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How responsive is business capital formation to its user cost? An exploration with micro data

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Abstract

The response of business capital formation to its user cost is critical to evaluating tax reform, deficit reduction, and monetary policy. Evidence for a substantial user cost elasticity, however, is sparse. Most evidence has been based on aggregate data, although several recent studies with firm-level data report substantial effects. With a particularly rich micro dataset containing over 26,000 observations, this paper explores what can be learned about the user cost elasticity. While the results depend to some extent on the specification and econometric technique, various diagnostics lead us to prefer a precisely estimated but small elasticity of approximately -0.25.

This paper is dedicated to the memory of Robert Eisner, who labored early and ably on uncovering the determinants of investment spending and highlighting the importance of the question found in the title of this paper.

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1. Introduction

The price sensitivity of business investment spending is a central element in economic analysis. A substantial response of capital spending to its user cost, which combines interest, tax, and depreciation rates with relative prices, is critical in controversies about the impact of fiscal policy, the transmission of monetary policy, and the performance of business-cycle models. This paper takes a fresh look at the user cost elasticity, exploring what can be learned about this key parameter from a particularly rich micro dataset containing over 26,000 observations.

The user cost elasticity of the capital stock (UCE) is a key parameter in analyzing fiscal policies. The simulation models of Auerbach and Kotlikoff (1987), İmrohoroğlu et al. (1998), and Razin and Yuen (1996), for example, are based on a Cobb-Douglas technology. This technology and its implied UCE of unity play a large role in assessing the quantitative effects of policy changes. For example, shifting from an income to a consumption tax increases steady-state net output by 3.8, 6.8, or 9.5% depending on whether the UCE is 0.5, 1.0, or 1.5, respectively (Engen et al., 1997, Tables 2A and 5). Indeed, the UCE is important in estimating the effects of a wide variety of fiscal measures designed to spur capital formation, such as cuts in the capital gains tax rate, reinstatement of the investment tax credit, and the adoption of a "flat tax." We consider the implications of our results for the effectiveness of these policies in Section 6.

The price sensitivity of investment is also important in the long-standing controversy about how monetary policy impacts real variables. In the standard description of the "transmission mechanism," monetary policy affects real activity by altering bank reserves and changing short-term interest rates and, through the term structure, long-term interest rates. With a substantial UCE, monetary policy can have an important effect on business investment spending. The absence of a significant UCE casts doubt on the validity of this version of the monetary transmission mechanism.[1]

Implicit assumptions about the UCE also loom large in real business cycle models. For example, Christiano and Eichenbaum (1992, p. 433) and Farmer (1997, p. 578) use a Cobb-Douglas production function, and hence they maintain that the UCE is unity. Thus, the ability of these RBC models to reproduce certain features of macroeconomic data is based in part on capital formation (defined in terms of foregone consumption) being quite responsive to variations in interest rates. We also note that business cycle models typically employ the user cost elasticity of the capital stock, not the often-estimated responsiveness of investment to the level of interest rates or the cost of capital. For this reason, our study

[1] This empirical shortcoming has led some researchers to favor a "credit view" of the transmission mechanism, which holds that monetary policy remains effective even with a low UCE. See Bernanke and Gertler (1995) for further discussion.

provides evidence more directly relevant to the empirical assessment of modern business cycle models than much previous research.

Despite the key role played by the UCE across a wide spectrum of economic analyses, the supporting evidence for a substantial UCE is sparse. A recent survey found little compelling evidence that, as historically implemented, tax and interest rate policies effectively stimulate business fixed investment (Chirinko, 1993). Blanchard (1986, p. 153) writes ''[i]t is well known that to get the user cost to appear at all in the investment equation, one has to display more than the usual amount of econometric ingenuity.'' Bernanke and Gertler (1995, p. 27) add that ''empirical studies of supposedly 'interest-sensitive' components of aggregate spending have in fact had great difficulty in identifying a quantitatively important effect of the neoclassical cost-of-capital variable.'' What should one make of the apparent inconsistency between widely held beliefs about a large UCE and the paucity of empirical support for such beliefs? Is the true UCE much lower than most economists assume, perhaps due to limited substitution possibilities in production? Most empirical studies of the price sensitivity of investment have been based on aggregate data, and the resulting estimates may be biased due to problems of simultaneity, capital market frictions, or firm heterogeneity that may be better addressed with micro data. Indeed, several recent studies (discussed in Section 5) with firm-level data report substantial effects of user cost variables on investment spending.[2]

We explore these questions with an extensive panel of firm data, constructed from Compustat ''full coverage'' files, that contain 4,095 manufacturing and non-manufacturing firms. The data account for almost half of aggregate U.S. capital spending in 1987, the middle of the sample period. This extensive coverage increases confidence when extrapolating the empirical results to the economy at large. We also tap a new source to construct the user cost of capital and have merged user cost variables defined at the industry level with Compustat firm data. Thus, our user cost data vary in both time-series and cross-sectional dimensions, reducing concerns about measurement error bias and likely improving the precision of estimation.

