Measurement Error and the Relationship between Investment and *q*

Timothy Erickson

Bureau of Labor Statistics

Toni M. Whited

University of Iowa

Many recent empirical investment studies have found that the investment of financially constrained firms responds strongly to cash flow. Paralleling these findings is the disappointing performance of the q theory of investment: even though marginal q should summarize the effects of all factors relevant to the investment decision, cash flow still matters. We examine whether this failure is due to error in measuring marginal q. Using measurement error–consistent generalized method of moments estimators, we find that most of the stylized facts produced by investment-q cash flow regressions are artifacts of measurement error. Cash flow does not matter, even for financially constrained firms, and despite its simple structure, q theory has good explanatory power once purged of measurement error.

I. Introduction

The effect of external financial constraints on corporate investment has been the subject of much research over the past decade. Underlying

We gratefully acknowledge helpful comments from Lars Hansen, two anonymous referees, Serena Agoro-Menyang, Brent Moulton, John Nasir, Huntley Schaller, and participants of seminars given at the 1992 Econometric Society summer meetings, the University of Pennsylvania, the University of Maryland, the Federal Reserve Bank of Philadelphia, Rutgers University, and the University of Kentucky. This paper was circulated previously under the title "Measurement-Error Consistent Estimates of the Relationship between Investment and Q."

[*Journal of Political Economy,* 2000, vol. 108, no. 5]

this line of inquiry is the premise that informational imperfections in equity and credit markets lead to a divergence between the costs of external and internal funds or, at the extreme, to rationing of external finance. Any difficulties the firm faces in obtaining outside financing then affect its real investment decisions. Recent interest in this topic started with Fazzari, Hubbard, and Petersen (1988), who showed empirically that for groups of firms perceived a priori to face financing constraints, investment responds strongly to movements in internal funds, even after one controls for investment opportunities. Hubbard (1998) cites numerous studies that have confirmed these results. This literature is the most prominent example of the empirical failure of the neoclassical intertemporal optimization model of investment.

Most tests of the neoclassical model and most empirical studies of the interaction of finance and investment are based on what is commonly referred to as the *q* theory of investment. Despite its repeated failure to explain both cross-section and time-series data, its popularity persists because of its intuitive appeal, simplicity, and sound theoretical underpinnings. Its popularity persists also because of conjectures that its empirical failure is spurious, a consequence of measurement error in *q*. In recent years, however, a number of studies that explicitly address measurement error have reaffirmed the earlier findings, particularly that of a significant role for internal funds (see, e.g., Blundell et al. 1992; Gilchrist and Himmelberg 1995). In the present paper we employ a very different approach to the measurement error problem and come to very different conclusions.

To understand the measurement error problem, it is crucial to think carefully about *q* theory. The intuition behind this theory can be found in Keynes (1936): "there is no sense in building up a new enterprise at a cost greater than that at which a similar existing enterprise can be purchased; whilst there is an inducement to spend on a new project what may seem an extravagant sum, if it can be floated off the stock exchange at an immediate profit" (p. 151). Grunfeld (1960) argued similarly that a firm should invest when it expects investment to be profitable and that an efficient asset market's valuation of the firm captures this expectation. He supported this reasoning by finding that firm market value is an important determinant of investment in a sample of U.S. firms. Tobin (1969) built on this work by using a straightforward arbitrage argument: the firm will invest if Tobin's *q*, the ratio of the market valuation of a firm's capital stock to its replacement value, exceeds one. Modern *q* theory is based on the first-order conditions in Lucas and Prescott (1971) and Mussa (1977) that require the marginal adjustment and purchase costs of investing to be equal to the shadow value of capital. Termed marginal *q*, this shadow value is the firm manager's expectation of the marginal contribution of new capital goods to future profit.

Testing this first-order condition typically relies on drawing a connection between the formal optimization model and the intuitive arguments of Keynes, Grunfeld, and Tobin. For most researchers, the first step in making this connection is to assume quadratic investment adjustment costs, which gives a first-order condition that can be rearranged as a linear regression in which the rate of investment is the dependent variable and marginal q is the sole regressor. The next step is to find an observable counterpart to marginal q. Building on results in Lucas and Prescott (1971), Hayashi (1982) simplified this task by showing that constant returns to scale and perfect competition imply the equality of marginal q with average q, which is the ratio of the manager's valuation of the firm's existing capital stock to its replacement cost. If financial markets are efficient, then their valuation of the capital stock equals the manager's, and consequently, average q should equal the ratio of this market valuation to the replacement value, that is, Tobin's q. In principle, Tobin's q is observable, though in practice its measurement presents numerous difficulties.

The resulting empirical models have been disappointing along several dimensions.[1] The R^2's are very low, suggesting that marginal q has little explanatory power. Further, many authors argue (incorrectly, as we show below) that the fitted models imply highly implausible capital stock adjustment costs and speeds. Finally, the theoretical prediction that marginal q should summarize the effects of all factors relevant to the investment decision almost never holds: output, sales, and, as emphasized above, measures of internal funds typically have statistically significant coefficient estimates and appreciable explanatory power if they are introduced as additional regressors. In particular, estimates of the coefficient on cash flow (the most common measure of internal funds) are typically larger and more significant for firms deemed to be financially constrained than for firms that are not.

These results have a variety of interpretations. If measured Tobin's q is a perfect proxy for marginal q and the econometric assumptions are correct, then, roughly speaking, q theory is "wrong." In other words, a manager's profit expectations do not play an important role in explaining investment, but internal funds apparently do. Alternatively, if q theory is "correct" and measured Tobin's q is a perfect proxy, then some of the econometric assumptions are wrong. For example, Hayashi and Inoue (1991) consider endogeneity of marginal q, and Abel and Eberly

[1] See Ciccolo (1975), Summers (1981), Abel and Blanchard (1986), and Blanchard, Rhee, and Summers (1993) for studies using aggregate data. Recent micro studies include Fazzari et al. (1988), Schaller (1990), Blundell et al. (1992), and Gilchrist and Himmelberg (1995).

(1996) and Barnett and Sakellaris (1998) consider nonlinear regression. A third possibility is that q theory and the econometric assumptions are correct, but measured Tobin's q is a poor proxy for marginal q.

Mismeasurement of marginal q can generate all the pathologies afflicting empirical q models. In the classical errors-in-variables model, for example, the ordinary least squares (OLS) R^2 is a downward-biased estimate of the true model's coefficient of determination, and the OLS coefficient estimate for the mismeasured regressor is biased toward zero. Irrelevant variables may appear significant since coefficient estimates for perfectly measured regressors can be biased *away* from zero. This bias can differ greatly between two subsamples, even if the rate of measurement error is the same in both. The spurious-significance problem is exacerbated by the fact that homoskedastic measurement error can generate conditionally heteroskedastic data, thus inappropriately shrinking OLS standard errors. Finally, the conditional expectation of the independent variable given the proxy is generally nonlinear, which may lead to premature abandonment of linear functional forms.[2]

Other explanations for the failure of investment-q regressions, such as finance constraints, fixed costs, learning, or simultaneity bias, are appealing but, unlike the measurement error hypothesis, cannot individually explain all of q theory's empirical shortcomings. It therefore is natural to try an explicit errors-in-variables remedy. Papers doing so include Abel and Blanchard (1986), Hoshi and Kashyap (1990), Blundell et al. (1992), Cummins, Hassett, and Hubbard (1994), Gilchrist and Himmelberg (1995), and Cummins, Hassett, and Oliner (1998). For the most part, these papers find significant coefficients on measures of internal funds. Notably, Gilchrist and Himmelberg find, like Fazzari et al., that for most ways of dividing their sample into financially constrained and unconstrained firms, the constrained firms' investment is more sensitive to cash flow.

We use a very different method. Following Geary (1942), we construct consistent estimators that use the information contained in the third- and higher-order moments of the joint distribution of the observed regression variables. By using generalized method of moments (GMM) (Hansen 1982) to exploit the information afforded by an excess of moment equations over parameters, we increase estimator precision and obtain the GMM J-test of overidentifying restrictions as a tool for detecting departures from the assumptions required for estimator consistency.

The results from applying OLS and GMM estimators to our data on U.S. manufacturing firms *both* cast doubt on the Fazzari et al. (FHP) hypothesis: that the investment of liquidity-constrained firms responds

[2] See Gleser (1992) for a discussion of this last point.

strongly to cash flow. As expected, the OLS regression of investment on measured Tobin's q gives an unsatisfyingly low R^2 and a significantly positive estimate for the coefficient on cash flow. However, the estimated cash flow coefficient is much greater for firms classified as *un*constrained, the reverse of what is predicted by the FHP hypothesis. This reverse pattern has been observed before in the literature and, like the expected pattern, can be explained in terms of measurement error.

