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Is It Inefficient Investment that Causes the Diversification Discount?

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ABSTRACT

Diversified conglomerates are valued less than matched portfolios of pure-play firms. Recent studies find that this diversification discount results from conglomerates' inefficient allocation of capital expenditures across divisions. Much of this work uses Tobin's q as a proxy for investment opportunities, therefore hypothesizing that q is a good proxy. This paper treats measurement error in q. Using a measurement-error consistent estimator on the sorts regressions in the literature, I find no evidence of inefficient allocation of investment. The results in the literature appear to be artifacts of measurement error and of the correlation between investment opportunities and liquidity.

DIVERSIFIED CONGLOMERATES APPEAR TO TRADE at a discount relative to matched portfolios of pure-play firms. Studies documenting this stylized fact include Lang and Stulz (1994), Berger and Ofek (1995), Comment and Jarrell (1995), and Servaes (1996). Recent empirical work has sought to find the source of this diversification discount. Building on the theoretical work in Milgrom and Roberts (1990), Stein (1997), and Scharfstein and Stein (2000) on the inefficiency of internal capital markets, this literature has asked specifically whether diversified firms allocate their capital expenditures inefficiently across divisions. The canonical answer has been yes and has been supported by two types of evidence. First, studies such as Lamont (1997), Shin and Stulz (1998), Shin and Park (1998), and Rajan, Servaes, and Zingales (2000) all find that internal capital markets in conglomerates transfer funds across divisions in a suboptimal manner. Second, studies such as Berger and Ofek and Scharfstein (1998) provide evidence suggesting that the divisions of conglomerates do not respond adequately to investment opportunities, in comparison to single-segment firms.

However, these findings about divisional investment may be an artifact of measurement error. Most of these studies rely on Tobin's q (the market value of the capital stock divided by its replacement value) as a proxy for investment opportunities, and Tobin's q is likely to be a poor proxy. Shin and Stulz

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(1998), Shin and Park (1998), and Scharfstein (1998) use regressions of investment on Tobin's q and cash flow. Lamont (1997) uses a similar approach, though he allows sales growth to proxy for investment opportunities instead of q. Berger and Ofek (1995) and Rajan et al. (2000) use different techniques, but still employ Tobin's q. The goal of this paper is to determine whether the measurement error problem has affected our understanding of the allocation of investment across conglomerate divisions.

The use of Tobin's q starts from the important observation, made by all of these studies, that some control for investment opportunities is essential for both identifying funds transfers across divisions of a conglomerate and isolating efficient funds transfers from inefficient ones. Otherwise, we may see evidence of funds transfers across divisions, but we may infer that they are inefficient because we cannot observe the investment opportunities of the divisions involved. Further, without a good proxy for investment opportunities, identifying funds transfers may itself be difficult. If we cannot observe investment opportunities accurately, cash flows in one division will appear to be a significant explanatory variable for investment in another, not because of internal capital markets, but merely because cash flows in the first division are correlated with investment opportunities in the second. This correlation can occur even in diversified firms, because, as pointed out in Chevalier (2000), the divisions of these firms can be related. For example, vertical relationships and the existence of common management can link the fortunes of divisions in different industries.

As hypothesized above, Tobin's q is unlikely to be an adequate control for investment opportunities. Most standard intertemporal models of investment imply that investment opportunities should be measured by an unobservable quantity: marginal q. This variable is usually defined as the firm manager's expectation of the present discounted value of the future marginal product of capital.[1] As has been widely discussed in the corporate finance and macroeconomics literatures, observable measures of Tobin's q may diverge substantially from unobservable marginal q.

Given the potential for serious measurement error in q, this paper addresses the issue of whether this measurement error may have been distorting the inferences made in previous studies about capital allocation within conglomerates. I revisit two of the main findings in this empirical literature: that a segment's investment depends on the cash flow of other segments, and that segments do not respond as strongly to investment opportunities as do single-segment firms. I scrutinize these findings by using an econometric technique that remedies the measurement-error problem, both with the intent of seeing if the qualitative conclusions of this work hold up and with the intent of developing a better understanding of the cross-subsidization problem.

Specifically, I examine the results from employing the measurement-error consistent estimators in Erickson and Whited (2000a) (hereafter EW estimators) on the sorts of regressions used in the empirical diversification literature. I opt for this technique, because, as I argue below, more conventional

[1] See, for example, Lucas and Prescott (1971), Hayashi (1982), and Abel and Eberly (1994).

remedies such as instrumental variables require assumptions that are both implausible and untestable. The EW estimators use the information contained in the third- and higher-order moments of the joint distribution of the observed regression variables. The idea is simple. It is well known that the first- and second-order moments of the observable data cannot be used to provide consistent regression slope estimates in the presence of measurement error. In contrast, Geary (1942) shows that it is possible to use third- and higher-order moments to deliver consistent estimates. Estimators of this type have rarely been used in practice because of skepticism about their precision and robustness to misspecification (see, e.g., Aigner et al., 1984, pp. 1338–1339). This skepticism may be justified with regard to ordinary method-of-moments estimators initially used in this approach. However, the EW technique improves upon Geary's idea by using GMM (Hansen, 1982) on equations that express moments of the observable data in terms of moments of unobservable marginal q and the unobservable regression and measurement errors. Using GMM allows for increased estimator precision. It also allows for the use of the GMM J-test of overidentifying restrictions whenever the number of moment equations exceeds the number of parameters to be estimated. The J-test can be used as a tool for detecting departures from the assumptions required for estimator consistency.

My findings can be summarized as follows. First, I use standard techniques to replicate the results in the empirical papers discussed above with data from divisions of U.S. conglomerates. I find that the investment of segments of diversified conglomerates responds less strongly to investment opportunities (proxied by Tobin's q) than the investment of stand-alone firms. I also find evidence of cross-subsidization of investment: Segment investment responds not only to its own cash flow, but also to other-segment cash flow. When I use the EW estimators, the results are strikingly different. Any evidence of differential responses to q or of cross-subsidization disappears. I conclude that a firm's internal capital market may be inefficient, but inefficiency does not manifest itself in investment-q regressions. Instead, the econometric model attributes observed cash flow sensitivities and differential q sensitivities to measurement error.

It is likely that cash flow sensitivities have appeared significant because typical proxies for divisional q fail to capture investment opportunities, and because investment opportunities are positively correlated with cash flow—not only a division's own cash flow, but other divisions' cash flows as well. Put differently, because marginal q is a summary statistic for investment, it captures the effects of the internal capital market on the manager's expectation of investment opportunities. Once we remove the noise from a proxy for marginal q, divisional cash flow movements no longer have any *incremental* explanatory power for investment.