The paper is organized as follows. The data set is central to this study, and it is described in Section 2. Substantial firm heterogeneity is documented. Section 3 derives the econometric equation and discusses the interpretation of the estimates. Section 4 presents our empirical results. We consider a variety of regression specifications, equation diagnostics, and estimation techniques, including the new ''orthogonal deviations'' estimator from Arellano and Bover (1995). After accounting for a number of biases, we obtain a range of estimates for the UCE from close to zero to roughly -0.50. Several diagnostic tests lead to our preferred

[2]See Cummins and Hassett (1992), Cummins et al. (1994, 1996), Cabellero et al. (1995), and surveys by Chirinko (1993) and Hassett and Hubbard (1997). Earlier studies that have used firm-level data include Eisner (1967, 1978) and Jorgenson and Siebert (1968).

instrumental variables estimates of the UCE, with a parsimonious specification, that fall in a narrow range around -0.25 with a standard error of 0.03 to 0.06. These estimates are statistically far from zero, but also far from unity and hence much different from values often assumed in policy analysis. We compare our approach and findings to other recent research based on micro data in Section 5. Section 6 presents some simple policy evaluations, and Section 7 concludes.

2. Data and firm-specific variation

To estimate the UCE, we link two data sources that each provide information particularly well-suited to our objectives. Investment, sales and cash flow data come from the extensive Compustat ‘‘full coverage’’ files. The user cost variable is constructed from industry-level information maintained by Data Resources, Inc. After deleting observations with missing data, trimming outliers, and computing the necessary lags, we have a sample of 4,095 firms from all sectors of the economy that provide 26,071 annual observations for the regressions from the period 1981 to 1991 (we estimate some regressions with fewer than 26,071 observations because differencing the data lowers the observation count).[3] In the middle of the sample (1987) our data account for 48% of aggregate U.S. non-residential fixed investment and 43% of sales of final and intermediate goods.

Compustat firm data provide us with substantial benefits *vis-à-vis* the aggregate time-series used in most empirical research on the UCE. One clear benefit arises from statistical efficiency. Obviously, we have a huge number of degrees of freedom. Even though many of the questions of interest deal with the effect of economy-wide changes (such as movements in tax or interest rates that affect all firms), micro data provide a large number of replicated ‘‘experiments’’ that greatly improve the precision of our results. Improved precision may be important for identifying the UCE, especially to the extent that aggregate results are imprecisely estimated and are therefore not able to reject the hypotheses of a UCE equal to zero or unity. Furthermore, with micro data a given parameter can be estimated over a relatively short time frame, thus lessening the role played by parameter instability across time.

The user cost data complement the extensive firm heterogeneity available from Compustat by providing additional micro-level variation. We obtained information on the user costs for 26 different capital assets (24 types of equipment and two

[3] Firm data often have large outliers, especially when regression variables are expressed in ratio form, as is common practice to avoid severe heteroscedasticity generated by firms of very different size. To protect against results driven by a small number of extreme observations, we exclude observations in the 1% upper and lower tails from the distribution for each independent variable in the regression. In the reported results, we did not eliminate outliers from the dependent variable to avoid a censored regression bias. The results did not change much, however, when the 1% tails were also deleted for the dependent variable.

types of structures). These underlying user costs, based on Hall and Jorgenson (1967) and modified by DRI, can be represented as:

$$U_{g,j,t} = [p^{I}_{j,t}/p^{Y}_{g,t}]\,[(1 - m_{j,t} - z_{j,t})/(1 - \tau_t)]\,[r_t + \delta_j] \qquad (1)$$

where $p^{I}_{j,t}$ is the asset-specific purchase price for asset j at time t, $p^{Y}_{g,t}$ is the industry g output price at time t, r_t is the financial cost of capital (the same for all industries and assets),[4] and δ_j is the asset-specific economic depreciation rate. The investment tax credit ($m_{j,t}$) and discounted value of tax depreciation allowances ($z_{j,t}$) also vary across assets. We created industry-specific user costs as a weighted average of the asset user costs. The weights are the proportion of capital accounted for by each asset for 26 different industries.[5] This industry information was then merged with the firm-level Compustat data using each firm's S.I.C. code.[6]

Table 1 provides summary statistics for the regression variables. The variable I_t/K_{t-1} is the investment-capital ratio (firm subscripts are suppressed for simplicity). Investment is Compustat's capital expenditure variable from firms' uses of funds statement. Capital is the estimated replacement value of plant and equipment as described in Appendix A. The $t-1$ subscript on the capital stock indicates that it is measured at the beginning of each accounting year. Output is measured by sales.[7] Nominal sales data are taken from the Compustat net sales figure and deflated by the industry-specific output price deflator used to define the user cost in Eq. (1) ($p^{Y}_{g,t}$). The growth rate of real sales is represented by $\Delta S_t/S_{t-1}$. Cash flow (CF_t), which is scaled by the beginning-of-period capital stock, is net after-tax income plus non-cash expenses (primarily depreciation). The $\Delta U_t/U_{t-1}$ variable is the percentage change in the user cost defined in Eq. (1). Further details about data definitions appear in Appendix A.