In contrast, our GMM estimates of the cash flow coefficient are small and statistically insignificant for subsamples of a priori liquidity-constrained firms as well as subsamples of unconstrained firms. Furthermore, the GMM estimates of the population R^2 for the regression of investment on true marginal q are, on average, more than twice as large as the OLS R^2. Similarly, the GMM estimates of the coefficient on marginal q are much larger than our OLS estimates, though, as noted above, we shall argue that these coefficients are not informative about adjustment costs. Measurement error theory predicts these discrepancies, and, in fact, we estimate that just over 40 percent of the variation in measured Tobin's q is due to true marginal q.

We organize the paper as follows. Section II reviews q theory, establishes criteria for its empirical evaluation, and describes likely sources of error in measuring marginal q. Section III presents our estimators and discusses their applicability to q theory. Section IV reports our estimates. Section V explains how a measurement error process that is the same for both constrained and unconstrained firms can generate spurious cash flow coefficient estimates that differ greatly between these two groups. The construction of our data set and Monte Carlo simulations of our estimators are described in Appendices A and B.

II. A Simple Investment Model

To provide a framework for discussing specification issues concerning our empirical work, we present a standard dynamic investment model in which capital is the only quasi-fixed factor and risk-neutral managers choose investment each period to maximize the expected present value of the stream of future profits. The value of firm i at time t is given by

$$V_{it} = E\left[\sum_{j=0}^{\infty}\left(\prod_{s=1}^{j} b_{i,t+s}\right)[\Pi(K_{i,t+j}, \xi_{i,t+j}) - \psi(I_{i,t+j}, K_{i,t+j}, \nu_{i,t+j}, \mathbf{h}_{i,t+j}) - I_{i,t+j}]\,\middle|\,\Omega_{it}\right], \quad (1)$$

where E is the expectations operator; Ω_{it} is the information set of the manager of firm i at time t; b_{it} is the firm's discount factor at time t; K_{it} is the beginning-of-period capital stock; I_{it} is investment; $\Pi(K_{it}, \xi_{it})$ is the profit function, $\Pi_K > 0$; and $\psi(I_{it}, K_{it}, \nu_{it}, \mathbf{h}_{it})$ is the investment adjustment cost function, which is increasing in I_{it}, decreasing in K_{it}, and convex in both arguments. The term $\mathbf{h}_{it}$ is a vector of variables, such as labor productivity, that might also affect adjustment costs, and ξ_{it} and ν_{it} are exogenous shocks to the profit and adjustment cost functions; both are observed by the manager but unobserved by the econometrician at time t. All variables are expressed in real terms, and the relative price of capital is normalized to unity. Note that any variable factors of production have already been maximized out of the problem.

The firm maximizes equation (1) subject to the following capital stock accounting identity:

$$K_{i,t+1} = (1 - d_i)K_{it} + I_{it}, \tag{2}$$

where d_i is the assumed constant rate of capital depreciation for firm i. Let χ_{it} be the sequence of Lagrange multipliers on the constraint (2). The first-order condition for maximizing the value of the firm in equation (1) subject to (2) is

$$1 + \psi_I(I_{it}, K_{it}, \nu_{it}, \mathbf{h}_{it}) = \chi_{it}, \tag{3}$$

where

$$\chi_{it} = E\left[\sum_{j=1}^{\infty}\left(\prod_{s=1}^{j} b_{i,t+s}\right)(1 - d_i)^{j-1}[\Pi_K(K_{i,t+j}, \xi_{i,t+j}) - \psi_K(I_{i,t+j}, K_{i,t+j}, \nu_{i,t+j}, \mathbf{h}_{i,t+j})] \middle| \Omega_{it}\right]. \tag{4}$$

Equation (3) states that the marginal cost of investment equals its expected marginal benefit. The left side comprises the adjustment and purchasing costs of capital goods, and the right side represents the expected shadow value of capital, which, as shown in (4), is the expected stream of future marginal benefits from using the capital. These benefits include both the marginal additions to profit and reductions in installation costs. Since we normalize the price of capital goods to unity, χ_{it} is the quantity "marginal q" referred to in the Introduction.

Most researchers to date have tested q theory via a linear regression of the rate of investment on χ_{it}. This procedure requires a proxy for the unobservable χ_{it} and a functional form for the installation cost function having a partial derivative with respect to I_{it} that is linear in I_{it}/K_{it} and ν_{it}. Below we consider at length the problem of obtaining a proxy. A class of functions that meets the functional form requirement and is also linearly homogeneous in I_{it} and K_{it} is given by

$$\psi(I_{it}, K_{it}, \nu_{it}, \mathbf{h}_{it}) = (a_1 + a_2\nu_{it})I_{it} + a_3\frac{I_{it}^2}{K_{it}} + K_{it}f(\nu_{it}, \mathbf{h}_{it}). \tag{5}$$

Here, f is an integrable function, and $a_1, \ldots, a_3$ are constants. We restrict $a_3 > 0$ to ensure concavity of the value function in the maximization problem. The adjustment cost functions chosen either explicitly or implicitly by all researchers who test q theory with linear regressions are variants of (5). Differentiating (5) with respect to I_{it} and substituting the result into (3) yields the familiar regression equation

$$y_{it} = \alpha_0 + \beta\chi_{it} + u_{it}, \tag{6}$$

where $y_{it} \equiv I_{it}/K_{it}$, $\alpha_0 \equiv -(1 + a_1)/2a_3$, $\beta \equiv 1/2a_3$, and $u_{it} \equiv -a_2\nu_{it}/2a_3$.

A. *Model Evaluation Criteria*

To evaluate this model, most authors regress y_{it} on a proxy for χ_{it}, usually a measure of Tobin's q, and then do one or more of the following three things: (i) examine the adjustment costs implied by estimates of β; (ii) examine the explanatory power of χ_{it}, as measured by the R^2 of the fitted model; and (iii) test whether other variables enter significantly into the fitted regression, since theory says that no variable other than χ_{it} should appear in (6). Some authors split their samples into subsamples consisting of a priori financially constrained and unconstrained firms and then perform these evaluations, especially point iii, separately on each subsample.

In the present paper we estimate financially constrained and unconstrained regimes by fitting the full sample to models that interact cash flow with various financial constraint indicators. We perform measurement error–consistent versions of points ii and iii. We ignore point i because any attempt to relate β to adjustment costs contains two serious pitfalls. First, equation (3) implies that a firm's period t marginal adjustment costs are identically equal to $\chi_{it} - 1$ and are therefore independent of β. Second, the regression equation (6) cannot be integrated back to a unique adjustment cost function but to a whole class of functions given by (5). Any attempt at evaluating a firm's average adjustment costs, ψ/I_{it}, requires a set of strong assumptions to choose a function from this class, and different arbitrary choices yield widely different estimates of adjustment costs.[3] Note that the constant of integration should not be interpreted as a fixed cost since it does not necessarily

[3] See Whited (1994) for further discussion and examples.

"turn off" when investment is zero. It can, however, be interpreted as a permanent component of the process of acquiring capital goods, such as a purchasing department.

B. Sources of Measurement Error

We now show how attempts to use Tobin's q to measure marginal q can admit serious error. To organize our discussion we use four quantities. The first is marginal q, defined previously as χ_{it}. The second is average q, defined as V_{it}/K_{it}, where the numerator is given by (1); recall that V_{it} is the manager's subjective valuation of the capital stock. The third is Tobin's q, which is the financial market's valuation of average q. Conceptual and practical difficulties exist in measuring the components of Tobin's q; we therefore introduce a fourth quantity called *measured* q, defined to be an estimate of Tobin's q. Measured q is the regression proxy for marginal q; average q and Tobin's q are simply devices for identifying and assessing possible sources of error in measuring marginal q.

These sources can be placed in three useful categories, corresponding to the possible inequalities between successive pairs of the four concepts of q. First, marginal q may not equal average q, which will occur whenever we have a violation of the assumption either of perfect competition or of linearly homogeneous profit and adjustment cost functions. A second source of measurement error is divergence of average q from Tobin's q. As discussed in Blanchard et al. (1993), stock market inefficiencies may cause the manager's valuation of capital to differ from the market valuation. Finally, even if marginal q equals average q and financial markets are efficient, numerous problems arise in estimating Tobin's q. Following many researchers in this area, we estimate Tobin's q by evaluating the commonly used expression

$$x_{it} = \frac{D_{it} + S_{it} - N_{it}}{K_{it}}. \tag{7}$$

Here D_{it} is the market value of debt, S_{it} is the market value of equity, N_{it} is the replacement value of inventories, and K_{it} is redefined as the replacement value of the capital stock. Note that the numerator only approximates the market value of the capital stock. The market values of debt and equity equal the market value of the firm, so the market value of the capital stock is correctly obtained by subtracting *all* other assets backing the value of the firm: not just the replacement value of inventories, but also the value of non–physical assets such as human capital and goodwill. The latter assets typically are not subtracted because data limitations make them impossible to estimate. An additional source of error is that D_{it}, N_{it}, and K_{it} must be estimated from accounting data that do not adequately capture the relevant economic concepts. As is typical of the literature, we estimate these three variables using recursive procedures; details can be found in Whited (1992). An alternative method of constructing K_{it} that addresses the problem of capital aggregation is given by Hayashi and Inoue (1991).

