throughout the day. In terms of linking the empirical facts to the model, this information flow is often related to the amount of volume during the day. However, conditional on U-shaped versus humped-shape volume days, do the U-shaped patterns in means and variances still persist? This analysis requires multiple inequality restrictions on the moments throughout the day. Since many market microstructure models impose only sign restrictions between different periods of trading (as in the models above), the inequality testing procedure is the appropriate framework for a statistical analysis.

Dynamic Asset Pricing Models

Rejections of the consumption based asset pricing model (e.g. Hansen and Singleton (1982)) have led many researchers to extend the model in various ways. In particular:

- Recent work has concentrated on developing model restrictions in the presence of market frictions such as short-sale restrictions, borrowing constraints and trading costs (see, for example, He and Modest (1992)).[18] From these constraints, a vari-

[18]We would like to thank George Constantinides for suggesting this particular application to testing

ety of first order conditions are attained, many of which imply inequality restrictions. As an example, one restriction implied by all of the above market frictions is $E_t[n_{t+1}R_{ft}] \leq 1$, where n_t is the marginal rate of substitution from the standard representative agent economy. The procedure outlined in Section 2 can be used to test these restrictions directly.[19]

- Another extension has been to consider restrictions on the mean and variance of marginal rates of substitution implied by various dynamic asset pricing models (see Hansen and Jagannathan (1991)). These restrictions take the form of inequality constraints on the moments of the distribution. Because these constraints can hold conditionally and across various assets, the multivariate nature of the test requires an inequality testing method. Given the potential impact of this bounds literature and the lack of any formal hypothesis testing, the procedure described in this paper should prove useful for future research in this area.

inequality restrictions in asset pricing models.

[19]Note that some complications arise in this nonlinear setting. For example, it may be necessary to estimate the marginal rate of substitution such as the risk-aversion parameter γ in a model in which the agent has constant relative risk averse (CRRA) preferences. This additional estimation can cause the test to be local around the estimated parameter values (see Wolak (1989,1991)). As an alternative, the econometrician can test whether the model is statistically violated for a given marginal rate of substitution as in Hansen and Jagannathan (1991) (i.e. for a given level of γ in a CRRA setting). Although this test is not as general as including the estimation of the marginal rate of substitution, it helps reduce the local nature of the analysis.

Appendix A

In this appendix, we provide a brief description of various methods for calculating the probabilistic weights on the χ^2 distributions (given in equation 8). In particular, the weights have a closed form solution for a small number of restrictions ($N \leq 5$) (see Kudo (1963) for exact calculations). For restrictions greater than five, there are numerical solutions up to eight; however, beyond this number, the problem becomes intractable.

As an alternative, Kodde and Palm (1986, Table 1, page 1246), however, provide upper and lower bound critical values which do not require calculation of the weights. For a given level of significance, the econometrician rejects the null if the statistic W exceeds the upper bound. Similarly, he cannot reject this hypothesis if W is less than the lower bound. These bounds are given by

$$\begin{aligned}\alpha_l &= \frac{1}{2}Pr(\chi_1^2 \geq c_l) \\ \alpha_u &= \frac{1}{2}Pr(\chi_{N-1}^2 \geq c_u) + \frac{1}{2}Pr(\chi_N^2 \geq c_u),\end{aligned}$$

where c_l and c_u are the lower and upper bounds respectively for the critical values of the test. Only for values in between these bounds does the econometrician have to calculate the weights.

Although the weights are not tractable for a large number of restrictions, Wolak (1989b) describes an approximate method for calculating the weights based on a Monte Carlo simulation. He suggests that the econometrician should simulate a multivariate normal distribution with mean zero and covariance matrix $\left(\frac{\hat{\Omega}}{T}\right)$ (denote these values θ^*_{Dz+}). The next step is to find the solution $\tilde{\tilde{\theta}}_{Dz+}$ to the minimization problem

$$\min_{\tilde{\theta}_{Dz+}} \qquad (\theta^*_{Dz+} - \tilde{\theta}_{Dz+})\left(\frac{\hat{\Omega}}{T}\right)^{-1}(\theta^*_{Dz+} - \tilde{\theta}_{Dz+})$$

$$\text{subject to } \tilde{\theta}_{Dz+} \geq 0.$$

For each replication, count the number of elements in the N vector $\tilde{\tilde{\theta}}_{Dz+}$ that exceed zero. As Wolak (1989b) points out, the approximate weight $\hat{w}\left(N, N-k, \frac{\hat{\Omega}}{T}\right)$ will be the fraction of replications in which $\tilde{\tilde{\theta}}_{Dz+}$ has exactly $N-k$ elements exceeding zero.

Table 1
The Ex-ante Risk Premium

The top part of this table lists the test statistics and cut-off levels for tests of the positivity of the ex ante risk premium (for the overall period, 1802 to 1989, as well as for two subperiods). The bottom part of the table lists the unconstrained mean of the risk-premium $\hat{\theta}_{Dz_1^+}$, associated with the instrument $z_{1t}^+ \equiv \max[0, R_{ft} - R_{lt}]$, and the unconstrained mean $\hat{\theta}_{Dz_2^+}$, associated with the instrument $z_{2t}^+ \equiv \frac{1}{(R_{mt}-R_{ft-1})^2}$.