A number of related papers have challenged the hypothesis that inefficient internal capital markets cause the diversification discount, as well as the maintained hypothesis that conglomerates trade at a discount. Maksimovic and Phillips (2000) find evidence in a large sample of U.S. manufacturing plants that it is the equilibrium distribution of comparative advantage across firms in an industry, and not agency costs, that gives rise to the discount. Chevalier (2000) looks at the investment behavior of firms prior to the mergers that combined them into diversified conglomerates. Her main finding is that the investment behavior of conglomerates documented in the literature occurs in the individual firms *before* they undertook a diversifying merger. Fluck and Lynch (1999) present a theoretical model in which low-value firms diversify but still trade at a discount to single-segment firms, even though the diversification creates value. In a similar vein, Graham, Lemmon, and Wolf (2000) demonstrate that about half of the observed diversification discount is due to the discount at which target firms traded before they were acquired by conglomerates.

This paper enters this story by providing direct evidence about the role of measurement error in understanding how internal capital markets affect divisional investment. This contribution differs from that of its immediate predecessor, Erickson and Whited (2000b). This earlier paper also uses high-order moment estimators on investment-q regressions; however, it explores a different issue: the effects of external financial constraints on investment. Specifically, the paper examines whether coefficients on cash flow are different across groups of firms differentiated by their access to external financial markets. In contrast, the current paper treats the issue of whether inefficient internal capital markets cause the segments of diversified conglomerates to invest suboptimally. The methods are similar because investment-q regressions have been used in both literatures. Here, however, instead of looking at cash-flow sensitivities, I focus on the response of segment investment to *other*-segment cash flow, and a comparison of the response of investment to q for single-segment firms and for segments of conglomerates.

I organize the paper as follows. Section I describes the construction of observable measures of q, presents the sources of measurement error in q, and discusses the effects of this measurement error on investment-q-cash flow regressions. Section II describes the data. Section III examines the data from a traditional standpoint, and Section IV discusses the EW estimator and presents the results from applying it. Section V concludes by offering alternative methods for testing the efficiency of internal capital markets. The Appendix contains a Monte Carlo experiment documenting the performance of the EW estimators.

I. Measurement

A. Firm Q

Following the literature on corporate diversification, I construct Tobin's q as the market value of assets divided by the book value of assets, both measured at the beginning of the period.[2] I construct the market value of assets

[2] Constructing Tobin's q using the algorithms in Lewellen and Badrinath (1997) or Erickson and Whited (2000b) does not alter the qualitative results significantly. However, because these algorithms have greater data requirements, the number of useful observations per year drops dramatically.

by adding to the book value of assets the market value of common equity and subtracting the book value of common equity and balance-sheet deferred taxes.

Recall that the market-to-book ratio serves as a proxy for unobservable marginal q. To motivate my empirical investigation, I first discuss how using this proxy to measure marginal q can result in serious error. The market-to-book ratio and marginal q will be equal only under stringent conditions. First, marginal q must equal a quantity called average q: the manager's expectation of the value of the firm's capital stock divided by its replacement value. Hayashi (1982) has shown that necessary conditions for equality include perfect competition and linearly homogeneous technology. Therefore, as shown by Hayashi, imperfect competition causes average q to exceed marginal q, and as shown in Abel and Eberly (1994), nonconstant returns to scale can bias average q up or down. Second, average q must equal Tobin's q: the stock market's valuation of the firm's capital stock divided by its replacement value. However, market inefficiencies or information asymmetry may cause the manager's valuation of capital to diverge from the market valuation in either direction. Finally, the market-to-book ratio must equal Tobin's q. This equality will hold exactly only if the firm's only assets are capital goods and if the market value of the firm's liabilities, especially debt, are equal to their book value.

B. Segment Q

I next turn to the issue of constructing q for a nontraded segment. A number of alternatives are available. The measure most frequently used in the empirical diversification literature is the median of the Tobin's qs of the single-segment firms in a particular segment's three-digit industry. Although popular, this proxy may be quite poor, since a segment's true q is unlikely to lie at the industry median. Indeed, the existence of a diversification discount implies that a segment's q will tend to lie below the industry median. This observation highlights the importance of addressing the issue of measurement error.

The "median q" poses a special problem for the EW technique, because this technique requires a homoskedastic measurement error, and because the difference between median q and true segment q, the measurement error, will be heteroskedastic. To see this point, note that all of the segments in a particular three-digit industry will be assigned the same q and that the variance of true investment opportunities within different three-digit industries is likely to vary widely. The differences in variance could arise either from differences in the breadth of the definition of the industry or in the degree of competition among the firms in the industry. Indeed, using the EW estimators on regressions containing median q produces strong rejections of the overidentifying restrictions implied by the model's assumptions.

Another alternative is to use a "fitted q," as in Billett and Mauer (1999). Constructing a fitted q requires estimating a regression of Tobin's q on observable income statement items for single segment firms and then imputing a segment-level q by using the resulting regression coefficients on the same segment-level income statement items. Note that unless the firm-level regression has an R^2 of one, fitted q will still contain measurement error. In my firm-level data, I obtain R^2s between 20 and 35 percent and a distribution for fitted q that has low skewness and kurtosis. This feature of the distribution is of concern, because the EW estimators are not identified and cannot be used if the mismeasured regressor is normally distributed. When I test the null that the model is unidentified using the test described in Erickson and Whited (2000a), I am unable to reject the null of an unidentified model. I cannot, therefore, use the EW estimators on a fitted q.

Because both median q and fitted q may contain measurement error, because I want to assess the effects of measurement error, and because neither of these measures can be used with an estimator that can accomplish this goal, I construct my own measure. As shown below, this alternative proxy yields qualitatively similar conclusions as the other measures when used in standard regressions. However, this alternative proxy does not suffer from the identification and misspecification difficulties exhibited by the other two proxies. To construct what I call "adjusted q," I use median q as a starting point and then build upon the finding in Lang and Stulz (1994) that the Tobin's q of a conglomerate is lower than the weighted average of its segments' industry-median qs. The existence of such a discount implies that there exist important differences in productivity and investment opportunities between stand-alone firms and conglomerate segments. Therefore, a segment's q cannot lie at the median, but will tend to lie below it. To account for this tendency, I adjust the industry q to reflect the diversification discount or premium that exists in the conglomerate to which the segment belongs. For example, if the conglomerate's q is above the weighted average of its segments' qs, I increase the segments' qs proportionately until the two quantities are equal. I conjecture that this adjustment alleviates heteroskedasticity in the measurement error because all of the segments in a particular industry are no longer being assigned the same q. This procedure has the obvious drawback that the diversification discount may be reflected more strongly in one segment's q than in another's, especially given the evidence in Maksimovic and Phillips (2000) that there exist productivity differences across the segments of most conglomerates. However, this measure may be an improvement over the industry median q, since it has a firm-level component.

C. Other Regression Variables

For both segments and firms, I measure investment as reported capital expenditures and cash flow as reported operating income plus reported depreciation, where both variables are normalized by beginning-of-period total firm assets. I use assets because most intertemporal models of investment with homogeneous technology provide empirical predictions in terms of the ratio of investment to assets, for example, Abel and Eberly (1994). Also, I choose to scale both firm and segment variables by total firm assets. This choice in part reflects conformity with the existing literature: Chevalier (2000) and most of the regressions in Shin and Stulz (1998) use total assets. This choice can also be couched in terms of a theoretical argument. If investment decisions in conglomerates are made at the firm level, then firm assets should be the appropriate scaling variable, and if these decisions are made at the segment level, then segment assets are more appropriate. Nonetheless, ambiguity in the theoretical guidance for choosing a scaling variable brings up the important issue of measurement error in a scaling variable, which I discuss below.