From this discussion it is clear that the measurement errors are serially correlated because market power persists over time, because deviations of market expectations from fundamental value are subject to persistent "fads," and because the procedures used to approximate the components of (7) directly induce serial correlation in its measurement error. These procedures use a previous period's estimate of a variable to calculate the current period's estimate, implying that the order of serial correlation will be at least as great as the number of time-series observations. This type of correlation violates the assumptions required by the measurement error remedies used in some of the papers cited in the Introduction. As shown below, however, our own estimators permit virtually arbitrary dependence.

III. Data and Estimators

Our data set consists of 737 manufacturing firms from the Compustat database covering the years 1992–95. Our sample selection procedure is described in Appendix A, and the construction of our regression variables is described in the appendix to Whited (1992). Initially we treat this panel as four separate (but not independent) cross sections. We specify an errors-in-variables model, assume that it holds for each cross section, and then compute consistent estimates of each cross section's parameters using the estimators we describe below. Assuming that the parameters of interest are constant over time, we next pool their cross-section estimates using a minimum distance estimator, also described below.

A. Cross-Section Assumptions

For convenience we drop the subscript t and rewrite equation (6) more generally as

$$y_i = \mathbf{z}_i\boldsymbol{\alpha} + \chi_i\beta + u_i. \tag{8}$$

For application to a split sample consisting only of a priori financially constrained (or unconstrained) firms, $\mathbf{z}_i$ is a row vector containing $z_{i0} = 1$ and $z_{i1} = (\text{cash flow})_i/K_i$. For application to a full sample, $\mathbf{z}_i$ further includes $z_{i2} = d_i z_{i1}$ and $z_{i3} = d_i$, where $d_i = 1$ if firm i is financially constrained and $d_i = 0$ otherwise. We assume that u_i is a mean zero error independent of $(\mathbf{z}_i, \chi_i)$ and that χ_i is measured according to

$$x_i = \gamma_0 + \chi_i + \epsilon_i, \tag{9}$$

where x_i is measured q and ϵ_i is a mean zero error independent of $(u_i, \mathbf{z}_i, \chi_i)$. The intercept γ_0 allows for the nonzero means of some sources of measurement error, such as the excess of measured q over Tobin's q caused by unobserved non–physical assets. Our remaining assumptions are that $(u_i, \epsilon_i, z_{i1}, z_{i2}, z_{i3}, \chi_i)$, $i = 1, \ldots, n$, are independently and identically distributed (i.i.d.), that the residual from the projection of χ_i on $\mathbf{z}_i$ has a skewed distribution, and that $\beta \neq 0$. The reason for the last two assumptions and a demonstration that they are testable are given in subsection *B*.

There are two well-known criticisms of equation (8) and its accompanying assumptions. First, the relationship between investment and marginal q (i.e., between y_i and χ_i) may be nonlinear. As pointed out by Abel and Eberly (1996) and Barnett and Sakellaris (1998), this problem may occur when there are fixed costs of adjusting the capital stock. These papers present supporting empirical evidence; recall, however, that a linear measurement error model can generate nonlinear conditional expectation functions in the data, implying that such evidence is ambiguous.

The second well-known criticism is that u_i may not be independent of $(\mathbf{z}_i, \chi_i)$ because of the simultaneous-equations problem. The possible dependence between u_i and χ_i arises because the "regression" (6) underlying (8) is a rearranged first-order condition. Recalling that u_i is inversely related to ν_i, note that ν_{it} does not appear in (4), the expression giving χ_{it}. This absence is the result of our one-period time to build assumption. To the extent that this assumption holds, therefore, ν_{it} can be related to χ_{it} only indirectly. One indirect route is the effect of ν_{it} on $K_{i,t+j}$, $j \geq 1$, and thence on the future marginal revenue product of capital. This route is blocked if we combine our linearly homogeneous adjustment cost function with the additional assumptions of (i) perfect competition and (ii) linearity of the profit function in $K_{i,t+j}$. The other indirect route is temporal dependence between ν_{it} and $\phi_{i,t+j} \equiv (\nu_{i,t+j}, \xi_{i,t+j}, \mathbf{h}_{i,t+j})$, $j \geq 1$. This route can be blocked by a variety of assumption sets such as the following: (iii*a*) ϕ_{it} is independent of $\phi_{i,t+j}$ for $j \geq 1$; *or* (iii*b*) ν_{it} is independent of ξ_{it} for all t, and the function f appearing in (5) is identically zero. Note that conditions i and ii, which are necessary, also eliminate the divergence of marginal from average q. Our estimates will be valuable, then, to the extent that measurement error is large, but mostly because of the other sources discussed in Section II*B*.

The possible dependence between u_i and the cash flow ratio, z_{i1}, occurs if current investment typically becomes productive, cash flow–producing capital within the period, a violation of our one-period time to build assumption. Other possible elements of $\mathbf{z}_i$ are dummy variables indicating the presence of liquidity constraints and the interactions of these dummies with z_{i1}. One dummy identifies firms lacking a bond rating; the other dummy identifies "small" firms. We argue below that firm size and bond ratings are independent of u_i.

We also see a noteworthy problem with our measurement error assumptions: they ignore mismeasurement of the capital stock. If capital is mismeasured, then, since it is the divisor in the investment rate y_i, the proxy x_i, and the cash flow ratio z_{i1}, these ratios are also mismeasured, with conditionally heteroskedastic and mutually correlated measurement errors.

It is clear that the criticized assumptions may not hold. However, only assumption violations large enough to qualitatively distort inferences are a problem. In Appendix B we present Monte Carlo simulations showing that it is possible to detect such violations with the GMM J-test of overidentifying restrictions.

B. *Cross-Section Estimators*

To simplify our computations we first "partial out" the perfectly measured variables in (8) and (9) and rewrite the resulting expressions in terms of population residuals. This yields

$$y_i - \mathbf{z}_i\boldsymbol{\mu}_y = \eta_i\beta + u_i \qquad (10)$$

and

$$x_i - \mathbf{z}_i\boldsymbol{\mu}_x = \eta_i + \epsilon_i, \qquad (11)$$

where

$$(\boldsymbol{\mu}_y,\ \boldsymbol{\mu}_x,\ \boldsymbol{\mu}_\chi) \equiv [E(\mathbf{z}_i'\mathbf{z}_i)]^{-1}E[\mathbf{z}_i'(y_i,\ x_i,\ \chi_i)]$$

and $\eta_i \equiv \chi_i - \mathbf{z}_i\boldsymbol{\mu}_\chi$. Given $(\boldsymbol{\mu}_y,\ \boldsymbol{\mu}_x)$, this is the textbook classical errors-in-variables model, since our assumptions imply that u_i, ϵ_i, and η_i are mutually independent. Substituting

$$(\hat{\boldsymbol{\mu}}_y,\ \hat{\boldsymbol{\mu}}_x) \equiv \left(\sum_{i=1}^{n}\mathbf{z}_i'\mathbf{z}_i\right)^{-1}\sum_{i=1}^{n}\mathbf{z}_i'(y_i,\ x_i)$$

into (10) and (11), we estimate β, $E(u_i^2)$, $E(\epsilon_i^2)$, and $E(\eta_i^2)$ with the GMM procedure described in the next paragraph. Estimates of the lth element of $\boldsymbol{\alpha}$ are obtained by substituting the GMM estimate of β and the lth elements of $\hat{\boldsymbol{\mu}}_y$ and $\hat{\boldsymbol{\mu}}_x$ into

$$\alpha_l = \mu_{yl} - \mu_{xl}\beta, \quad l \neq 0. \tag{12}$$

Estimates of $\rho^2 \equiv 1 - [\text{Var}\,(u_i)/\,\text{Var}\,(y_i)]$, the population R^2 for (8), are obtained by evaluating

$$\rho^2 = \frac{\boldsymbol{\mu}_y'\text{Var}\,(\mathbf{z}_i)\boldsymbol{\mu}_y + E(\eta_i^2)\beta^2}{\boldsymbol{\mu}_y'\text{Var}\,(\mathbf{z}_i)\boldsymbol{\mu}_y + E(\eta_i^2)\beta^2 + E(u_i^2)} \tag{13}$$

at $\hat{\boldsymbol{\mu}}_y$, $\hat{\boldsymbol{\mu}}_x$, the sample covariance matrix for $\mathbf{z}$, and the GMM estimates of β, $E(\eta_i^2)$, and $E(u_i^2)$.