The Compustat variables in the first three rows of Table 1 have positively skewed distributions as one would expect in firm data. The gross investment-to-

[4] The financial cost of capital is a weighted average of the cost of equity (the dividend–price ratio for Standard and Poor's Composite Stock Price Index plus an expected long-run growth rate of 2.4%, with a weight of 0.67) and the cost of debt (average yield on new issues of high-grade corporate bonds adjusted to a AAA basis, with a weight of 0.33). The cost of debt is lowered by its tax deductibility and the expected inflation rate, defined as a weighted average of past GDP deflator growth rates.

[5] These weights are from the Bureau of Economic Analysis capital flow tables and reflect asset usage by establishment. The Compustat data reflect ownership by company. The combination of industry aggregate data for the user cost and firm data for investment and other items may induce measurement error because some firms operate in a variety of industries. To the extent that such measurement error is constant within firms, however, it will be captured in firm fixed effects.

[6] Because the DRI user cost data are quarterly, we average them to obtain an annual user cost. The averages are computed at the firm level to account for the fact that firms have different fiscal years. The user cost information is therefore tailored to each firm's specific accounting period, which introduces further cross-sectional heterogeneity in the data and a more accurate measure of the user cost.

[7] The primary variation in output is due to sales. Blinder and Maccini (1991, Table 3) report that the ratio of the variance of output to the variance of sales is 1.03.

Table 1
Summary statistics for micro data[a]

Variable	Mean	Median	Within-firm standard deviation	Firm-specific time variation
I_t/K_{t-1}	0.173	0.125	0.163	0.979
$\Delta S_t/S_{t-1}$	0.030	0.018	0.223	0.976
CF_t/K_{t-1}	0.226	0.185	0.272	0.987
$\Delta U_t/U_{t-1}$	−0.013	−0.023	0.071	0.674

[a] Panel data for Compustat firms from 1981 to 1991, as described in the text. I_t/K_{t-1} is the ratio of firm capital spending to the beginning of period capital stock, $\Delta S_t/S_{t-1}$ is firm sales growth, CF_t/K_{t-1} is the ratio of firm cash flow to the beginning-of-period capital stock and $\Delta U_t/U_{t-1}$ is the percentage change in the user cost of capital. The within-firm standard deviation is computed after subtracting firm-by-firm means of each variable from each observation. This statistic therefore measures variation in the time dimension of the panel only. The firm-specific time variation is one minus the R^2 statistic from a regression of each mean-differenced variable on a set of time dummies, as described in Eq. (2). The extent of firm-specific time variation is very robust if cross sectional firm variation is removed by first differences or orthogonal deviations (discussed in Section 4).

capital ratios (mean of 0.173 and median of 0.125) are consistent with moderate capital stock growth, assuming that depreciation rates are in the range of 10 to 12%. Mean real sales grew by 3.0% per year in our 1981–1991 sample, although median sales growth was more modest at 1.8%. The within-firm standard deviations reported in Table 1 show substantial variability of the firm data across time.[8] The within-firm standard deviations exceed the means for all three Compustat variables.

Of particular note, given the emphasis here on firm-level variation, is the information on the percentage of firm-specific time variation in the data. This percentage is 1 minus the R^2 from the regression:

$$(X_{i,t} - a_i) = b_t + e_{i,t} \tag{2}$$

where $(X_{i,t} - a_i)$ represents mean-differenced variables for firm i at time t, b_t is the coefficient on a time dummy that is one for period t and zero otherwise, and $e_{i,t}$ is an error term. Because the data are mean differenced, cross-section variation is eliminated. The statistic reported in Table 1, therefore, indicates the proportion of time variation in the data that cannot be explained by aggregate time effects, i.e., the variance of $e_{i,t}$ relative to the variance of $(X_{i,t} - a_i)$. If this statistic equals zero, firm-specific variation is completely absent. For the Compustat variables (I_t/K_{t-1}, $\Delta S_t/S_{t-1}$, and CF_t/K_{t-1}), over 97% of time-series variation is firm specific. This statistic is lower for the user cost because variation in the interest rate and the tax

[8]These standard deviations measure variability in the data across time, not across firms. To eliminate cross sectional variation, we subtract the firm-by-firm means from each variable prior to computing the standard deviation.

parameters is determined to a greater degree by aggregate factors. Nonetheless, over 67% of the firm-specific time-series variation in the composite user cost is not explained by aggregate time dummies, indicating that the data we construct from the DRI source also has substantial micro-level variation.[9]

3. Econometric investment equations: Specification issues

The primary choice of an econometric specification to estimate the UCE is between a structural model, with estimating equations derived explicitly from an optimization problem, or a distributed lag model that relies less on theory.[10] Distributed lag models (e.g., Hall and Jorgenson, 1967; Eisner and Nadiri, 1968) relate investment to current and lagged values of the user cost, sales, and other factors. These models usually perform well empirically. Furthermore, despite the availability of alternative specifications, distributed lag models continue to be the model of choice among forecasters (e.g., Prakken et al., 1991; and Sinai, 1992). The Lucas Critique, however, raises important questions about interpreting estimated coefficients from these models.