D. The Effects of Measurement Error

The potential for error is great, especially since many different sources may be contributing to measurement error in marginal q. Further, evidence is starting to emerge that it is empirically important. Lewellen and Badrinath (1997) present an example in which several standard measures of q are highly inaccurate. Taking an alternative approach, Erickson and Whited (2000b) use the same estimator as the one used here, but examine a different proxy for q—one widely used in the macroeconomics literature. They estimate the percentage of the variation in this proxy that is due to true marginal q to be only about 40 percent.

Because measurement error is likely to be severe, its effects are likely to be important. It is useful, therefore, to review the direction of the biases that result from measurement error, where I will focus on the classical error-in-variables model: the econometric model upon which I base all of my work. This model is a linear regression in which one or more of the explanatory variables is measured with error, and in which the difference between the true unobserved regressor and its proxy (the measurement error) is uncorrelated with all other variables in the model. In the one-mismeasured version of this model, the 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 towards zero. Also, coefficient estimates for perfectly measured regressors can be biased in either direction, depending on the direction of their correlation with the mismeasured regressor. Therefore, additional variables that do not belong in the regression may appear significant.

As applied to the problem of understanding the investment policies of diversified firms, these results on bias imply that observed sensitivities of investment to divisional cash flow may be spurious. For example, divisional cash flow will be positively correlated with true marginal q, since marginal q is the expected present discounted value of the marginal cash flows generated by capital. Further, if the divisions of a conglomerate are in any way related to one another, the q of one division may be correlated with cash flows of other divisions. These positive correlations imply that the observed cash-flow sensitivities will be biased upward. The classical errors-in-variables model also implies that if the proxy for q of a stand-alone firm is a better proxy for investment opportunities than the proxy for q of a segment, then measurement error may be responsible for the difference in sensitivity of investment to q between stand-alone firms and segments of conglomerates.[3]

These conjectures are based on the bias results from the classical errors-in-variables model. One important possibility to consider is that the true measurement-error process is not classical, for example, because the measurement error is correlated with cash flow or because all of the regression variables are scaled by a variable that is itself mismeasured. No research has been done to devise estimators for cases such as these, and the coefficient biases are impossible to assess without the researcher using prior information on the quality of the proxy for marginal q, for example, Krasker and Pratt (1986) and Erickson (1993). Given this ambiguity, formal data analysis is necessary to accomplish two tasks. First, although Erickson and Whited (2000b) find that the classical model is an acceptable characterization of firm investment, the question remains open as to whether the classical model characterizes divisional investment regressions, primarily because these latter regressions contain more regressors, use different data, and use a very different proxy for marginal q. Second and more importantly, I seek to determine if classical measurement error can explain the results found by previous studies.

II. Data

My data are from the nonfinancial firms in the combined annual and full coverage 1999 Standard and Poor's COMPUSTAT industrial files that are also covered by the most recent COMPUSTAT business information file, which contains data from 1992 through 1998. I omit all firm- and segment-level observations whose primary SIC classification is between 6000 and 6999, since Tobin's q will be inappropriate for highly levered financial firms.

The COMPUSTAT Industrial Segment (CIS) database reports the line of business information for COMPUSTAT firms. These segment data are generated as part of the disclosure requirements mandated under the Statement of Financial Accounting Standards No. 14. This statement requires firms to report material segment information such as operations in different four-digit SIC code industries for fiscal years ending after December 15, 1977. If an industry segment makes up more than 10 percent of the firm's total revenues, operating income, or identifiable assets, the firm is required to provide data on five variables by each industry segment: net sales, oper-

[3] Note that attempts to assess the impact of diversification on Tobin's q do not suffer from measurement error biases. In this case, Tobin's q is a "left-hand-side" variable, and the presence of measurement error will not bias coefficient estimates. In fact, it will tend to reduce the significance of any results, which means that the findings of Lang and Stulz (1994) and Bodnar, Tang, and Weintrop (1997) are likely to be more statistically significant than they report.

ating income, depreciation, capital expenditures, and identifiable assets. The basis of segmentation is left to the discretion of the firm, but is generally recorded at the four-digit SIC level.

I select my sample by first deleting any firm-year observations with missing data. To detect coding errors, I also delete any observation for which reported debt due in years one through five is greater than reported total debt, and for whom reported changes in the capital stock cannot be accounted for by reported acquisition and sales of capital goods and by reported depreciation. Third, I delete any observation for which the firm experienced a merger accounting for more than 15 percent of the book value of its assets. Finally, I delete any observations if the sum of segment assets deviates by more than 25 percent from reported total firm assets.

My sample period runs from 1993 through 1998. I delete the first year of data because I normalize all of my regression variables by beginning-of-period assets. I end up with between 1,772 and 2,818 single-segment firms per year, between 586 and 695 multiple-segment firms per year, and between 1,565 and 1,871 segments of multiple-segment firms per year.

The first panel of Table I reports descriptive statistics for my firm-level data, where I have divided my firms into single-segment firms and multiple-segment firms. Not surprisingly, the conglomerates are substantially larger than the single-segment firms. Further, the single-segment firms have higher rates of investment, higher values for Tobin's q, and higher cash flow than the conglomerates. In other words, they appear to be growing faster, and the higher values for q indicate that the stock market thinks they have better investment opportunities. The lower panel of Table I reports similar statistics for my segment-level data. Notice first that the segments of conglomerates tend to be larger than stand-alone firms: the first row shows that the mean size of a segment of a diversified firm is approximately 1.5 times the mean size of a stand-alone firm. Also notice that the diversification discount is evident in the adjusted qs, whose means and medians are lower than the median qs.

III. Regressing Investment on *Q* and Cash Flow

In all of the regressions that follow, I treat my data as six separate (but not independent) cross sections. In particular, I have not used the feature that they constitute a panel to treat the potential problem of firm-specific fixed effects, which manifest themselves in cross sections as correlations between regressor and error. To remedy the bias arising from the fixed-effects problem, it is possible to transform the observations for each firm into deviations from that firm's average or into first differences. For my data, however, after either transformation, I find no evidence that the resulting models satisfy the identifying assumption, described below, required by the EW estimator. I therefore use untransformed data—a decision that does have theoretical support. Dynamic investment models suggest that fixed effects may not be important for investment-q regressions, because these

Table I

Descriptive Statistics

Calculations are based on a sample of manufacturing firms from the combined annual and full coverage 1999 Standard and Poor's COMPUSTAT industrial files that are also covered by COMPUSTAT's Business Information File. The sample period is 1993 through 1998. Industry q is defined as the median value of the Tobin's qs of single-segment firms belonging to a particular segment's three-digit industry. Adjusted q is calculated by scaling industry q up or down depending on whether the firm to which the segment belongs trades at a premium or a discount.