Our GMM estimators are based on equations expressing the moments of $y_i - \mathbf{z}_i\boldsymbol{\mu}_y$ and $x_i - \mathbf{z}_i\boldsymbol{\mu}_x$ as functions of β and the moments of u_i, ϵ_i, and η_i. There are three second-order moment equations:

$$E[(y_i - \mathbf{z}_i\boldsymbol{\mu}_y)^2] = \beta^2E(\eta_i^2) + E(u_i^2), \tag{14}$$

$$E[(y_i - \mathbf{z}_i\boldsymbol{\mu}_y)(x_i - \mathbf{z}_i\boldsymbol{\mu}_x)] = \beta E(\eta_i^2), \tag{15}$$

and

$$E[(x_i - \mathbf{z}_i\boldsymbol{\mu}_x)^2] = E(\eta_i^2) + E(\epsilon_i^2). \tag{16}$$

The left-hand-side quantities are consistently estimable, but there are only three equations with which to estimate the four unknown parameters on the right-hand side. The third-order product moment equations, however, consist of two equations in two unknowns:

$$E[(y_i - \mathbf{z}_i\boldsymbol{\mu}_y)^2(x_i - \mathbf{z}_i\boldsymbol{\mu}_x)] = \beta^2E(\eta_i^3) \tag{17}$$

and

$$E[(y_i - \mathbf{z}_i\boldsymbol{\mu}_y)(x_i - \mathbf{z}_i\boldsymbol{\mu}_x)^2] = \beta E(\eta_i^3). \tag{18}$$

Geary (1942) was the first to point out the possibility of solving these two equations for β. Note that a solution exists if the identifying assumptions $\beta \neq 0$ and $E(\eta_i^3) \neq 0$ are true, and one can test the contrary hypothesis $\beta = 0$ or $E(\eta_i^3) = 0$ or both by testing whether the sample counterparts to the left-hand sides of (17) and (18) are significantly different from zero.

Given β, equations (14)–(16) and (18) can be solved for the remaining right-hand-side quantities. We obtain an overidentified equation system by combining (14)–(18) with the fourth-order product moment equations, which introduce only one new quantity, $E(\eta_i^4)$:

$$E[(y_i - \mathbf{z}_i\boldsymbol{\mu}_y)^3(x_i - \mathbf{z}_i\boldsymbol{\mu}_x)] = \beta^3E(\eta_i^4) + 3\beta E(\eta_i^2)E(u_i^2), \tag{19}$$

$$E[(y_i - \mathbf{z}_i\boldsymbol{\mu}_y)^2(x_i - \mathbf{z}_i\boldsymbol{\mu}_x)^2] = \beta^2[E(\eta_i^4) + E(\eta_i^2)E(\epsilon_i^2)] + E(u_i^2)[E(\eta_i^2) + E(\epsilon_i^2)], \quad (20)$$

and

$$E[(y_i - \mathbf{z}_i\boldsymbol{\mu}_y)(x_i - \mathbf{z}_i\boldsymbol{\mu}_x)^3] = \beta[E(\eta_i^4) + 3E(\eta_i^2)E(\epsilon_i^2)]. \quad (21)$$

The resulting eight-equation system (14)–(21) contains the six unknowns (β, $E(u_i^2)$, $E(\epsilon_i^2)$, $E(\eta_i^2)$, $E(\eta_i^3)$, $E(\eta_i^4)$). We estimate this vector by numerically minimizing a quadratic form in

$$\begin{pmatrix} \frac{1}{n}\sum_{i=1}^{n}[(y_i - \mathbf{z}_i\hat{\boldsymbol{\mu}}_y)^2] - [\beta^2 E(\eta_i^2) + E(u_i^2)] \\ \vdots \\ \frac{1}{n}\sum_{i=1}^{n}[(y_i - \mathbf{z}_i\hat{\boldsymbol{\mu}}_y)(x_i - \mathbf{z}_i\hat{\boldsymbol{\mu}}_x)^3] - \beta[E(\eta_i^4) + 3E(\eta_i^2)E(\epsilon_i^2)] \end{pmatrix},$$

where the matrix of the quadratic form is chosen to minimize asymptotic variance. This matrix differs from the standard optimal weighting matrix by an adjustment that accounts for the substitution of ($\hat{\boldsymbol{\mu}}_x$, $\hat{\boldsymbol{\mu}}_y$) for ($\boldsymbol{\mu}_x$, $\boldsymbol{\mu}_y$); see Erickson and Whited (1999) for details.

Although the GMM estimator just described efficiently utilizes the information contained in equations (14)–(21), nothing tells us that this system is an optimal choice from the infinitely many moment equations available. We therefore report the estimates obtained from a variety of equation systems; as will be seen, the estimates are similar and support the same inference. We use three specific systems: (14)–(18), (14)–(21), and a larger system that additionally includes the equations for the fifth-order product moments and the third-order non–product moments. We denote estimates from these nested systems as GMM3, GMM4, and GMM5.[4]

Along with estimates of α_1, α_2, β, and ρ^2, we shall also present estimates of $\tau^2 \equiv 1 - [\text{Var}(\epsilon_i)/\text{Var}(y_i)]$, the population R^2 for (9). This quantity is a useful index of measurement quality: the quality of the proxy variable x_i ranges from worthless at $\tau^2 = 0$ to perfect at $\tau^2 = 1$. We estimate τ^2 in a way exactly analogous to that for ρ^2.

The asymptotic distributions for all the estimators of this section can be found in Erickson and Whited (1999).

C. *Identification and the Treatment of Fixed Effects*

Transforming the observations for each firm into deviations from that firm's four-year averages or into first differences is a familiar preventive remedy for bias arising when fixed effects are correlated with regressors.

[4] Cragg (1997) gives an estimator that, apart from our adjustment to the weighting matrix, is the GMM4 estimator.

For our data, however, after either transformation we can find no evidence that the resulting models satisfy our identifying assumptions $\beta \neq 0$ and $E(\eta_i^3) \neq 0$: the hypothesis that the left-hand sides of (17) and (18) are both equal to zero cannot be rejected at even the .1 level, for any year and any split-sample or full-sample specification.[5] In fact, the great majority of the p-values for this test exceed .4. In contrast, untransformed (levels) data give at least some evidence of identification with split-sample models and strong evidence with the interaction term models; see tables 1 and 2 below. We therefore use data in levels form. Our defense against possible dependence of a fixed effect in u_i (or ϵ_i) on $(\mathbf{z}_i, \chi_i)$ is the J-test. The test will have power to the extent that the dependence includes conditional heteroskedasticity (which is simulated in App. B), conditional skewness, or conditional dependence on other high-order moments.

D. Combining Cross-Section Estimates Using Minimum Distance Estimation

Let γ denote any one of our parameters of interest: α_1, α_2, β, ρ^2, or τ^2. Suppose that $\hat{\gamma}_1, \ldots, \hat{\gamma}_4$ are the four cross-section estimates of γ given by any one of our estimators. An estimate that is asymptotically more efficient than any of the individual cross-section estimates is the value minimizing a quadratic form in $(\hat{\gamma}_1 - \gamma, \ldots, \hat{\gamma}_4 - \gamma)$, where the matrix of the quadratic form is the inverse of the asymptotic covariance matrix of the vector $(\hat{\gamma}_1, \ldots, \hat{\gamma}_4)$. Newey and McFadden (1994) call this a classical minimum distance estimator. A nice feature of this estimator is that it does not require assuming that the measurement errors ϵ_{it} are serially uncorrelated.[6]

For each parameter of interest we compute four minimum distance estimates, corresponding to the four types of cross-section estimates: OLS, GMM3, GMM4, and GMM5. To compute each minimum distance estimator, we need to determine the covariances between the cross-section estimates being pooled. Our estimate of each such covariance is the covariance between the estimators' respective influence functions (see Erickson and Whited 1999).

[5] The liquidity constraint criteria "firm size" and "bond rating" are defined in Sec. IV. The Wald statistic used for these tests, based on the sample counterparts to the left-hand sides of (17) and (18), is given in Erickson and Whited (1999). The intercept is deleted from $\boldsymbol{\alpha}$, the vector $\mathbf{z}_i$ is redefined to exclude $z_{i0} = 1$, and γ_0 is eliminated from (9) when we fit models to transformed data.

[6] We can also pool four estimates of the entire vector of parameters of interest, $(\alpha_1, \alpha_2, \beta, \rho^2, \tau^2)$, obtaining an asymptotic efficiency gain like that afforded by seemingly unrelated regressions. However, this estimator performs unambiguously worse in Monte Carlo simulations than the estimators we use, probably because the 20×20 optimal minimum distance weighting matrix is too large to estimate effectively with a sample of our size.

E. Previous Approaches

It is useful to note how the measurement error remedies used by other authors differ from our own. One alternative approach is to assume that ϵ_{it} is serially uncorrelated, thereby justifying the estimators of Griliches and Hausman (1986) or the use of lagged values of measured q_{it} as instruments. Studies doing so are those by Hoshi and Kashyap (1990), Blundell et al. (1992), and Cummins, Hassett, and Hubbard (1994). As noted, however, a substantial intertemporal error correlation is highly likely. Another approach is that of Abel and Blanchard (1986), who proxy marginal q by projecting the firm's series of discounted marginal profits onto observable variables in the firm manager's information set. Feasible versions of this proxy, however, use estimated discount rates and profits, creating a measurement error that can be shown to have deleterious properties similar to those in the classical errors-in-variables model. For example, Gilchrist and Himmelberg (1995), who adapt this approach to panel data, assume one discount rate for all firms and time periods; insofar as the true discount rates are correlated with cash flow, this procedure creates a measurement error that is correlated with the proxy. Finally, a third alternative approach is that of Cummins, Hassett, and Oliner (1998), who proxy marginal q by a discounted series of financial analysts' forecasts of earnings.