This concern has led to an alternative approach for specifying investment models that imposes more structure on the econometric model, such as Q and Euler equation models. Unfortunately, the resulting investment models do not usually perform well empirically (see Oliner et al., 1995). The applied econometrician must choose between distributed lag models that are empirically dependable but conceptually fragile and structural models that have a stronger theoretical foundation but an unsteady empirical superstructure. While the Lucas Critique offers a compelling theoretical case for structural models, its empirical relevance has been questioned.[11] (Section 6 presents a new test of the empirical importance of the Lucas Critique that exploits panel data.) Furthermore, distributed lag models provide a direct estimate of the user cost elasticity of primary concern to this study. Thus, we estimate a distributed lag model, though our policy assessments must be tempered by the above caveat.

Our model is based on a firm's demand for capital and, with the addition of

[9]We compute similar statistics for two other methods of eliminating cross-section variation: first differences and orthogonal deviations (discussed in Section 4). The proportion of firm-specific variation remaining in each variable after these transformations is very similar to the statistics reported in Table 1.

[10]See Chirinko (1993) for a more detailed survey of econometric investment models and empirical results and an extensive list of references to several of the issues discussed in this section.

[11]For example, the impact of the Lucas Critique [see Lucas (1976)] on investment models is examined by Chirinko (1988), who assumes that the volatile fiscal environment of the 1980s reflected unanticipated changes in the policy regime. Instability associated with the Lucas Critique is identified, but it is not quantitatively important. Using a much different framework, Taylor (1989) arrives at a similar conclusion.

dynamics, demand for investment. The demand for capital follows directly from the first-order conditions for profit-maximizing behavior with static expectations. Assuming that the production function has a constant elasticity of substitution (σ) between capital and variable inputs, we obtain the following well-known relation between the desired (or optimal) stock of capital (K_t^*), the level of sales (or output), and the user cost (or rental price) of capital (U_t),

$$K_t^* = \zeta S_t U_t^{-\sigma}, \tag{3}$$

where U_t is defined in Eq. (1) and ζ is the CES distribution parameter. The parameter σ is the UCE, the focus of our analysis.

Absent any dynamic considerations, the firm would achieve K_t^* instantaneously. Dynamics enter when translating the stock demand for capital to a flow demand for investment, which is divided between replacement and net components. Capital is assumed to depreciate geometrically at a constant mechanistic rate (δ); hence, replacement investment (I_t^r) is proportional to the beginning-of-period capital stock,

$$I_t^r/K_{t-1} = \delta. \tag{4}$$

Net investment (I_t^n) is the change in the capital stock between periods $t-1$ and t, and is scaled by the existing stock. This ratio (plus 1.0) equals K_t/K_{t-1}, and it adjusts according to the weighted geometric mean of relative changes in the desired capital stock,

$$\begin{aligned} I_t^n/K_{t-1} + 1.0 = K_t/K_{t-1} &= \prod_{h=0}^{H} [K_{t-h}^*/K_{t-h-1}^*]^{\mu_h} \\ &= \prod_{h=0}^{H} [\Delta K_{t-h}^*/K_{t-h-1}^* + 1.0]^{\mu_h} \end{aligned} \tag{5}$$

where the μs represent the delivery lag distribution extending for H+1 periods.[12] Taking logs of (5), using the approximation $\ln(1+x) \approx x$, differencing the logarithm of (3) and substituting it into (5) for ($\Delta K^*/K^*$), using (4) for replacement investment, and appending a stochastic error (ϵ_t), we obtain the distributed lag investment equation:

$$\begin{aligned} I_t/K_{t-1} &= I_t^r/K_{t-1} + I_t^n/K_{t-1} \\ &= \delta + \sigma \sum_{h=0}^{H} \mu_h(\Delta U_{t-h}/U_{t-h-1}) + \sum_{h=0}^{H} \mu_h(\Delta S_{t-h}/S_{t-h-1}) + \varepsilon_t \end{aligned} \tag{6}$$

We consider two important extensions of Eq. (6). First, it has been frequently

[12] The geometric adjustment process is employed in Eq. (5) because, with the pronounced trends in I and ΔS and large differences in firm sizes, it is preferable to specify the investment equation with all variables as ratios or rates.

argued that a measure of liquidity should enter the model to account for access to internal funds that affect the timing of investment. In this model, liquidity is measured as cash flow (CF_t) and, to avoid units problems, cash flow enters relative to the existing capital stock (see Fazzari et al., 1988b). The specification of this variable – CF_t/K_{t-1} – implies that the effects of liquidity on investment expenditures are short-run, perhaps distributed over several periods.[13]