	Single-segment Firms	Multiple-segment Firms
Firm-level statistics		
Total assets (millions of 1992 dollars)		
Mean	800.107	3756.905
Median	74.759	526.208
Investment/assets		
Mean	0.070	0.085
Median	0.050	0.061
Tobin's q		
Mean	1.173	1.031
Median	0.908	0.885
Cash flow/assets		
Mean	0.159	0.194
Median	0.171	0.188
Segment level statistics		
Identifiable assets (millions of 1992 dollars)		
Mean		1256.084
Median		200.532
Investment/firm assets		
Mean		0.031
Median		0.018
Industry q		
Mean		1.678
Median		1.593
Adjusted q		
Mean		1.514
Median		1.324
Cash flow/firm assets		
Mean		0.055
Median		0.043

models predict that marginal q is a summary statistic for investment. Therefore, any time-invariant variables that are relevant for investment should be captured by marginal q. In addition, because the measurement error model used below generates overidentifying restrictions that are disrupted by regressor-error correlations, I can, and do, *test* for their presence.

Before correcting for measurement error, I first present OLS regressions that are comparable to those found in the literature. This exercise has two important motivations. As mentioned above, I treat my unbalanced panel of firms from COMPUSTAT as separate cross sections. Because much of the

Table II

Firm and Segment Regressions of Investment on Q and Cash Flow

Calculations are based on a sample of single segment nonfinancial firms from the combined annual and full coverage 1999 Standard and Poor's COMPUSTAT industrial files and on a sample of nonfinancial segments from COMPUSTAT's Business Information File. The sample period is 1993 through 1998. Investment and cash flow are scaled by total firm assets. Firm q is calculated at the ratio of the market value of assets to the book value of assets. Segment q is calculated as the median of the qs of the firms in a segment's three-digit industry, scaled by the firm's diversification discount. Regressions are run using OLS. White (1980) standard errors are in parentheses under the parameter estimates.

	q	Cash Flow	R^2
Firms			
1993	0.009	0.135	0.142
	(0.002)	(0.012)	
1994	0.005	0.152	0.160
	(0.002)	(0.010)	
1995	0.006	0.137	0.131
	(0.003)	(0.010)	
1996	0.007	0.118	0.135
	(0.002)	(0.009)	
1997	0.004	0.128	0.138
	(0.002)	(0.007)	
1998	0.005	0.103	0.068
	(0.002)	(0.010)	
Segments			
1993	0.003	0.189	0.117
	(0.002)	(0.028)	
1994	0.003	0.208	0.131
	(0.001)	(0.029)	
1995	0.004	0.270	0.183
	(0.002)	(0.028)	
1996	0.003	0.214	0.129
	(0.002)	(0.028)	
1997	0.005	0.183	0.154
	(0.002)	(0.028)	
1998	0.005	0.151	0.086
	(0.002)	(0.019)	

literature uses fixed-effect regressions, my intent, in part, is to demonstrate that using cross sections provides roughly the same flavor of results as the panel regressions. Also, I wish to demonstrate that my use of adjusted segment q does not qualitatively alter the results found by other researchers.

Table II compares the investment of single-segment firms to that of the segments of multisegment firms. The dependent variable is the ratio of capital expenditures to the replacement value of total firm assets for both the single-segment firms and the segments. In the case of the single-segment firms, I use their own qs in the regressions, and for the segments, I use the adjusted qs. Investment appears to respond almost twice as strongly to q for the single-segment firms as it does for the segments. The average coefficient on q in the firm regressions is 0.0062, whereas in the segment regressions, it is only 0.0038. Further, in three of the six years, the coefficient on q is statistically significantly higher for the firms than for the segments. Scharfstein (1998) argues that this sort of result implies that managers of single-segment firms appear to pay much more attention to investment opportunities than do the managers of segments within conglomerates. Put more simply, low-q segments tend to overinvest relative to stand-alone firms and high-q segments tend to underinvest. The inference is then drawn that inefficient investment may contribute to the diversification discount.

Next, note that the coefficient on cash flow is significant in both sets of regressions but that the coefficients are larger for the segments than for the single-segment firms. To interpret this result, recall the argument in Fazzari, Hubbard, and Petersen (1988) that even with q fixed, external financial constraints will cause investment to respond strongly to cash flow. Taking this argument at face value, these results indicate that the segments appear to be more liquidity constrained than the single-segment firms, which points to the inefficient functioning of internal capital markets within conglomerates. In other words, stand-alone firms cannot always succeed in obtaining funds from external capital markets, but segments have a more difficult time obtaining funds from headquarters. This result stands in contrast to the finding in Shin and Stulz (1998) that firms have higher cash flow sensitivities than segments. However, my result is an artifact of my decision to scale investment and cash flow by total assets instead of by segment assets. When I do scale by segment assets, I can replicate the results in Shin and Stulz (1998).[4]

To examine another commonly used test for the functioning of internal capital markets, I include other-segment cash flow in the segment regressions. Table III shows that, consistent with the results in Shin and Stulz (1998), although other-segment cash flow is significant, it is a less important determinant of investment than own-segment cash flow. Shin and Stulz argue that if the internal capital market is functioning well, the cash flow of the firm as a whole should matter more for a segment's investment than its own cash flow. Therefore, other-segment cash flow should carry a larger coefficient than own cash flow. Taking this argument at face value, the results here imply that the internal capital market functions, but not well. Headquarters appears to transfer funds from one segment to another, but

[4] As shown in Maksimovic and Phillips (2000), larger segments tend to be more profitable and therefore have both higher cash flow and investment. Scaling by firm assets allows this association between cash flow and investment to be apparent. Scaling by segment assets will tend to reduce the investment and cash flow variables for large segments and increase these variables for small segments. This effect will then reduce the cross-sectional association between investment and cash flow.

Table III

Segment Regressions of Investment on *Q*, Cash Flow, and Other Segment Cash Flow

Calculations are based on a sample of nonfinancial segments from COMPUSTAT's Business Information File. The sample period is 1993 through 1998. Segment q is calculated as the median of the qs of the firms in a segment's three-digit industry, scaled by the firm's diversification discount. Investment, own-segment cash flow and other-segment cash flow are scaled by total firm assets. Regressions are run using OLS. White (1980) standard errors are in parentheses under the parameter estimates.

	q	Cash Flow	Other Cash Flow	R^2
1993	0.001	0.201	0.065	0.130
	(0.002)	(0.031)	(0.035)	
1994	0.002	0.217	0.069	0.149
	(0.002)	(0.030)	(0.021)	
1995	0.001	0.295	0.117	0.220
	(0.002)	(0.028)	(0.023)	
1996	0.002	0.217	0.055	0.144
	(0.002)	(0.029)	(0.032)	
1997	0.004	0.181	0.036	0.160
	(0.002)	(0.029)	(0.017)	
1998	0.003	0.164	0.084	0.109
	(0.002)	(0.020)	(0.017)	

the individual segments must still rely mostly on their own resources to fund their projects.[5]