IV. Estimates and Tests from U.S. Firm-Level Manufacturing Data

Much of the recent empirical q literature has emphasized that groups of firms classified as financially constrained behave differently than those that are not. In particular, many studies have found that cash flow enters significantly into investment-q regressions for groups of constrained firms, a result that has been interpreted as implying that financial market imperfections cause firm-level investment to respond to movements in internal funds. In addressing this issue, we need to tackle two preliminary matters. First, we need to find observable variables that serve to separate our sample of firms into financially constrained and unconstrained groups. Second, we need to see whether our estimators can perform well on these subsamples.

The investment literature has studied a number of indicators of potential financial weakness. For example, Fazzari et al. (1988) use the dividend payout ratio, arguing that dividends are a residual in the firm's real and financial decisions. Therefore, a firm that does not pay dividends must face costly external finance; otherwise it would have issued new shares or borrowed in order to pay dividends. Whited (1992) classifies firms according to whether they have bond ratings or not. The intuition here is that a firm with a bond rating has undergone a great deal of public scrutiny and will be less likely to encounter the asymmetric information problems that lead to financial constraints. Other authors have chosen variables such as firm size, debt-to-assets ratios, interest coverage ratios, age, and, as in Hoshi, Kashyap, and Scharfstein (1991), membership in a Japanese *keiretsu*.

In choosing our measures of potential financial weakness, we first discard those that are not relevant to the United States or those that are not readily available, such as firm age. More important, we discard those variables that are endogenously determined with the firm's investment decision. For example, firms often issue debt precisely to fund current and future investment, which means that either the current or lagged debt-to-assets ratio may be correlated with the error in an investment-q regression. Similarly, dividends are quite likely to be determined simultaneously with investment since the manager must decide whether the marginal dollar of cash flow is worth more to shareholders invested inside the corporation or paid out as dividends.

Given these considerations, we have chosen firm size and the existence of a bond rating as indicators of financial strength. The rationale for using size is that small firms are more likely to be younger and therefore less well known; thus they are more likely to face information asymmetries. Since firm size is not a choice variable for the manager in the short run and is unlikely to depend on investment over the short time period covered by our panel, we can regard it as exogenous. Because it is a continuous variable, we classify a firm as "small" if for all four years of the sample it is in the lower third of each year's distribution of total assets *and* each year's distribution of the capital stock. This procedure divides our 737 firms into 217 constrained and 520 unconstrained firms. Alternate definitions requiring membership in the lower half or quarter of one or both distributions produced qualitatively similar results.

Turning to our other measure of financial health, we classify a firm as unconstrained if it has a Standard & Poor's bond rating in all four years of the sample. This division gives 459 constrained firms and 278 unconstrained firms. We regard bond ratings as exogenous because agencies that provide bond ratings tend to base their judgments more on a consistent history of good financial and operating performance than on current operating decisions.

Even when one supposes that firm size and bond rating are perfectly exogenous, using either of these variables to sort firms into putatively constrained/unconstrained groups is likely to misclassify some firms. These misclassifications will not affect the size of tests of the null hypothesis that cash flow does not affect investment for any firm. They are likely, however, to reduce the power of such tests when the FHP hypothesis is true.

TABLE 1
p-Values from Identification Tests: Interaction Term Models

Interaction Term Model	1992	1993	1994	1995
Bond rating	.005	.041	.013	.023
Firm size	.003	.069	.023	.031
Bond rating and firm size	.004	.068	.021	.030

Note.—The null hypothesis is $\beta = 0$ or $E(\eta_i^3) = 0$ or both. The model is identified if the null hypothesis is false.

A. *The Models We Estimate*

Even with data in levels, the subsamples determined by the firm size or bond rating criteria provide limited evidence that our identifying assumptions $\beta \neq 0$ and $E(\eta_i^3) \neq 0$ hold: none of the unconstrained-firm subsamples, and only some of the constrained-firm subsamples, give .05 level rejections of the hypothesis that the left-hand sides of (17) and (18) are both equal to zero. Monte Carlo results in Appendix table B3 suggest that the test is accurately sized and has good power for models using the full sample but limited power for the smaller sample sizes produced by splitting. We conclude that our split-sample models may not be identified or else may not be reliably estimated by high-order moments because of insufficient sample size.

Because of this identification ambiguity, we shall report estimates of models for which there *is* strong evidence of identification. Specifically, we use complete (not split) cross sections to estimate a model having two additional regressors in $\mathbf{z}_i$: a 0-1 dummy variable equal to one if firm i is liquidity constrained and an interaction of this dummy with the cash flow ratio z_{i1}. We consider two versions of this extended model, distinguished by whether we use the size or bond rating criterion to define the dummy. Table 1 shows that the size-defined interaction term model gives the desired test rejections at the .05 level for three of the four years, with the p-value of the exceptional year equal to .069. The bond rating interaction term model provides rejections for all years, having a maximum p-value of .041.

Split-sample estimation implies a model in which each parameter is allowed to differ in value between the financially constrained and unconstrained regimes. Our interaction term model is equivalent to constraining β and the other parameters estimated directly by high-order moment GMM to be the same in both regimes, while leaving $\boldsymbol{\mu}_y$, $\boldsymbol{\mu}_x$, and $\mathrm{Var}(\mathbf{z}_i)$ unconstrained. We test this constraint by using the Wald statistic given in Greene (1990, sec. 7.4) to see whether the difference between the GMM estimates from the financially constrained and unconstrained subsamples is significantly different from zero at the .05 level. For each year the majority, or all, of the three tests (one each for the GMM3, GMM4, and GMM5 estimators) fail to reject the constraint. These results

TABLE 2
Bond Rating Interaction Model: Estimates of β, the Coefficient on Marginal q

	OLS	GMM3	GMM4	GMM5
1992	.014	.048	.026	.053
	(.002)	(.020)	(.006)	(.013)
1993	.013	.041	.041	.053
	(.002)	(.008)	(.009)	(.008)
1994	.014	.082	.048	.022
	(.003)	(.074)	(.010)	(.005)
1995	.018	.048	.036	.062
	(.004)	(.013)	(.016)	(.012)
Minimum distance	.014	.045	.034	.033
	(.002)	(.006)	(.005)	(.005)

Note.—Standard errors are in parentheses under the parameter estimates. For OLS we use the heteroskedasticity-consistent standard errors of White (1980).

are questionable in view of the ambiguous identification of the subsamples, but we would be uncomfortable if the constraint were rejected.

B. *Estimates of the Bond Rating Interaction Term Model*

We shall report the minimum distance estimates described in Section III*D* for three different interaction term models. Space limitations prevent us from also reporting, for every model, the annual cross-section estimates that underlie the minimum distance estimates. Instead, we shall report the annual estimates for one model. We choose the bond rating interaction term model because it performs well on the identification tests and because we feel that the bond rating criterion is a more direct indicator of liquidity constraints than the firm size criterion.

Table 2 presents both annual and minimum distance estimates of the coefficient on marginal q for the bond rating interaction term model. To illustrate the impact of measurement error on inference, we present, alongside our GMM estimates, OLS estimates calculated under the assumption of perfect measurement. The annual OLS estimates of the coefficient on q are clustered tightly around the modal estimate of .014. The minimum distance estimator that pools these estimates, henceforth referred to as an OLS-MD estimate, is also .014. We note that these values are quite similar to the estimates from the panel data studies surveyed by Schaller (1990). By comparison, the GMM estimates for each year are from 1.6 to 5.9 times larger than the OLS estimate from the same year. The GMM3-MD, GMM4-MD, and GMM5-MD estimates are from 2.4 to 3.2 times larger than the OLS-MD estimate. We present these results primarily for comparison with those from other studies since, for the reasons given earlier, the coefficient on q cannot be interpreted in terms of adjustment costs. However, it *can* be interpreted

TABLE 3
Bond Rating Interaction Model: Estimates of α_1 and $\alpha_1 + \alpha_2$: Cash Flow Responses of Financially Unconstrained and Constrained Firms

	OLS	GMM3	GMM4	GMM5
		α_1		
1992	.378	−.082	.214	−.153
	(.088)	(.346)	(.120)	(.214)
1993	.369	−.005	−.008	−.168
	(.067)	(.156)	(.168)	(.171)
1994	.396	−.692	−.140	.273
	(.101)	(1.192)	(.180)	(.118)
1995	.465	.014	.197	−.207
	(.106)	(.218)	(.249)	(.223)
Minimum distance	.392	−.041	.105	.100
	(.061)	(.123)	(.098)	(.093)
		$\alpha_1 + \alpha_2$		
1992	.131	−.061	.063	−.090
	(.073)	(.150)	(.086)	(.120)
1993	.062	−.060	−.061	−.113
	(.031)	(.060)	(.063)	(.072)
1994	.102	−.404	−.147	.045
	(.051)	(.587)	(.098)	(.064)
1995	.071	−.194	−.086	−.323
	(.078)	(.143)	(.157)	(.148)
Minimum distance	.074	−.089	−.060	−.023
	(.026)	(.053)	(.052)	(.051)

Note.—Standard errors are in parentheses under the parameter estimates. For OLS we use the heteroskedasticity-consistent standard errors of White (1980). The standard errors for the sum of the cash flow coefficients are obtained via the delta method.

in terms of elasticities. Although we do not have a constant elasticity functional form and cannot observe marginal q, we can nevertheless conduct crude calculations using the median firm and our proxy for marginal q. For 1992–95 the OLS elasticities are .20, .20, .23, and .25, whereas the corresponding elasticities implied by the smallest GMM estimate for each year are .37, .65, .36, and .50. Note that while the response of investment to marginal q remains inelastic, it does increase noticeably.