Period	Statistic	P-level	5% Cut-off	5% Lower Bound	5% Upper Bound
1802-1989	4.630	.034	3.829	2.706	5.138
1802-1895	3.359	.066	3.859	2.706	5.138
1896-1989	4.072	.042	3.769	2.706	5.138

Period	$\hat{\theta}_{Dz_1^+}$	(s.e.)	$\hat{\theta}_{Dz_2^+}$	(s.e.)
1802-1989	-1.799	(.844)	13.939	(20.286)
1802-1895	-2.529	(1.56)	-10.777	(15.252)
1896-1989	-1.069	(.585)	38.655	(36.742)

Table 2

The Monotonicity of Term Premiums

Tables 2A and 2B provide tests of whether term premiums are monotonically increasing in maturity. The data are collected from the Fama files for one to eleven month bills (in yearly percent) over the period 1974-1990. The top part of each table lists the test statistic and corresponding cut-off levels. The statistic's P-level is calculated using a Monte-Carlo simulation since the statistic's value falls between the 5% upper and lower bound values. The bottom part of each table lists the unconstrained means for the difference in term premium measures, associated with the instruments $z_{1t}^{+} \equiv 1$, $z_{2t}^{+} \equiv \max[0, h_{1_{t,t+1}} - h_{6_{t,t+6}}]$, and $z_{3t}^{+} \equiv \max[0, h_{6_{t,t+6}} - h_{11_{t,t+11}}]$. Table 2A examines the difference in term premiums $\hat{\theta}_{\tau,Dz_i^+}$, $\tau = 3, \ldots, 11$, associated with each instrument. Table 2B examines four holding period returns on bills — $B1/S0$, $B3/S1$, $B6/S3$ and $B11/S6$, where $B\tau/S\gamma$ means buying a τ-period bill and then selling it with γ periods to maturity — associated with the same instruments.

Table 2A

Statistic	P-level	5% Cut-off	5% Lower Bound	5% Upper Bound
5.578	.810	26.411	2.706	39.531

τ	$\hat{\theta}_{\tau,Dz_1^+}$	(s.e.)	$\hat{\theta}_{\tau,Dz_2^+}$	(s.e.)	$\hat{\theta}_{\tau,Dz_3^+}$	(s.e.)
11	-.0874	(.0558)	-.0253	(.0209)	-.0195	(.0165)
10	-.0657	(.0846)	-.0068	(.0067)	-.0470	(.0310)
9	.0926	(.0688)	-.0173	(.0198)	-.0132	(.0108)
8	.2078	(.0707)	.0138	(.0103)	.0070	(.0135)
7	-.0578	(.0776)	-.0268	(.0226)	-.0153	(.0089)
6	.0476	(.0516)	-.0179	(.0189)	-.0010	(.0123)
5	.1905	(.0789)	-.0205	(.0188)	-.0133	(.0093)
4	-.0020	(.0786)	-.0147	(.0188)	-.0050	(.0051)
3	.3641	(.0861)	-.0014	(.0093)	.0098	(.0085)

Table 2B

Statistic	P-level	5% Cut-off	5% Lower Bound	5% Upper Bound
2.037	NN	NN	2.706	16.274

τ	$\hat{\theta}_{\tau,Dz_1^+}$	(s.e.)	$\hat{\theta}_{\tau,Dz_2^+}$	(s.e.)	$\hat{\theta}_{\tau,Dz_3^+}$	(s.e.)
$B11S6 - B6S3$	.2249	(.1462)	.0255	(.0246)	-.0095	(.0361)
$B6S3 - B3S1$	.3235	(.1383)	-.0310	(.0248)	-.0102	(.0120)
$B3S1 - B1S0$	.6036	(.0802)	-.0196	(.0137)	.0344	(.0266)

Table 3

The Monotonicity of the Size Effect

The top part of this table lists the test statistic and cut-off level for the test of the monotonicity of the ex ante size effect. The data are weekly data taken over the sample period 7/1962-12/1990. The test statistic tests the hypothesis, using the instruments $z_{1t}^{+} = 1$ and $z_{2t}^{+} = \max(0, -R_{ew,t})$, where $R_{ew,t}$ is previous weekly return of the equally weighted portfolio. The statistic's value falls above the 5% Upper Bound. The bottom part of the table lists the unconstrained means of the difference in size portfolio returns, $R_{i-1,t+1} - R_{i,t+1}$, wher i is increasing in firm size. The means are given by $\hat{\theta}_{i,Dz_j^+}$, $i = 1, ..., 4$ and $j = 1, 2$. All the estimates of the means and standard errors are multiplied by 1000.

Statistic	P-level	5% Cut-off	5% Lower Bound	5% Upper Bound
34.476	NN	NN	2.706	14.853

$E_t[R_{i-1,t+1} - R_{i,t+1}]$	$\hat{\theta}_{i,Dz_1^+}$	(s.e.)	$\hat{\theta}_{i,Dz_2^+}$	(s.e.)
$1-2$	1.940	(.309)	-.008	(.003)
$2-3$	0.520	(.165)	-.012	(.002)
$3-4$	0.099	(.162)	-.014	(.003)
$4-5$	0.351	(.213)	-.023	(.005)

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