IV. Measurement-error Consistent Estimation

A. Estimators

Now I wish to see if the OLS results stand up when one corrects for measurement error. Two types of measurement-error remedies have dominated the empirical investment literature: the use of lagged variables as instruments for q and the Griliches and Hausman (1986) remedy. However, both of these techniques are valid only if the measurement error in q is serially uncorrelated. In my opinion, however, the required lack of serial correlation

[5] I have also run the above regressions using both median q and fitted q. This exercise is motivated by the observation that if one of the proxies I use contains more information about marginal q than the others, I ought to see different regression results. However, both sets of regressions provide the same qualitative conclusions as those using adjusted q. I do, however, find one difference between the regressions using adjusted q and those using median q. The other-segment cash flow coefficients in the latter are about 50 percent higher, though the coefficients on q itself are only about 10 percent smaller. This result provides indirect suggestive evidence that my adjustment of median q reduces measurement error.

is implausible in the q context, even approximately. Several of the sources of measurement error can persist for longer than one year—the frequency of COMPUSTAT data. Examples of the sources of measurement error discussed above that tend to be persistent include market power, nonconstant returns to scale, deviations of the market value of debt from the book value, and, to a lesser extent, market inefficiencies.[6]

I use a different method for obtaining consistent estimates with an imperfect proxy. Following Erickson and Whited (2000a, 2000b), I employ estimators that use third- and higher-order moments of the joint distribution of the observed regression variables. Three assumptions are necessary to use these estimators. First, investment-q regressions must be characterized by the standard linear errors-in-variables model, which I write as follows. Let y_i be the rate of investment, and q_i be marginal q. Then the measurement error model I use can be written as

$$y_i = z_i \alpha + q_i \beta + u_i, \tag{1}$$

where z_i is a row vector of perfectly measured regressors. Setting $z_i = 1$ gives the basic q model; setting $z_i = (1, z_{i1})$, where z_{i1} is the ratio of cash flow to the capital stock, gives a model that also contains cash flow; and setting $z_i = (1, z_{i1}, z_{i2})$ gives the model that further contains other-segment cash flow. Let x_i denote a proxy for q_i, and write

$$x_i = \gamma + q_i + \varepsilon_i. \tag{2}$$

Note that the intercept in this equation allows for systematic bias in the measurement of marginal q that might, for example, arise because of the market-power-induced excess of average over marginal q described above.[7] Note also that this specification rests on the implicit assumption that all of the sources of measurement error discussed in Section I can be captured by a single additive error.

The second assumption is that u_i and ε_i are *i.i.d.* and independent of each other and of (q_i, z_i). Before stating the third assumption, it is convenient to transform the regression by expressing y_i, x_i, and q_i in terms of residuals from population regressions of each of these variables on z_i. Define $\mu_y \equiv [E(z_i'z_i)]^{-1}E(z_i'y_i)$ and $\mu_x \equiv [E(z_i'z_i)]^{-1}E(z_i'x_i)$. Letting $\dot{y}_i \equiv y_i - \mu_y z_i$, $\dot{x}_i \equiv x_i - \mu_x z_i$, and $\dot{q}_i \equiv q_i - \mu_x z_i$, we can write equations (1)–(2) as

$$\dot{y}_i = \dot{q}_i \beta + u_i \tag{3}$$

$$\dot{x}_i = \dot{q}_i + \varepsilon_i. \tag{4}$$

[6] Note that the measurement error can be serially correlated even though, as explained above, true marginal q captures all determinants of investment, including those that are serially correlated. For example, if the proxy does not capture some of these serially correlated factors, then the difference between marginal q and its proxy (the measurement error) will be serially correlated.

[7] Erickson and Whited (2000a) show that biased measurement renders estimation of the intercept of (1) impossible. In this application, however, the intercept has little economic content.

This transformation simplifies computation substantially. Erickson and Whited (2000a) show that estimates of the jth element of α are obtained by substituting the GMM estimate of β and the jth elements of OLS estimates of μ_y and μ_x into

$$\alpha_j = \mu_{yj} - \mu_{xj}\beta \quad j \neq 0. \tag{5}$$

The EW estimators are based on equations expressing the observable moments of $\dot{y}_i$ and $\dot{x}_i$ as functions of β and the unobservable moments of u_i, ε_i, and $\dot{q}_i$. Assuming finite moments, the assumptions on equations (1)–(2) straightforwardly imply

$$E(\dot{y}_i^2) = \beta^2 E(\dot{q}_i^2) + E(u_i^2) \tag{6}$$

$$E(\dot{y}_i \dot{x}_i) = \beta E(\dot{q}_i^2) \tag{7}$$

$$E(\dot{x}_i^2) = E(\dot{q}_i^2) + E(\varepsilon_i^2) \tag{8}$$

$$E(\dot{y}_i^2 \dot{x}_i) = \beta^2 E(\dot{q}_i^3) \tag{9}$$

$$E(\dot{y}_i \dot{x}_i^2) = \beta E(\dot{q}_i^3) \tag{10}$$

$$E(\dot{y}_i^3 \dot{x}_i) = \beta^3 E(\dot{q}_i^4) + 3\beta E(\dot{q}_i^2) E(u_i^2) \tag{11}$$

$$E(\dot{y}_i^2 \dot{x}_i^2) = \beta^2 (\dot{q}_i^4) + \beta^2 E(\dot{q}_i^2) E(\varepsilon_i^2) + E(u_i^2) E(\dot{q}_i^2) + E(u_i^2) E(\varepsilon_i^2) \tag{12}$$

$$E(\dot{y}_i \dot{x}_i^3) = \beta E(\dot{q}_i^4) + 3\beta E(\dot{q}_i^2) E(\varepsilon_i^2). \tag{13}$$

I replace the eight left-hand side moments with their sample counterparts and then use GMM to find the vector of six right-hand side quantities $(\beta, E(\dot{q}_i^2), E(u_i^2), E(\varepsilon_i^2), E(\dot{q}_i^3), E(\dot{q}_i^4))$ that come as close as possible, according to the minimum-variance GMM weighting matrix, to achieving the equalities of equations (6)–(13).[8]

The third assumption, an identifying assumption that ensures this estimator is consistent, is that both β and $E(\dot{q}_i^3)$ are nonzero. If this holds, then equations (9)–(10) imply $\beta = E(\dot{y}_i^2 \dot{x}_i)/E(\dot{y}_i \dot{x}_i^2)$. Given β, equations (6)–(8) and (10)–(11) can be solved for the remaining unknowns. Equations (12)–(13) provide overidentifying restrictions that increase efficiency. Using fifth- and sixth-order moment equations analogous to equations (6)–(13) provides further overidentifying restrictions, as the number of equations grows faster than the number of unknowns.