We now turn to the central issue of liquidity constraints and the sensitivity of investment to cash flow. When comparing our results to those in the existing literature, note that the cash flow coefficient in our interaction term model gives the response for unconstrained firms, whereas the response for constrained firms equals the sum of the cash flow coefficient and the interaction term coefficient. Table 3 presents our estimates of these quantities.

The annual OLS estimates of the cash flow coefficient in table 3 are all positive and significant, as is the OLS-MD estimate. In contrast, only two of the 12 annual GMM estimates are significantly positive at the .05

level, and the GMM3-MD, GMM4-MD, and GMM5-MD are all insignificant. Since the GMM standard errors are typically larger than those for OLS, it is useful to point out that all but one of the annual GMM estimates are closer to zero than the OLS estimate of the same year, and all the GMM-MD estimates are closer to zero than OLS-MD. Since the estimated coefficient gives the response of unconstrained firms, the magnitude of the OLS-type estimates is unexpected; we shall remark on this anomaly below.

Table 3 also shows that the annual OLS estimates of the sum of the cash flow and interaction term coefficients, and the OLS-MD estimate that pools these estimates of the sum, are all positive and significant—the expected result for liquidity-constrained firms according to Fazzari et al (1988). As was the case for unconstrained firms, however, virtually all the GMM and GMM-MD estimates are insignificant at the .05 level; in fact, the only significant estimate is negative. Further, the majority of the GMM estimates are closer to zero than the corresponding OLS estimate, despite the fact that the OLS estimates, contrary to expectations, are much closer to zero than the OLS coefficients for unconstrained firms.

The GMM results clearly do not support the FHP hypothesis. On the other hand, the inconsistent OLS estimates cannot be said to support the FHP hypothesis either, since they indicate that liquidity-constrained firms are *less* sensitive to cash flow than unconstrained firms.[7] Although odd, this type of "wrong-way" differential cash flow sensitivity has been reported by other researchers. For example, Gilchrist and Himmelberg (1995), Kaplan and Zingales (1997), Kadapakkam, Kumar, and Riddick (1998), and Cleary (1999) all provide evidence that firms classified as unconstrained can have higher cash flow coefficients. In Section V below we show how untreated measurement error can generate spurious differential cash flow sensitivities, both the wrong-way pattern we experience and the "right-way" pattern predicted by the FHP hypothesis.

Next we examine the explanatory power parameter ρ^2, which, as the population R^2 for (8), measures the usefulness or approximate correctness of q theory. There exist three versions of this parameter in the interaction term model: one in which the quantities $\boldsymbol{\mu}_y$, $\boldsymbol{\mu}_x$, and $\mathrm{Var}(\mathbf{z}_i)$ appearing in (13) describe the a priori unconstrained-firm regime, a second version in which they describe the constrained-firm regime, and a third in which they describe the combined population. The combined population values for β, $E(\eta_i^2)$, and $E(u_i^2)$ appear in (13) in all three versions. Table 4 reports estimates of the third version only, since regime-

[7] It is worth noting that we obtain similar results with standard panel data techniques. The OLS estimates in first differences and instrumental variable estimates in levels and first differences give the same "wrong-way" differential cash flow sensitivity. Annual OLS estimates in deviations and first differences have this feature for every year.

TABLE 4
Bond Rating Interaction Model: Estimates of ρ^2, Population R^2 of the Investment Equation

	OLS	GMM3	GMM4	GMM5
1992	.228	.484	.417	.450
	(.034)	(.095)	(.089)	(.098)
1993	.211	.435	.385	.521
	(.042)	(.091)	(.074)	(.074)
1994	.219	.664	.459	.311
	(.041)	(.417)	(.071)	(.047)
1995	.201	.359	.271	.416
	(.036)	(.088)	(.085)	(.070)
Minimum distance	.215	.436	.405	.384
	(.025)	(.046)	(.046)	(.036)

Note.—We define the OLS estimate of ρ^2 to be the OLS R^2. Standard errors are in parentheses under the parameter estimates.

specific parameters are of little interest if the FHP hypothesis does not hold. The GMM estimates are 1.4 to 2.5 times higher than the corresponding OLS R^2, evidence that simple q theory explains investment considerably better than previously thought.

The large discrepancy between the GMM and OLS estimates above is due to the poor quality of the proxy for marginal q. Recall that proxy quality is described by τ^2, which ranges from zero to unity as the proxy ranges from worthless to perfect as a measure of marginal q. There are three versions of this parameter, analogous to those of the previous paragraph, and table 5 gives estimates of the combined population version. The estimates lie between .3 and .7, with an average of .46, suggesting that our proxy is quite noisy.

Table 6 presents the p-values for the J-statistics of this model. We find only one rejection at the .05 level, and its accompanying p-value is .046. We therefore conclude that our data on investment, q, and cash flow are consistent with the overidentifying restrictions generated by our errors-in-variables model.

Table 7 presents the p-values for the test of overidentifying restrictions associated with our minimum distance estimates (the minimum distance analogue to the J-test). The hypothesis tested is that the parameter value is constant over the four years. Time variation in α_1 and $\alpha_1 + \alpha_2$ is of interest since it violates the hypothesis that cash flow does not matter. Time constancy of α_1 is not rejected at the .05 level by any test, whereas that for $\alpha_1 + \alpha_2$ is rejected only by the GMM5-MD test, with a p-value of .046. The last result reflects the large negative GMM5 estimate for 1995. Time constancy of the adjustment cost parameter β is strongly rejected by the GMM5-MD test, but by no other tests. It should be noted that Monte Carlo results in Appendix B suggest that the time constancy

TABLE 5
Bond Rating Interaction Model: Estimates of τ^2, Population R^2 of the Measurement Equation

	GMM3	GMM4	GMM5
1992	.379	.535	.302
	(.096)	(.092)	(.071)
1993	.398	.418	.353
	(.059)	(.062)	(.064)
1994	.332	.469	.703
	(.155)	(.060)	(.124)
1995	.508	.593	.477
	(.068)	(.126)	(.065)
Minimum distance	.442	.463	.391
	(.048)	(.049)	(.050)

Note.—Standard errors are in parentheses under the parameter estimates.

tests (but not the GMM J-tests) may reject the null at rates very different from the nominal .05 level.

C. *Estimates of Other Models*

Table 8 reports minimum distance estimates derived from the firm size interaction term model. Table 8 also includes a model containing *both* the firm size and bond rating dummies and their interactions with cash flow. The coefficient sum estimate reported for this model is the sum of the cash flow coefficient and both interaction term coefficients. Comparing this sum to the cash flow coefficient characterizes the difference between the 215 firms that are liquidity constrained according to both the bond rating and firm size criteria and the 275 firms that are unconstrained according to both criteria.

The minimum distance estimates from table 8 reinforce the results of the previous section. No more than about 50 percent of the variation in measured q can be attributed to true marginal q, and correcting for measurement error approximately doubles the estimates of both β and ρ^2. Most important, apart from the significant *negative* estimate of α_1 from the firm size interaction model, cash flow does not matter, for either liquidity-constrained or unconstrained firms. In other words, our estimates do not support the FHP hypothesis of differential cash flow sensitivity.

As a final check on the robustness of our results, we examined whether using other measures of marginal q makes a difference. We reestimated all models using as alternative proxies the tax-adjusted versions of Tobin's q in Poterba and Summers (1983). This gave only small quantitative differences in our estimates and test statistics, and no qualitative differences in our inferences. Although in theory tax adjustments should improve the measurement of marginal q, in practice this improvement

TABLE 6
BOND RATING INTERACTION MODEL: p-VALUES OF J-TESTS OF OVERIDENTIFYING RESTRICTIONS

Year	GMM4	GMM5
1992	.251	.046
1993	.372	.141
1994	.704	.218
1995	.298	.506

appears negligible. The reason, we suspect, is that the adjustments use firm-level effective tax rate estimates that are themselves quite noisy. The gain from adjusting for taxes appears to be offset by this additional source of noise. We also tried a simple ratio of the market value of assets to the book value of assets, which is another proxy for marginal q used in the corporate finance literature. Here, too, we found no qualitative differences in our results.