Second, in the presence of non-static expectations and delivery lags, the terms in Eq. (3) would be distributed over current and future periods and interpreted as expected values. Approximating K_t^* linearly in logs, differencing with respect to time, and assuming that expectations of the resulting output and user cost terms are based on extrapolations of their past values, we obtain an investment equation with distributed lag coefficients that mix expectation and technology parameters. Because the number of lags used in the extrapolations need not be equal, the lengths of the sales and user cost lags may differ. In addition, the possibility that capital is ''putty-clay'' implies that output changes lead to a more rapid investment response than user cost changes (Eisner and Nadiri, 1968; Bischoff, 1971), and hence the coefficients on $\Delta U_{i,t-h}/U_{i,t-h-1}$ and $\Delta S_{i,t-h}/S_{i,t-h-1}$ may differ. An examination of alternative lag lengths indicates that annual lags of 0 to 6 for $\Delta U_{i,t}/U_{i,t-1}$ and lags of 0 to 4 for $\Delta S_{i,t}/S_{i,t-1}$ and $CF_{i,t}/K_{i,t-1}$ are appropriate initially with ordinary least squares (OLS) estimates. These considerations lead to the following specification that includes an ''i'' subscript to denote firm-specific variables and coefficients:

$$I_{i,t}/K_{i,t-1} = \delta_i + \sum_{h=0}^{6} \alpha_h(\Delta U_{i,t-h}/U_{i,t-h-1}) + \sum_{h=0}^{4} \beta_h(\Delta S_{i,t-h}/S_{i,t-h-1}) + \sum_{h=0}^{4} \gamma_h(CF_{i,t-h}/K_{i,t-h-1}) + \epsilon_{i,t} \tag{7}$$

The coefficients are assumed to be the same across firms except for the depreciation rate, which is firm-specific and varies with a firm's mix of capital assets. The response of the long-run capital stock to percentage changes in the user cost (uniform across firms) is captured by the sum of the αs, which we refer to as the UCE.[14]

[13]If financing constraints affect K_t^* in the long-run, then, like sales and the user cost, CF_t would enter as a percentage change (see Chirinko and Schaller, 1995). There is no evidence in our data that the percentage change in CF_t has any positive effect on investment.

[14]To see that the sum of the αs represents the elasticity of the long-run capital stock with respect to the user cost, consider the following abbreviated version of Eq. (7):

$$I/K = \delta + I^n/K = \delta + \Delta K/K = \delta + \mathrm{SUM}(\alpha)(\Delta U/U) + \cdots$$

Canceling δs and rearranging yields an expression for the elasticity: $(\Delta K/K)/(\Delta U/U) = \mathrm{SUM}(\alpha)$. Note that this derivation assumes that $\Delta U/U$ is uniform across all firms. This assumption is relaxed when analyzing policy in Section 6.

4. Econometric results

In this section, we present regression estimates of the UCE. We begin with OLS regressions. Hausman tests, however, reveal possible correlation between the error term and the regressors. We therefore employ instrumental variable methods that lead to our preferred estimate of the UCE, which is approximately −0.25.

4.1. Ordinary least squares estimates

Table 2 presents OLS estimates of Eq. (7), with and without cash flow.

Table 2
Ordinary least squares regressions

$$I_{i,t}/K_{i,t-1} = \alpha_6(L)\,\Delta U_{i,t}/U_{i,t-1} + \beta_4(L)\Delta S_{i,t}/S_{i,t-1} + \gamma_4(L)\,CF_{i,t}/K_{i,t-1} + \phi_i + \epsilon_{i,t}{}^{a}$$

	Excluding cash flow		Including cash flow	
	Mean difference	First difference	Mean difference	First difference
$\Delta U_{i,t}/U_{i,t-1}$				
α_0	−0.144 (0.016)	−0.082 (0.018)	−0.088 (0.016)	−0.055 (0.018)
α_1	−0.205 (0.015)	−0.142 (0.023)	−0.155 (0.014)	−0.117 (0.022)
α_2	−0.155 (0.015)	−0.100 (0.024)	−0.123 (0.014)	−0.086 (0.023)
α_3	−0.060 (0.015)	−0.015 (0.025)	−0.024 (0.014)	−0.001 (0.025)
α_4	−0.054 (0.015)	−0.046 (0.026)	−0.037 (0.014)	−0.038 (0.025)
α_5	−0.099 (0.015)	−0.116 (0.027)	−0.087 (0.014)	−0.101 (0.026)
α_6	−0.004 (0.023)	−0.037 (0.026)	0.012 (0.022)	−0.023 (0.025)
SUM (α)	**−0.721 (0.054)**	**−0.538 (0.117)**	**−0.502 (0.053)**	**−0.421 (0.114)**
$\Delta S_{i,t}/S_{i,t-1}$				
β_0	0.120 (0.004)	0.085 (0.006)	0.079 (0.004)	0.047 (0.006)
β_1	0.082 (0.004)	0.051 (0.007)	0.033 (0.004)	0.004 (0.007)
β_2	0.067 (0.005)	0.039 (0.007)	0.029 (0.005)	0.006 (0.007)
β_3	0.033 (0.004)	0.008 (0.007)	0.006 (0.005)	0.011 (0.007)
β_4	0.021 (0.005)	0.009 (0.006)	0.006 (0.005)	0.002 (0.006)
SUM (β)	**0.322 (0.012)**	**0.192 (0.025)**	**0.153 (0.012)**	**0.049 (0.025)**
$CF_{i,t}/K_{i,t-1}$				
γ_0			0.102 (0.004)	0.130 (0.005)
γ_1			0.101 (0.004)	0.105 (0.005)
γ_2			0.036 (0.004)	0.041 (0.005)
γ_3			0.018 (0.004)	0.015 (0.005)
γ_4			0.009 (0.004)	0.003 (0.005)
SUM (γ)			**0.265 (0.007)**	**0.296 (0.016)**
R^2	0.411	0.422	0.457	0.466