[8] The optimal weighting matrix would be the covariance matrix of the left-hand sides of (6)–(13), if (μ_x, μ_y) were known. However, because OLS estimates of (μ_x, μ_y) must be substituted for their population values, the optimal matrix differs. Instead, it is the covariance matrix of the sum of two vectors: (i) left-hand sides of (6)–(13) and (ii) a vector of adjustment terms that accounts for the extra variation induced by this "plug-in" procedure. See Erickson and Whited (2000a) for details.

The identifying assumption is testable. The hypothesis that this assumption is false can be rejected if the sample counterparts to the left-hand sides of equations (9)–(10) are significantly different from zero; see Erickson and Whited (2000a). I reject this null for all of the specifications that I run. This result makes sense in the context of an investment-q regression. Since marginal q is by definition a shadow value, it cannot fall below zero and therefore must have a distribution with a nonzero third moment; that is, a skewed distribution.

Clearly, as is the case with any econometric model, the remaining assumptions will not hold exactly. For example, the relationship between investment and q may be nonlinear, the omission of fixed effects may induce a correlated error and regressor, or the measurement or regression errors might be heteroskedastic. Regression error heteroskedasticity is of particular concern if there is reason to believe that β varies over firms (see Greene (1997, p. 669)). Finally, the measurement error equation (2) may not represent the true discrepancy between marginal q and its proxy. The most important problem with regard to the last point is that potential mismeasurement of the scaling variable violates my measurement-error assumptions. If the scaling variable is mismeasured, then, since it is the divisor in all of the regression variables, these ratios are also mismeasured, with conditionally heteroskedastic and mutually correlated measurement errors.

The interesting question, however, is not whether the assumptions are exactly true, but whether their violation qualitatively distorts my inferences. This observation underlies the importance of the GMM J-test of overidentifying restrictions, since these restrictions will be disrupted by any of the above assumption violations. Further as demonstrated in Erickson and Whited (2000a), the J-test can have useful power to detect these misspecifications.

The EW estimators not only generate measurement-error-consistent parameter estimates and a specification test, but also produce estimates of two interesting quantities. The first is the R^2 of equation (1); that is, the R^2 of the regression of investment on unobservable marginal q. I will denote this quantity as ρ^2. The second is the R^2 of equation (2), which can be thought of as the percentage of the variation in the market-to-book ratio that is due to true marginal q. I will denote this quantity as τ^2. Estimating these quantities is possible because the EW technique provides estimates of the variances of the unobserved regressor, the equation error, and the measurement error. The Appendix contains a Monte Carlo experiment demonstrating the efficacy of these estimators. The Monte Carlo indicates that these estimators perform well on simulated data that closely resembles my actual data. I also use the Monte Carlo to choose the highest moment order that I employ below. Because I find that the best finite-sample performance is obtained by using second through fifth moments, the results that follow are from an estimator based on these moments.[9]

[9] Cragg (1997) gives an estimator of β that is the same as the fourth moment EW estimator, except for his omission of the required adjustment to the GMM weighting matrix. He does not provide estimators for τ^2 and ρ^2.

Table IV

GMM Estimates of the Investment-*Q*-Cash-Flow Model

Calculations are based on a sample of single segment nonfinancial firms from the combined annual and full coverage 1999 Standard and Poor's COMPUSTAT industrial files and on a sample of nonfinancial segments from COMPUSTAT's Business Information File. The sample period is 1993 through 1998. Investment and cash flow are scaled by total firm assets. Firm q is calculated at the ratio of the market value of assets to the book value of assets. Segment q is calculated as the median of the qs of the firms in a segment's three-digit industry, scaled by the firm's diversification discount. Regressions are run using the fifth moment estimator in Erickson and Whited (2000a). ρ^2 is the R^2 of the regression containing true unobservable marginal q, and τ^2 is the percentage of the variation in my proxy for q that is due to true unobservable marginal q. Asymptotic standard errors are in parentheses under the parameter estimates. P-values are in parentheses under the J-statistics.

	q	Cash Flow	ρ^2	τ^2	J-Statistic
Firms					
1993	0.087	0.027	0.245	0.227	7.378
	(0.021)	(0.036)	(0.037)	(0.040)	(0.194)
1994	0.063	0.054	0.215	0.260	8.757
	(0.019)	(0.036)	(0.029)	(0.041)	(0.119)
1995	0.123	−0.091	0.218	0.292	9.297
	(0.046)	(0.091)	(0.039)	(0.042)	(0.098)
1996	0.052	0.022	0.185	0.328	6.097
	(0.016)	(0.039)	(0.025)	(0.044)	(0.297)
1997	0.053	0.051	0.185	0.233	9.643
	(0.018)	(0.033)	(0.021)	(0.038)	(0.086)
1998	0.041	0.078	0.110	0.173	13.778
	(0.024)	(0.022)	(0.025)	(0.081)	(0.017)
Segments					
1993	0.061	0.029	0.173	0.117	9.596
	(0.012)	(0.047)	(0.035)	(0.035)	(0.088)
1994	0.069	0.031	0.176	0.108	3.892
	(0.010)	(0.044)	(0.034)	(0.026)	(0.565)
1995	0.077	0.078	0.234	0.156	4.220
	(0.036)	(0.104)	(0.040)	(0.037)	(0.518)
1996	0.080	−0.019	0.186	0.107	5.181
	(0.024)	(0.078)	(0.041)	(0.027)	(0.394)
1997	0.087	0.008	0.320	0.090	5.938
	(0.022)	(0.078)	(0.061)	(0.032)	(0.312)
1998	0.030	0.082	0.120	0.213	3.390
	(0.012)	(0.045)	(0.024)	(0.053)	(0.640)

B. Results

The top panel of Table IV presents the results from using the GMM estimator on the investment-q-cash-flow model for the single segment firms. The results here correspond to those in Erickson and Whited (2000b), except that these results are generated using the market-to-book ratio as proxy for marginal q, instead of a proxy for q used in the macroeconomics literature. Note that the GMM estimates for each year are on average almost 10 times larger than the least squares estimates given in Table II, which is what one would expect from the attenuation bias generated by the classical errors-in-variables model. More importantly, all but one of the coefficients on cash flow are insignificantly different from zero. The fourth column of the table gives the estimates of ρ^2, the "true" R^2 of the regression. These figures are on average 1.6 times as large as the OLS R^2s given in Table III. The next column gives the estimates of τ^2. All of these estimates are significantly different from zero, their average falling at 0.252. In other words, about 25 percent of the variation in the market-to-book ratio is due to true marginal q. It is interesting to contrast this result with the finding in Erickson and Whited (2000b) that the τ^2 for the proxy used by macroeconomists is about 40 percent. The simple market-to-book ratio appears to be a slightly poorer proxy.

I now investigate whether using this technique removes the link between investment and cash flow at the segment level, as it does at the firm level. This issue is interesting because the insignificance of cash flow at the firm level is based on the intuition in Lucas and Prescott (1971) that marginal q should be the sole explanatory variable for firm investment. Also, Gomes (2001) shows that the effects of financial constraints should be reflected in marginal q. However, at the segment level, no such theoretical result exists, primarily because central headquarters has input into the investment decisions of its segments. Under these conditions, the productivity of the firm as a whole may affect a single segment's investment. It is, therefore, an open empirical question whether a segment's q will capture this effect. Only if it does will segment q be the only explanatory variable. Also, because of this fundamental difference between segments and firms, it remains an open question whether the investment-q sensitivity should be the same for segments as it is for single-segment firms.