V. Spurious Differences in Cash Flow Sensitivity

The large difference between the OLS-estimated cash flow sensitivities for our constrained and unconstrained firms is not due to different levels of measurement quality, but rather to differences in the variance of the cash flow ratio. We shall explain this phenomenon for split-sample estimation; the explanation for the interaction term models is essentially the same. We conjecture that other authors' estimates of differential cash flow sensitivity can be explained similarly.

Consider the element of (12) corresponding to the cash flow coefficient,

$$\alpha_1 = \mu_{y1} - \mu_{x1}\beta, \tag{22}$$

and recall that μ_{y1} and μ_{x1} are the probability limits of the OLS slope estimates from the regressions of y_i on z_{i1} and x_i on z_{i1}. Suppose that $\alpha_1 \equiv 0$ (cash flow does not matter), so that $\beta = \mu_{y1}/\mu_{x1}$. Further suppose, for a simple example, that the constrained sample is generated by a process in which $(\mu_{y1}, \mu_{x1}) = (.2, 5)$, whereas the unconstrained sample is generated by $(\mu_{y1}, \mu_{x1}) = (.6, 15)$. Then $\alpha_1 = .2 - 5\beta$ for the first group and $\alpha_1 = .6 - 15\beta$ for the other. Substituting the true value $\beta = \mu_{y1}/\mu_{x1} = .04$ into either gives $\alpha_1 = 0$. Substituting instead the attenuated value $\beta = .015$ gives $\alpha_1 = .125$ and $\alpha_1 = .375$. In words, the bias afflicting the marginal-q coefficient can be the same for both groups, yet estimates of the cash flow coefficient will tend to be much larger for one group than for the other.

In this example, μ_{y1} and μ_{x1} differ substantially across subsamples

TABLE 7
Bond Rating Interaction Model: p-Values of Parameter Constancy Tests

Parameter	OLS	GMM3	GMM4	GMM5
β	.437	.861	.133	.000
α_1	.688	.948	.247	.092
$\alpha_1 + \alpha_2$	.637	.578	.318	.046
ρ^2	.922	.782	.226	.052
τ^2	…	.473	.281	.003

whereas the ratio μ_{y1}/μ_{x1} remains constant. Our estimates $\hat{\mu}_{y1}$ and $\hat{\mu}_{x1}$ approximate these requirements, as table 9 shows for the bond rating split. Insight into the subsample differences is suggested by the identity $\mu_{x1} = \text{Cov}(x_i, z_{i1})/\text{Var}(z_{i1})$: for both the bond rating and firm size splits, the sample variance of z_{i1} is about three times larger for the constrained firms than for the unconstrained firms.

VI. Conclusion

We have tackled directly the problem of how, when using a noisy proxy for marginal q, to estimate the investment–marginal q relationship and test for the effects of financial constraints on investment. Using our approach, we find no evidence that cash flow belongs in the investment-q regression, whether or not firms are deemed financially constrained. It should not be surprising that our results differ from most of those in the literature on finance constraints. The motivation for including cash flow in the regression is not based on a formal model, but rather on a loose analogy with the "excess sensitivity" arguments in the consumption literature. The tenuous connection between these empirical tests and any formal theory suggests that significant coefficients on cash flow need not be evidence of finance constraints. Furthermore, as discussed in Chirinko (1993), the effects of liquidity constraints may be reflected in marginal q because they may cause managers' discount rates to rise.

We feel that our results go a long way toward rehabilitating q theory: despite its restrictive assumptions and simple structure, it apparently explains much more data variation than had been previously thought. Having said this, we must add that we do not think that q theory is the "last word" on the theory of investment. Other aspects of the investment process, such as learning, gestation lags, and capital heterogeneity, are intuitively important. Our results strongly suggest, however, that future work to evaluate their empirical importance should not ignore the problem of measurement error in marginal q. Further, as chronicled in Dixit and Pindyck (1994), the theoretical investment literature has been mov-

TABLE 8
MINIMUM DISTANCE ESTIMATES OF OTHER INTERACTION TERM MODELS

	OLS	GMM3	GMM4	GMM5
		β		
Firm size	.014	.046	.057	.041
	(.002)	(.007)	(.005)	(.004)
Bond rating and firm size	.013	.046	.042	.037
	(.002)	(.006)	(.004)	(.004)
		α_1		
Firm size	.226	−.125	−.190	−.012
	(.059)	(.089)	(.093)	(.074)
Bond rating and firm size	.400	−.049	.042	.091
	(.061)	(.125)	(.094)	(.091)
		α_1 + Coefficients on Interaction Term(s)		
Firm size	.043	−.062	−.061	−.002
	(.023)	(.051)	(.049)	(.046)
Bond rating and firm size	.043	−.061	−.024	.003
	(.023)	(.050)	(.044)	(.045)
		ρ^2		
Firm size	.210	.433	.451	.399
	(.029)	(.055)	(.055)	(.038)
Bond rating and firm size	.221	.442	.357	.389
	(.025)	(.048)	(.047)	(.033)
		τ^2		
Firm size		.442	.350	.440
		(.053)	(.041)	(.042)
Bond rating and firm size		.445	.449	.438
		(.048)	(.045)	(.043)

NOTE.—We define the OLS estimate of ρ^2 to be the OLS R^2. Standard errors are in parentheses under the parameter estimates.

TABLE 9
ESTIMATES OF μ_{x1}, μ_{y1}, AND Var (z_{i1})

	1992	1993	1994	1995
$\hat{\mu}_{y1}$:				
Constrained	.210	.120	.206	.233
Unconstrained	.566	.544	.620	.741
$\hat{\mu}_{x1}$:				
Constrained	5.686	4.362	7.429	8.966
Unconstrained	13.634	13.317	15.980	15.277
$\hat{\mu}_{y1}/\hat{\mu}_{x1}$:				
Constrained	.037	.027	.028	.026
Unconstrained	.042	.041	.039	.049
$\widehat{\text{Var}}(z_{i1})$:				
Constrained	.022	.023	.016	.013
Unconstrained	.008	.007	.006	.005

ing away from convex adjustment cost models, such as that underlying q theory, toward theories that incorporate irreversibility and fixed costs. Our results have obvious implications for testing such theories since many of their predictions are formulated in terms of marginal q. Finally, we caution that our results do *not* imply that investment is insensitive to external financial constraints. Rather, the popular method of looking at coefficients on cash flow may be misleading, and other possible tests for liquidity constraints should be explored further.

Appendix A

Data

The data are taken from the 3,869 manufacturing firms (standard industrial classification codes 2000–3999) in the combined annual and full coverage 1996 Standard & Poor's Compustat industrial files. We select our sample by first deleting any firm with missing data. To eliminate coding errors, we also delete any firm for which reported short-term debt is greater than reported total debt or for which reported changes in the capital stock cannot be accounted for by reported acquisition and sales of capital goods and by reported depreciation. We also delete any firm that experienced a merger accounting for more than 15 percent of the book value of its assets.

Appendix B

Monte Carlo Simulations

Readers may reasonably be skeptical of our empirical results since they are produced by unusual estimators and tests based on high-order moments. We therefore report some Monte Carlo simulations using artificial data very similar to our real data, generated with parameter values very close to our real GMM estimates. Some of the simulations use deliberately misspecified data generating processes (DGPs) to investigate test power. We report only those outputs that assist the reader in interpreting the results of Section IV.

Our first Monte Carlo simulation demonstrates that under correct specification the cross-section GMM estimates can be very accurate, as well as distinctly superior to OLS estimates made under the false assumption of correct measurement. We generate 10,000 samples of 737 observations, the size of a cross section of our actual data. Each observation has the form $(y_i, z_{i1}, z_{i2}, z_{i3}, x_i)$, where $z_{i2} = z_{i1}d_i$ and $z_{i3} = d_i$ is a dummy variable. The first and second moments of the simulation distribution for (z_{i1}, z_{i2}, z_{i3}) equal the averages, over our four cross sections, of the corresponding real-data sample moments from the bond rating interaction term model. We generate (y_i, x_i) according to (8) and (9), where $(\boldsymbol{\alpha}, \beta)$ and the distribution for $(\chi_i, \epsilon_i, u_i)$ are such that (i) the assumptions of Section III*A* are satisfied; (ii) $\alpha_1 = \alpha_2 = \alpha_3 = 0$ (cash flow does not matter); (iii) the OLS estimate (a_0, a_1, a_2, a_3, b) of the regression of y_i on $(1, z_{i1}, z_{i2}, z_{i3}, x_i)$ equals, on average over the simulation samples, the average OLS estimate over our four real cross sections; (iv) β, ρ^2, and τ^2 are close to the average real-data GMM estimates; and (v) the residuals $y_i - \mathbf{z}_i\hat{\boldsymbol{\mu}}_y$ and $x_i - \mathbf{z}_i\hat{\boldsymbol{\mu}}_x$ have, on average over the simulation samples, first and second moments equal to, and higher-

