In future work, adding more structure to the alternative model of financial-market imperfections should yield differential predictions to help test the validity of financial explanations for investment. In my past work, for example, I showed (Gross, 1997) that imposing liquidity constraints leads to strong nonlinearities in the predicted effects of financial variables, which are confirmed in the data. It would be even more useful to explicitly model the information or agency problem which generated the financial-market imperfection in the first place. It may be too much to ask for an empirically tractable, theoretically justified model of financial-market imperfections which allows for nonconvex adjustment costs and can deal with the heterogeneity present in real-world data. However, it will be difficult to fully understand the microeconomics of investment without such a model.

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Comment

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1. *Introduction*

In this stimulating paper, Gilchrist and Himmelberg (hereafter, GH) use Compustat data to test and quantify the effect of financial factors on investment. I like the paper's innovative willingness to estimate and test a structural model with financial frictions. In my view, this is a welcome advance on a common practice of estimating a model without such frictions and testing whether the model fails in ways consistent with financial frictions. By specifying precisely and completely how financial frictions affect investment, GH can quantify the effects of those frictions,

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and get a sense for whether still richer models are required to adequately explain investment.

I divide my comments into two parts. First is a summary of the literature on borrowing constraints and investment. Then follows a discussion of the structural model (Sections 3–5 in GH). I conclude that the present paper leaves some important questions unanswered. While I am receptive to GH's interpretation, in the end I do not find it compelling.

2. *Borrowing Constraints and Investment*

In typical parametrizations of the models of Hall and Jorgenson (1967) and Tobin (1969), investment is determined by the ratio of output to the cost of capital or by stock-market-based measures of Tobin's Q, perhaps along with lags of these variables or of investment itself. For want of a better term, I will refer to such variables as the *traditional* determinants of investment. At least since Meyer and Kuh (1957), however, it has been recognized that cash flow is a good predictor of aggregate investment. Clark (1979) concluded that cash flow and output together predict better than do the neoclassical determinants, a finding broadly consistent with the somewhat weaker role for cash flow found in the more recent study by Kopcke (1993).

In a pioneering paper, Fazzari, Hubbard, and Petersen (1988) established a complementary set of results with individual firm data. They ran investment regressions in which right-hand-side variables included some traditional determinants of investment as well as cash flow. They found that cash flow had a statistically significant effect on investment for firms that one might expect to be financially constrained, namely, ones with low dividend payouts. As documented in Hubbard's (1998) survey, many subsequent studies have found that measures of liquidity, such as cash flow or debt, are statistically significant predictors of investment for firms displaying low dividends, no bond rating, or some other presumed signals of financial illiquidity. While a literature has risen in reaction, arguing that in one or another dataset the empirical evidence in fact does not suggest that financial factors are important (e.g., Hayashi, 1997; Kaplan and Zingales, 1997; Cummins, Hassett, and Oliner, 1998), it is my judgment that there now exists a strong case that financial variables have information about investment not contained in the traditional measures.

The predictive power of cash flow suggests a difference between internal and external costs of funds. Indeed, it has been shown formally that

in the presence of informational frictions, such as imperfect or costly monitoring of a firm's behavior by a lender (Townsend, 1979; Myers and Majluf, 1984), the shadow value of internal funds will be larger than that of external funds, at least some of the time. Cash flow will therefore likely affect investment, even after controlling for traditional determinants.

In my opinion, however, the empirical literature has not made a good case that informational frictions in fact rationalize the empirical importance of financial variables.[1] It is well recognized that misspecification of the parametric form of the traditional model, or inappropriate accounting for unobservable disturbances or for the informational role of financial variables, could cause spurious significance of financial variables, significance that would disappear if the frictionless model were appropriately specified.