The bottom panel of Table IV presents the GMM estimates of the same regression using the segment data. The results here are even more striking. The coefficients on q are now much larger than their OLS counterparts in Table III. This dramatic change in the coefficients on q renders the coefficients for the single-segment firms and for the segments insignificantly different from one another in every year. Correcting for measurement error leads to the inference that segments cannot be over- or underinvesting relative to stand-alone firms, since over- or underinvestment is measured *relative* to investment opportunities, and since both segments and firms pay equal attention to investment opportunities. In contrast to the conclusions reached by Berger and Ofek (1995) and Scharfstein (1998), these results indicate that inefficient levels of investment are not a source of the diversification discount. Next note that none of the cash-flow coefficients are significant and that the overidentifying restrictions never produce a rejection. The fourth column of the table indicates that the proxy for q used in these regressions is of quite poor quality: at a level of 0.132, the average value for τ^2 is just over half of what it is for the firms.

The difference in measurement quality between firm q and segment q has an interesting economic interpretation. If all of the segments of a conglomerate suffer from the diversification discount equally, then my measure of segment q—the industry-median q scaled by the diversification discount—should have the same measurement quality as firm q. On the other hand, the model in Maksimovic and Phillips (2000) suggests that there should be intrafirm differences in the adjustments to industry q, due to differences in segment productivity. The result that my measure of segment q has poorer measurement quality therefore provides indirect evidence that the productivity differences across segments are important.

Finally, in both panels of Table IV I report Hansen's (1982) J-test of the overidentifying restrictions generated by the measurement-error model I use. Out of the 12 regressions, only one of the tests produces a rejection at the five percent level. This result is even more striking given the Monte Carlo evidence in the Appendix of the tendency of the test to overreject in finite samples. The above discussion of sources of misspecification makes it clear that an investment-q-cash-flow regression need not fit perfectly into the mold of the classical errors-in-variables model. However, the lack of rejections indicates that the fit is good. It is important to note that the lack of rejections does not imply that the null hypothesis of no misspecification is true, but that sources of potential misspecification such as nonlinearity, simultaneity, a mismeasured scaling variable, and fixed effects are not important enough to disrupt the EW estimators.[10]

Table V presents results from using the EW estimator on regressions containing both cash flow and other-segment cash flow. Once again, I observe higher coefficients on q, low values of τ^2, insignificant coefficients on own cash flow, and few rejections of the overidentifying restrictions. I also observe insignificant coefficients on other-segment cash flow. Internal capital markets may very well exist; however, the evidence here suggests that the significance of other-segment cash flow in Table III is an artifact of measurement error and of the relatedness of segments. To see this last point, it is helpful to refer back to equation (5). If the segments of a conglomerate were unrelated, then the investment opportunities of one would be unrelated to the cash flow of another, and μ_{x2} would be zero. In this case, the rise in β from using the EW estimators would have no effect on the coefficient on other-segment cash flow. However, OLS estimates of μ_{x2} are all positive and significant, suggesting that segments are, in fact, related.[11] Therefore, correcting for measurement error does affect the coefficient on other-segment cash flow.

[10] As an informal test of whether the choice of scaling variable matters, I also ran the segment regressions using segment assets as a scaling variable. The coefficient estimates were qualitatively the same, though I found two more rejections of the overidentifying restrictions.

[11] Measurement error in q does not bias these estimates, since q is the dependent variable.

Table V

GMM Estimates of the Regressions of Investment on *Q*, Cash Flow, and Other-Segment Cash Flow

Calculations are based on a sample of segments of manufacturing firms from COMPUSTAT's Business Information File. The sample period is 1993 through 1998. Industry q is defined as the median value of the Tobin's qs of single-segment firms belonging to a particular segment's three-digit industry. Adjusted q is calculated by scaling industry q up or down depending on whether the firm to which the segment belongs trades at a premium or a discount. Investment, own-segment cash flow and other-segment cash flow are scaled by total firm assets. Regressions are run using the fifth moment measurement-error consistent estimator in Erickson and Whited (2000a). ρ^2 is the R^2 of the regression containing true unobservable marginal q, and τ^2 is the percentage of the variation in Tobin's q that is due to true unobservable marginal q. Asymptotic standard errors are in parentheses under the parameter estimates. P-values are in parentheses under the J-statistics.

	q	Cash Flow	Other Cash Flow	ρ^2	τ^2	J-Statistic
1993	0.055	0.040	−0.049	0.181	0.169	13.345
	(0.008)	(0.041)	(0.038)	(0.043)	(0.042)	(0.020)
1994	0.058	0.060	−0.001	0.193	0.138	4.849
	(0.007)	(0.038)	(0.025)	(0.036)	(0.029)	(0.435)
1995	0.071	0.098	0.022	0.280	0.188	8.402
	(0.030)	(0.091)	(0.046)	(0.046)	(0.038)	(0.135)
1996	0.066	0.024	−0.002	0.193	0.124	6.424
	(0.016)	(0.060)	(0.021)	(0.043)	(0.028)	(0.267)
1997	0.051	0.085	−0.012	0.247	0.123	4.826
	(0.011)	(0.043)	(0.024)	(0.048)	(0.038)	(0.437)
1998	0.049	0.029	−0.007	0.179	0.221	7.220
	(0.012)	(0.045)	(0.033)	(0.028)	(0.034)	(0.205)

I cannot address two related questions with the EW estimators. First, my measure of cash flow (operating income plus depreciation) may not represent the total amount of liquidity available for capital expenditures, since a firm with accidentally low cash flow but high liquid assets would be able to invest without tapping external capital markets. Second, a different source of measurement error in my regressions can arise from the latitude with which firms allocate earnings to their segments. Because capital expenditures are a left-hand-side variable, any measurement error it contains will not induce any bias; however, mismeasured cash flow can induce bias.

One possible remedy for these two problems is to assume that cash flow is mismeasured and then to use an EW estimator that contains multiple mismeasured regressors. However, this model fails the test for identification in all years. Recall that identification relies on the nonnormality of the *mis*-measured regressors. I therefore conjecture that this lack of identification arises because cash flow is now being treated as mismeasured and because it has low skewness and kurtosis. I can, nonetheless, address the question of the significance of cash flow indirectly using the results in Klepper and Leamer (1984), who treat the case of two mismeasured regressors. Because mismeasurement of cash flow implies that the regressions in Table V contain three mismeasured regressors, I can only comment on the robustness of the results in Table IV but not on those in Table V.