[a] Estimates with micro data (1981–1991) and ordinary least squares as described in the text. Standard errors are in parentheses. The polynomials in the lag operator α_6(L), β_4(L), and γ_4(L) are of order 6 and 4 and contain contemporaneous values. To maintain comparability across fixed effect estimators, the R^2 statistic is defined to account for firm-specific intercepts as described in Appendix A.

Different depreciation rates in the model give an *a priori* reason for intercepts to vary across firms. Statistical considerations, such as persistent measurement error, also suggest the need to control for firm-specific effects. We therefore present two alternative estimators that eliminate firm ''fixed effects:'' mean differences and first differences.[15] All four regressions reported in Table 2 give dramatic results for the UCE. The sum of the distributed lag coefficients on $\Delta U_{i,t}/U_{i,t-1}$, our empirical measure of the UCE which we denote as SUM (α), ranges from -0.421 to -0.721. We can strongly reject the null hypothesis that the SUM (α) is zero, and many of the individual coefficients on $\Delta U_{i,t}/U_{i,t-1}$ are statistically different from zero as well. The hypothesis that the UCE is unity, however, as often assumed in calibrated models used for policy analysis, is also rejected.

Insofar as cash flow is an important determinant of investment, omitting it from the regression will bias the estimated SUM (α) if cash flow and the change in user cost are correlated. We examine this possibility by including cash flow in the third and fourth regressions reported in Table 2. Both the mean-difference and first-difference models strongly reject the null hypothesis that investment is independent of cash flow. Including cash flow lowers the effect of sales growth, which is not surprising given the likely positive correlation between sales growth and cash flow.[16] More important for our purposes, however, including cash flow lowers the absolute value of the estimated SUM (α) from -0.721 to -0.502 in the mean-difference regression and from -0.538 to -0.421 in the first-difference regression.

One explanation for this finding is ''income effects'' induced by financing constraints. For a firm operating in perfect capital markets, a user cost change induces substitution effects only. But as discussed in Fazzari et al. (1988a), changes in user costs will change firms' total costs and their available internal finance. Changing internal finance can affect the behavior of financially constrained firms over and above the effects arising from substitution alone. A lower investment tax credit, for example, may have standard incentive effects on the demand for capital and investment but, for financially constrained firms, the resulting decline in cash flow could reduce investment further than if the firm

[15] An F test resoundingly rejects the equality of the firm intercepts and hence the appropriateness of a pooled estimate. A Hausman (1978) test ($\chi^2(12)=116.3$) strongly rejects the independence of the firm effects and the regressors, implying that a random effects estimator would not be consistent. Interestingly, the UCE estimates for the pooled and random effects models of -0.66 and -0.63, respectively, are between the comparable estimates in the first two columns of Table 2.

[16] The effect of cash flow on the sales growth coefficients leads to the question of whether the importance of cash flow arises from financing constraints or cash flow's role as a proxy for expected demand. This issue has been considered extensively in the financing constraint literature (see the survey by Hubbard, 1998). Results vary across different studies, but evidence has been compiled to support the view that much of the cash flow effect is due to financing constraints. This issue is not of major concern in our context, however, because of our focus on the UCE.

operated in perfect capital markets.[17] The existence of these ''income effects'' is consistent with our findings in Table 2. In the regressions without cash flow, the estimated SUM (α) captures both the conventional substitution effect as well as the income effect induced by financing constraints, which affect investment in the same direction. When we add cash flow, however, the estimated SUM (α) can be interpreted as the user cost elasticity holding cash flow constant; that is, as a measure of the conventional substitution effect alone. As noted in Section 3, it is this substitution effect that represents the long-run impact of user cost changes on the desired capital stock. ''Income effects'' through cash flow operate only in the short run.

4.2. Simultaneity and instrumental variables estimates

The estimates in Table 2 may be adversely affected by correlation between the error term and the regressors (as documented by specification tests below). Indeed, simultaneity bias provides a possible explanation for low estimates of the UCE and, in particular, why our estimates are far from unity. Investment comprises a volatile component of aggregate demand, positively correlated with the business cycle, and business cycle movements correlate with interest rates. Positive investment shocks, for example, can cause positive movements in output and the demand for credit that affect the required rates of return on debt and equity. Conventional wisdom (e.g., Mankiw and Summers, 1988, p. 716) suggests that simultaneity between investment shocks and interest rates biases the UCE toward zero. Furthermore, firm investment shocks may be contemporaneously correlated with sales and cash flow, or industry investment shocks may affect the relative price of capital goods (as in Goolsbee, 1998). These problems suggest the need for instrumental variables (IV) estimation. The extensive variation in micro data will likely provide better instruments than can be obtained at the aggregate level.