Support for the view that financial frictions rather than misspecification are key could come from establishing that the estimates on financial factors fall in a tight range predicted by an underlying theoretical model, that a model with financial frictions does not reject tests of overidentifying restrictions, that no variables beyond those posited by the model have substantial explanatory power for investment, and so on. To my knowledge, little research in this literature attempts to do so. Instead, the bulk of the literature that I am familiar with evaluates (and usually rejects) a null neoclassical model. No doubt this is in large part because quantitatively tractable models of financial frictions are difficult to come by. Nonetheless, this approach yields diminishing returns for people who, like me, are already persuaded that cash flow and other financial variables help predict the investment of firms that display signs of illiquidity.

I therefore am very receptive to Gilchrist and Himmelberg's effort to formulate and evaluate a model with financial frictions.

3. *Gilchrist and Himmelberg's Model*

Sections 3–5 of GH specify, estimate, and test a model of investment in the presence of financial frictions—that is, the null model is one with financial frictions (though a frictionless model is nested within the null model).

Specifically, this part of GH:

1. Nor, even assuming that these frictions in fact do account for the econometric results, does this or related literature establish that the frictions have nontrivial effects on aggregate output, although it has been shown that such frictions potentially have aggregate effects (Carlstrom and Fuerst, 1997; Kiyotaki and Moore, 1997; Jones, 1998). But further discussion of that point will take us pretty far from the GH paper.

1. Constructs a present value called *fundamental Q*—a specific example of what I called a "traditional" measure above—by positing a parametric functional form, linearizing, and using data on output, capital, and other variables, as in Abel and Blanchard (1986). Fundamentals depend only on the marginal profit of capital (MPK) and, in contrast to Abel and Blanchard (1986) and many other papers, not on discount factors (i.e., not on user costs of capital), which are assumed not to vary.
2. Approximates the effects of financial frictions by (i) showing that these will manifest themselves in time-varying discount factors; (ii) linearizing to separate the discount factor from the frictionless fundamentals; (iii) assuming the linearized term is also linear in observable financial data, namely, cash and equivalents, or working capital less long-term debt.
3. Regresses investment on lagged investment, fundamental *Q*, and the present value of financial frictions (*financial Q*). The finding is that financial frictions are both statistically and economically important.

As a mechanical matter, the regressions are novel essentially in entering the financial variable in a constrained form, as a present value; the common procedure in this literature is simply to add a variable like cash and equivalents to whatever variables are included by virtue of the traditional model under examination. The important novelty is a willingness to maintain, at least tentatively, that the addition of the financial variable results in a complete model for investment. GH acknowledge that the model in fact does miss an important aspect of investment behavior, in that the data want lagged investment on the right-hand side (Section 5.3), and they make rationalizing this extra term a high priority for future research. But they also clearly feel that the present specification documents strong economic and statistical effects from financial frictions.

While I do find GH's results suggestive, I am not as convinced as are GH that they have documented strong effects. In explaining this viewpoint, I will not promote an alternative interpretation of their results, still less provide a different point estimate of the quantitative effects of financial frictions. Rather, I want to raise some questions, which, until answered, suggest caution in interpreting GH's findings. These questions include:

1. In their theoretical development, GH assume that in the absence of financial frictions the discount rate is constant across firms and time (Section 3.1); had they carried through the algebra with time-varying discount rates, there would have been an additional term, involving

the present value of future values of discount rates (see Abel and Blanchard, 1986). The presence of time dummies in the empirical work implicitly allows time variation in discount rates that is common to all firms; the presence of a fixed effect means one can interpret such variation as occurring around a firm-specific mean discount rate. (That is, these dummies allow for time variation in discount rates that is perfectly correlated across firms, with each firm possibly having a different mean discount rate.) But firm-specific variation (i.e., imperfect correlation across firms) is swept into the regression disturbance. And we have a raft of finance studies indicating that discount rates vary across firms. Such variation may affect the estimates on the coefficients on the two present values included by GH. If the variation is largely uncorrelated with the GH present values (perhaps this is the case for most firms presumed to be financially unconstrained in Tables 5 and 6), the GH interpretation is still legitimate. But if the variation is correlated with the included present values (perhaps firms that are financially constrained undertake riskier projects), omitted-variable bias will invalidate the GH procedure. So: how are the estimates and interpretation affected by cross-sectional variation in discount factors stemming from traditional forces (rather than from financial frictions)?