Klepper and Leamer (1984) require the researcher to input prior information. They provide a method for computing a threshold level of τ^2 above which the true values of the coefficients on cash flow and q will have the same sign as their OLS estimates. In other words, if the researcher believes that the measurement quality parameters for both q and cash flow are above this threshold, then he should also believe that the coefficients on q and cash flow are positive. For the firm-level data these thresholds lie between 0.504 and 0.733, and for the segment level data these thresholds lie between 0.544 and 0.637. Given the low estimates for τ^2 in Tables IV and V, and given my prior belief that the coefficient on q is positive, I believe it is unlikely that the cash flow coefficient is positive.

V. Conclusion

Diversified conglomerates are well documented to be valued less than matched portfolios of pure-play firms. This paper addresses the question of whether inefficient investment causes the diversification discount. The evidence suggests that the answer is no. In reaching this conclusion, I have first replicated many of the results in the literature in order to demonstrate that my data set and econometric methods are comparable to those used by other researchers. First, I find lower coefficients on Tobin's q for segments of multisegment firms, which, as some authors argue, may point to less of a response of investment to profitable investment opportunities in diversified conglomerates. Second, I find that a segment's investment is sensitive to the cash flow of other divisions—a result that many suggest provides evidence that corporate headquarters shifts funds across divisions. However, I reemphasize that these two results are predicated on the assumption that the market-to-book ratio is a good proxy for marginal q. This paper has made an attempt to solve the measurement error problem. In particular, I use measurement-error consistent estimators and find that these two results disappear. Investment does not respond to any measure of cash flow for segments or firms, and the response of investment to q is the same for segments and firms.

What conclusions should one draw from this dramatic turnaround in the results? At the very least, it tells us that using questionable proxies can seriously bias inference. We have also learned that investment-q-cash-flow regressions can tell us little about divisional investment policies. More importantly, the results indicate that in comparison to single-segment firms, segments of conglomerates do not over- or underinvest relative to their investment opportunities. I have also provided indirect evidence that conglomerate segments are related and that there are differences in productivity across segments of the same firm. Finally, this paper reconciles the evidence of internal capital market efficiency in Maksimovic and Phillips (2000) and the evidence of inefficiency in the rest of the literature, by showing that much of the existing evidence of inefficiency is likely to be an artifact of measurement error.

A potential interesting topic for further research on internal capital markets is whether the segments of diversified firms are satisfying a set of first-order conditions. Unlike investment-q-cash-flow regressions, this method would provide a direct causal link between the investment policy of a conglomerate and its value, since examining first-order conditions tells us whether a firm is optimizing or not. Another topic might deal with the idea that conglomerates could easily be misallocating resources other than physical capital across divisions. Attempts to find and isolate misallocation of R&D expenditures, human capital, and other factors of production could provide interesting insights into the functioning of internal capital markets.

Appendix

To allay skepticism of empirical results that have been produced by unusual estimators, I report a Monte Carlo simulation using artificial data very similar to my real data, both in terms of sample size and observable moments. Although I have two data sets, for brevity I report only one combined Monte Carlo experiment. I take this approach because the observable first seven moments of the variables in my firm and segment data are insignificantly different from one another, and because the results from running two separate Monte Carlos were almost identical. I generate 10,000 simulated data sets similar to my actual data. I set the parameters β, ρ^2 and τ^2 approximately equal to the averages of the corresponding GMM estimates from Table III, and I set α_1 and α_2, the coefficients on my "cash flow" and "other cash flow" variables, z_{i1} and z_{i2}, equal to zero, corresponding to the null hypothesis that marginal q should be the sole regressor. Table AI reports the mean value of an estimator, its mean absolute deviation (MAD) and, except for α_1 and α_2, the probability an estimate is within 20 percent of the true value. Because the simulation true values of α_1 and α_2 are zero, I report the probability that these estimates are in the interval $(-.02, .02)$. Table AI shows that every GMM estimator is clearly superior to OLS, by the probability concentration, MAD, and bias criteria. The GMM5 estimator gives the best estimates of all parameters. Finally, Table AI reports the size of the GMM J-test of overidentifying restrictions as well as the size of the one-sided GMM test of the null hypotheses $\alpha_i = 0$, $i = 1, 2$, against the alternatives $\alpha_i > 0$. The slight tendency to overreject in both cases only strengthens the inferences I made above.

Table AI

Monte Carlo Performance of GMM and OLS Estimators

Indicated expectations and probabilities are estimates based on 10,000 Monte Carlo samples of size 2,000. The samples were generated by

$$y_i = z_i \alpha + \chi_i \beta + u_i$$

$$x_i = \gamma + \chi_i + \varepsilon_i,$$

where χ_i is lognormally distributed and ε_i and u_i are chi-squared variables with one degree of freedom. The vector z_i contains an intercept and two variables, each of which is a linear combination of χ_i and two standard normal variables. GMMn denotes the GMM estimator based on moments up to order $M = n$. OLS denotes estimates obtained by regressing y_i on (x_i, z_i).

True values: $\beta = 0.05$, $\alpha_1 = \alpha_2 = 0$, $\rho^2 = 0.25$, $\tau^2 = 0.20$.

	OLS	GMM3	GMM4	GMM5	GMM6
$E(\hat{\beta})$	0.008	0.053	0.052	0.050	0.054
$\text{MAD}(\hat{\beta})$	0.042	0.014	0.012	0.011	0.015
$\text{P}(\lvert\hat{\beta} - \beta\rvert \leq 0.2\beta)$	0.000	0.644	0.561	0.640	0.603
$E(\hat{\alpha}_1)$	0.201	−0.014	−0.010	0.000	−0.019
$\text{MAD}(\hat{\alpha}_1)$	0.201	0.078	0.069	0.065	0.082
$\text{P}(\lvert\hat{\alpha}_1 - \alpha_1\rvert \leq 0.02)$	0.000	0.451	0.409	0.444	0.415
$E(\hat{\alpha}_2)$	0.072	−0.007	−0.003	0.000	−0.007
$\text{MAD}(\hat{\alpha}_2)$	0.072	0.044	0.039	0.038	0.044
$\text{P}(\lvert\hat{\alpha}_2 - \alpha_2\rvert \leq 0.02)$	0.014	0.266	0.264	0.269	0.256
$E(\hat{\rho}^2)$	0.066	0.201	0.214	0.224	0.239
$\text{MAD}(\hat{\rho}^2)$	0.184	0.061	0.054	0.052	0.060
$\text{P}(\lvert\hat{\rho}^2 - \rho^2\rvert \leq 0.2\rho^2)$	0.001	0.468	0.538	0.570	0.562
$E(\hat{\tau}^2)$	—	0.160	0.162	0.181	0.184
$\text{MAD}(\hat{\tau}^2)$	—	0.048	0.051	0.044	0.047
$\text{P}(\lvert\hat{\tau}^2 - \tau^2\rvert \leq 0.2\tau^2)$	—	0.494	0.489	0.604	0.537
Rejection rates of asymptotic nominal five percent tests					
J-test	—	—	0.049	0.071	0.109
t-test of H_0: $\alpha_1 = 0$	—	0.077	0.097	0.099	0.111
t-test of H_0: $\alpha_2 = 0$	—	0.065	0.073	0.070	0.072

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