TABLE B1
A. Monte Carlo Performance of GMM and OLS Estimators

	OLS	GMM3	GMM4	GMM5
$E(\hat{\beta})$	.014	.040	.041	.040
$\text{MAD}(\hat{\beta})$	.026	.008	.006	.007
$\text{P}(\lvert \hat{\beta} - \beta \rvert \leq .01)$	.001	.738	.818	.755
$E(\hat{\alpha}_1)$	.387	−.007	−.012	−.001
$\text{MAD}(\hat{\alpha}_1)$	.387	.137	.112	.123
$\text{P}(\lvert \hat{\alpha}_1 - \alpha_1 \rvert \leq .1)$	.001	.529	.572	.514
$E(\hat{\alpha}_1 + \hat{\alpha}_2)$	.090	−.002	−.003	−.001
$\text{MAD}(\hat{\alpha}_1 + \hat{\alpha}_2)$	.090	.051	.047	.049
$\text{P}(\lvert (\hat{\alpha}_1 + \hat{\alpha}_2) - (\alpha_1 + \alpha_2) \rvert \leq .1)$	.599	.886	.907	.903
$E(\hat{\rho}^2)$	.239	.407	.421	.406
$\text{MAD}(\hat{\rho}^2)$	.134	.083	.072	.063
$\text{P}(\lvert \hat{\rho}^2 - \rho^2 \rvert \leq .1)$	.230	.672	.734	.794
$E(\hat{\tau}^2)$	…	.480	.423	.403
$\text{MAD}(\hat{\tau}^2)$	…	.091	.098	.105
$\text{P}(\lvert \hat{\tau}^2 - \tau^2 \rvert \leq 0.1)$	…	.713	.819	.825

B. Average of the Sample Moments

	Variance	Third Standardized Moment	Fourth Standardized Moment	Fifth Standardized Moment
	From 10,000 Trials			
$y - \hat{\boldsymbol{\mu}}_y \mathbf{z}$	.010	3.042	22.295	214.59
$x - \hat{\boldsymbol{\mu}}_x \mathbf{z}$	5.326	2.539	14.950	117.61
	From Our Four Years of Real Data			
$y - \hat{\boldsymbol{\mu}}_y \mathbf{z}$	.010	2.847	19.351	159.55
$x - \hat{\boldsymbol{\mu}}_x \mathbf{z}$	5.349	2.954	14.213	84.52

Note.—The true model is

$$y_i = .023 + .04\chi_i + 0z_{1i} + 0d_i z_{1i} + 0d_i + u_i, \quad \rho^2 = .372,\ \tau^2 = .437.$$

The estimated model is

$$y_i = \alpha_0 + \beta\chi_i + \alpha_1 z_{1i} + \alpha_2 d_i z_{1i} + \alpha_3 d_i + u_i.$$

The sample size is 737, with 10,000 trials. In panel B, the nth standardized moment is defined as the nth moment divided by the standard deviation raised to the nth power.

order moments comparable to, the corresponding average sample moments from our real data; see panel B of table B1.

Panel A of table B1 reports estimator performance for the parameters of interest from the interaction term model. We report the mean of an estimator, its mean absolute deviation (MAD), and, except for β, the probability an estimate is within .1 of the true value. Because β is quite small, we report the probability that its estimates are within .01 of the true value. By every criterion the GMM estimators are clearly superior to OLS.

We also record the actual sizes of GMM tests based on asymptotic .05 signif-

TABLE B2
Monte Carlo *J*-Test Rejection Rates

	GMM4	GMM5
Size	.040	.068
Power:		
Nonlinear functional form	.326	.520
Heteroskedastic regression error	.342	.533
Heteroskedastic measurement error	.400	.506
Mismeasured capital stock	.230	.426

Note.—All rejection rates are calculated using an asymptotic .05 significance level critical value.

icance level critical values. Those for the *J*-tests are given in table B2, where they are seen to be approximately correct. Those for the *t*-tests of the nulls $\alpha_1 = 0$ and $\alpha_1 + \alpha_2 = 0$ versus positive alternatives are not in the table, but their minimum is .068, evidence of a tendency to overreject that supports our findings of insignificance in Section IV*B*.

We next simulate four different misspecified DGPs to investigate the power of the *J*-test. Each is obtained by introducing one type of misspecification into the correctly specified "baseline" DGP described above. We make y_i depend nonlinearly on χ_i, or we mismeasure the capital stock (modeled by multiplying each (y_i, x_i, z_{i1}) from the baseline DGP sample by an i.i.d. lognormal variable), or we make the standard deviation of u_i or ϵ_i depend on $(\mathbf{z}_i, \chi_i)$ (since, e.g., failure of conditions for independence of u_i and χ_i given in Sec. III*A* will cause $\text{Cov}(z_{ij}, u_i^2)$ to be nonzero). We limit the degree of each misspecification so that the absolute biases in the GMM estimates of β, α_1, α_2, and ρ^2 are no larger than the absolute differences between the means of the OLS and GMM estimates from the baseline DGP. Table B2 shows that the GMM5 *J*-test exhibits usefully large power, ranging from .426 to .533. These numbers clearly depend on how a misspecification is "specified" in our experiment; some specifications will produce more power and others will produce less. Also, we did not combine misspecifications, which we suspect would increase test power.

Table B3 refers to the identification test of Section III*C*. Power numbers are

TABLE B3
Monte Carlo Performance of Identification Tests

Observations	Size	Power
	Interaction Term Model	
737	.043	.516
	Basic Model	
737	.044	.507
500	.047	.377
200	.049	.147

Note.—All rejection rates are calculated using an asymptotic .05 significance level critical value. The "basic model," which excludes dummies and interaction terms, is the model we fit to split samples. The null hypothesis is $\beta = 0$ or $E(\eta_i^3) = 0$ or both.

TABLE B4
Monte Carlo Performance of GMM-MD and OLS-MD Estimators

	OLS	GMM3	GMM4	GMM5
$E(\hat{\beta})$	.014	.039	.039	.038
$\text{MAD}(\hat{\beta})$	.026	.003	.003	.003
$\text{P}(\lvert \hat{\beta} - \beta \rvert \leq .01)$	.000	.982	.985	.971
$E(\hat{\alpha}_1)$	.393	−.006	−.007	.003
$\text{MAD}(\hat{\alpha}_1)$	.393	.132	.100	.113
$\text{P}(\lvert \hat{\alpha}_1 - \alpha_1 \rvert \leq .1)$	.000	.570	.626	.556
$E(\hat{\alpha}_1 + \hat{\alpha}_2)$	.091	.000	−.000	.002
$\text{MAD}(\hat{\alpha}_1 + \hat{\alpha}_2)$	.091	.037	.030	.032
$\text{P}(\lvert (\hat{\alpha}_1 + \hat{\alpha}_2) - (\alpha_1 + \alpha_2) \rvert \leq .1)$	.675	.953	.982	.978
$E(\hat{\rho}^2)$	.229	.412	.415	.397
$\text{MAD}(\hat{\rho}^2)$	.189	.055	.053	.043
$\text{P}(\lvert \hat{\rho}^2 - \rho^2 \rvert \leq .1)$	.000	.871	.889	.944
$E(\hat{\tau}^2)$	…	.464	.457	.439
$\text{MAD}(\hat{\tau}^2)$	…	.042	.035	.033
$\text{P}(\lvert \hat{\tau}^2 - \tau^2 \rvert \leq .1)$	…	.944	.981	.984

taken from the baseline DGP. Size numbers are obtained by replacing the baseline regressors with normal variates having the same first and second moments. The test is accurately sized at all sample sizes and has good power at the full-sample size but low power at a sample size like that of our smaller split samples.

Finally, table B4 confirms that the GMM-MD estimators can outperform the cross-section GMM estimators. Each of the 737 observations in a sample from this simulation consists of a draw from the baseline DGP and three additional draws conditional on the value of the financial constraint dummy from draw 1. Time dependence is generated by decomposing u_{it} into two equal-variance components: one that is fixed over the four cross sections and one that is i.i.d. The

TABLE B5
Monte Carlo Performance of Parameter Constancy Tests

	OLS	GMM3	GMM4	GMM5
	Size			
β	.103	.094	.173	.283
α_1	.071	.006	.011	.021
$\alpha_1 + \alpha_2$	.057	.021	.027	.028
ρ^2	.065	.117	.113	.197
τ^2	…	.103	.106	.191
	Power			
β	.211	.287	.404	.513
α_1	.290	.368	.513	.540
$\alpha_1 + \alpha_2$	.934	.684	.763	.782
ρ^2	.980	.810	.858	.920
τ^2	…	.502	.682	.776

actual size of the .05 level time constancy test associated with each estimator is given in table B5; note the very poor approximation for some of these tests. The actual sizes of the one-sided t-tests of $\alpha_1 = 0$ and $\alpha_1 + \alpha_2 = 0$ are not in the table, but their minimum is .127. The power numbers in table B5 are obtained by altering the DGP so that $\alpha_1 = \alpha_1 + \alpha_2 = .26$, $\beta = .05$, $\rho^2 = .547$, and $\tau^2 = .696$ in even-numbered "years."

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