2. When lagged investment is added to the GH equation, the term proves significant, but, the authors tell us, the point estimates on the two present values change little. It is, however, premature to conclude, as the authors seem to, that once the model is expanded to formally allow for lagged investment, the statistical or economic importance of the two present values will change little. This is because in the expanded model the present values will be calculated in a different way, implying that the present model's regressions contain noisy measures of the relevant objects. If one rationalizes the lag with costs of adjustment or serially correlated shocks, the relevant regression will involve a lag of investment and two present values, and the two present values will be different from the ones presently included. (For example, with costs of adjustment, a certain quadratic will be factored, with forward solution of the unstable root to this quadratic affecting the present value calculation.) If, instead (or in addition), one rationalizes the lag with an information role for investment—that is, the firm forecasts the present values using data not available to the econometrician—the regression will still involve only two present values, but once again the two will be different from the ones presently included (see Kiyotaki and West, 1996). So: what happens when the model is extended to rationalize the predictive power of lagged investment?
3. GH state that their linearization implies that the external finance premium is proportional to their measure of financial liquidity (either cash and equivalents, or working capital less long-term debt), with a factor of proportionality ϕ. They also conclude that the regression estimate of ϕ is pretty much consistent with a back-of-the-envelope calculation. I could not quite follow the details of this calibration, so I will not comment except to state such a check on the reasonableness of the estimate is an important one. An additional check is suggested by the model's implication that one can read interest-rate spreads off the measure of financial liquidity. If one backs out a series of spreads (presumably identified only up to the addition of a constant), what does the series look like, and how does it compare to observed data on lending rates?

I look forward to the answers to questions such as these in the next paper in the Gilchrist Himmelberg's research program.

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Discussion

Much of the discussion focused on the adequacy of the baseline model, particularly the use of the sales-to-capital ratio as a measure of the marginal profitability of fixed capital. Janice Eberly pointed out that the proportionality of profitability and sales requires that variable factors, such as labor, be adjustable instantaneously. If there are costs of adjusting labor, then shocks to wages (for example) will affect profitability and cash flow without having a short-run effect on output. On a related issue, Fabio Schiantarelli noted that, if there are costs of adjusting the capital stock, the net marginal profitability of capital depends on the investment-to-capital ratio as well as sales. Jason Cummins asked about the omission of inventories from the analysis. Charles Himmelberg replied to Cummins that, possibly because of the use of annual data, it makes little difference empirically whether inventories are included, that is, either sales or production can be used as the measure of profitability. More generally, Himmelberg defended the baseline model by pointing out that it seems to work well empirically for bond-rated firms, which are less likely to be financially constrained. Deviations from the baseline model are thus reasonably interpreted as arising from financial factors.

A second issue was the possible effects of unmodeled heterogeneity among firms. Cummins suggested that the restriction that the data-generating process for shocks is identical across firms is unrealistic. He noted that financial analysts produce earnings forecasts using firm-specific models. He reported that, in his own work with coauthors, the use of firm-specific earnings expectations reduced the measured effect of cash-flow variables significantly. Himmelberg disputed the result that

earnings expectations measures "drive out" cash-flow variables from investment equations, indicating that this was not the case for the specifications used in the present paper. He also argued that earnings expectations might be interpreted as projections of whether the firm can repay its debt, that is, as an indicator of financial condition, rather than as forecasts of long-term profitability. Also on the theme of heterogeneity, Benjamin Friedman proposed that investment–cash-flow relationships might be quite different at the firm level than at the industry level. For example, a firm might be impelled to invest by an increase in competitive pressure, resulting in a negative relationship of current cash flow and future investment for the firm, even though that relationship might well be positive for the industry as a whole.

Econometric issues also received some discussion. Stephen Oliner proposed the use of a larger set of instruments to reduce problems of endogeneity.