Following common practice, we employ undifferenced lags of the regressors as instruments. There is a problem with this approach, however, for the mean-difference estimator when, as in the present case, instruments are pre-determined but not strictly exogenous. The problem is that the period t error term that arises following the mean-difference transformation will be correlated with the pre-determined instruments dated period t, $t-1$, $t-2$, etc. The transformed error term contains the mean of the firm's error over the entire sample; that is, $(\epsilon_1 + \epsilon_2 + \cdots +$

[17] We thank an anonymous referee for pointing out that lower cash flow may also reduce investment for poorly performing firms that move into tax loss status and lose investment incentives, or higher cash flow may stimulate investment for firms that move out of tax loss status and shelter taxable income. See Auerbach and Poterba (1987). The specific reason for the cash flow effect, however, is not important for our objective of obtaining an unbiased estimate of the UCE.

$\epsilon_T)/T$, which invalidates lags of pre-determined regressors as instruments.[18] To solve this problem, Arellano (1988) and Arellano and Bover (1995) propose an ''orthogonal deviation'' transformation for panel data that sweeps out fixed effects by subtracting the mean of future observations from each regressor. With this transformation, once-lagged, pre-determined regressors are valid instruments. The orthogonal deviations estimator is asymptotically equivalent to the first-difference instrumental variables estimator.[19] Moreover, it may be more efficient than the first-difference estimator with twice-lagged pre-determined regressors as instruments when, as usually happens in practice, a subset of the available orthogonality conditions is used. Indeed, in the results presented in Tables 3 and 4, the standard errors on SUM (α) from the orthogonal deviations estimator are 30 to 50% lower than the comparable standard errors from the first-difference estimator.

We present IV results in Table 3 for the mean-difference (possibly biased), first-difference, and orthogonal deviations estimators. (The instrument list appears in the footnote to the table.) Hausman tests reject the least squares specifications with P values of 5% or less, implying that consistent estimation requires instrumental variables.[20] We also present two measures of ''instrument relevance'' in Table 3 for each regressor: the conventional R^2 from the first-stage regression and the Shea partial-R^2. Shea (1997) shows that the first-stage R^2 can be a misleading measure of instrument relevance if the regressors are highly correlated with only a subset of the instruments.[21] Not surprisingly, instrument relevance (by

[18]The bias in the mean-difference estimator with pre-determined variables as instruments is of order $1/T$, where T is the number of time observations in the panel. Hence, this estimator is consistent as T goes to infinity. In practice, however, panel data sets usually provide a relatively small number of time-series observations for each firm. Our regressions are based on twelve time periods, which is larger than many panels, but not sufficiently large that we can confidently rely on asymptotic results that depend on large T. See Arellano and Bover (1995) and Urga (1992). The problem with pre-determined but not strictly exogenous instruments does not arise for the first-difference estimator because the first-difference transformation subtracts a single lagged value of each regressor rather than the mean value of the regressor over the panel.

[19]With OLS, the orthogonal deviations and mean difference estimates are numerically identical.

[20]The Hausman test compares the OLS and instrumental variable (IV) estimates from models using the same transformation. For the full model, the Hausman test statistic is distributed $\chi^2(17)$ under the null hypothesis of no correlation between the error term and the regressors. The P values are 0.00 for the OLS mean difference (equivalent to OLS orthogonal deviations) versus IV orthogonal deviations and 0.04 for OLS first difference versus IV first difference. When the Hausman test statistic is computed for the $\Delta U/U$ coefficients alone, the comparable P values are 0.03 for either transformation.

[21]The intuition of Shea's argument is as follows. Even though the instruments as a group might explain a large portion of the variance of the regressors, a subset of the instruments (possibly a small subset) may be responsible for the good fit in all of the first-stage regressions. Other instruments (possibly most of them) may be of little use in explaining the variance in the regressors. In this case, there may not be enough independent information in the instrument set to achieve ''practical'' identification even with high first-stage R^2s. Shea's partial R^2 statistic corrects for this problem.

Table 3

Instrumental variable estimates $I_{i,t}/K_{i,t-1} = \alpha_6(L)\,\Delta U_{i,t}/U_{i,t-1} + \beta_4(L)\,\Delta S_{i,t}/S_{i,t-1} + \gamma_4(L)\,CF_{i,t}/K_{i,t-1} + \phi_i + \epsilon_{i,t}$ [a]

	Mean difference				First difference				Orthogonal deviations			
	Coeff. estimate	Std. error	First-stage R^2	Partial R^2	Coeff. estimate	Std. error	First-stage R^2	Partial R^2	Coeff. estimate	Std. error	First-stage R^2	Partial R^2
$\Delta U_{i,t}/U_{i,t-1}$												
α_0	0.021	(0.062)	0.283	0.072	0.128	(0.100)	0.214	0.046	−0.020	(0.080)	0.258	0.051
α_1	−0.129	(0.021)	0.869	0.519	−0.121	(0.047)	0.614	0.345	−0.212	(0.037)	0.803	0.279
α_2	−0.120	(0.022)	0.866	0.474	−0.110	(0.047)	1.000	0.395	−0.128	(0.033)	0.865	0.344
α_3	0.013	(0.024)	0.847	0.386	0.066	(0.042)	1.000	0.536	−0.023	(0.029)	0.856	0.424
α_4	−0.009	(0.022)	0.863	0.461	0.015	(0.040)	1.000	0.620	−0.051	(0.030)	0.881	0.382
α_5	−0.063	(0.023)	0.810	0.442	−0.033	(0.047)	1.000	0.513	−0.095	(0.042)	0.816	0.234
α_6	0.034	(0.041)	0.880	0.327	−0.006	(0.041)	1.000	0.581	−0.028	(0.049)	0.901	0.417
SUM (α)	**−0.254**	**(0.140)**			**−0.060**	**(0.228)**			**−0.557**	**(0.157)**		
$\Delta S_{i,t}/S_{i,t-1}$												
β_0	0.028	(0.048)	0.033	0.001	0.055	(0.097)	0.021	0.005	−0.106	(0.130)	0.022	0.002
β_1	0.021	(0.009)	0.822	0.267	0.035	(0.021)	0.445	0.165	0.074	(0.018)	0.696	0.117
β_2	0.022	(0.009)	0.834	0.293	0.039	(0.013)	1.000	0.474	0.051	(0.008)	0.816	0.608
β_3	0.002	(0.007)	0.827	0.509	0.011	(0.012)	1.000	0.522	0.033	(0.010)	0.839	0.399
β_4	0.007	(0.006)	0.823	0.622	0.015	(0.009)	1.000	0.740	0.031	(0.008)	0.860	0.597
SUM (β)	**0.080**	**(0.068)**			**0.155**	**(0.091)**			**0.084**	**(0.107)**		
$CF_{i,t}/K_{i,t-1}$												
γ_0	0.316	(0.115)	0.041	0.001	0.528	(0.102)	0.021	0.002	0.514	(0.097)	0.048	0.002
γ_1	0.049	(0.026)	0.594	0.023	−0.045	(0.039)	0.201	0.029	−0.053	(0.039)	0.339	0.019
γ_2	0.033	(0.005)	0.663	0.704	0.024	(0.010)	1.000	0.396	0.010	(0.008)	0.661	0.405
γ_3	0.015	(0.005)	0.669	0.735	0.002	(0.008)	1.000	0.594	−0.002	(0.008)	0.768	0.489
γ_4	0.008	(0.005)	0.671	0.560	0.003	(0.007)	1.000	0.788	0.002	(0.006)	0.817	0.685
SUM (γ)	**0.421**	**(0.092)**			**0.511**	**(0.077)**			**0.472**	**(0.052)**		

[a] See notes to Table 2. The instruments for the mean-difference and orthogonal deviations regressions are the (untransformed) values of $\Delta U_{i,t}/U_{i,t-1}$ lagged one through nine years and $\Delta S_{i,t}/S_{i,t-1}$, and $CF_{i,t}/K_{i,t-1}$ lagged one through seven years. The instruments for the first-difference regression are the (untransformed) values of $\Delta U_{i,t}/U_{i,t-1}$ lagged two through ten years and $\Delta S_{i,t}/S_{i,t-1}$, and $CF_{i,t}/K_{i,t-1}$ lagged two through eight years. The partial R^2 statistic is based on Shea (1997).

Table 4
Instrumental variable estimates for parsimonious lag lengths $I_{i,t}/K_{i,t-1} = \alpha_2(L)\,\Delta U_{i,t}/U_{i,t-1} + \beta_4(L)\,\Delta S_{i,t}/S_{i,t-1} + \gamma_4(L)\,CF_{i,t}/K_{i,t-1} + \phi_i + \epsilon_{i,t}$[a]

	Mean difference				First difference				Orthogonal deviations			
	Coeff. estimate	Std. error	First-stage R^2	Partial R^2	Coeff. estimate	Std. error	First-stage R^2	Partial R^2	Coeff. estimate	Std. error	First-stage R^2	Partial R^2
$\Delta U_{i,t}/U_{i,t-1}$												
α_0	−0.012	(0.040)	0.283	0.153	−0.003	(0.062)	0.214	0.105	−0.014	(0.047)	0.258	0.145
α_1	−0.113	(0.017)	0.869	0.781	−0.139	(0.035)	0.614	0.483	−0.144	(0.021)	0.803	0.662
α_2	−0.093	(0.019)	0.866	0.620	−0.099	(0.037)	1.000	0.295	−0.101	(0.025)	0.864	0.452
SUM (α)	**−0.218**	**(0.046)**			**−0.241**	**(0.075)**			**−0.260**	**(0.046)**		
$\Delta S_{i,t}/S_{i,t-1}$												
β_0	0.066	(0.049)	0.033	0.011	0.047	(0.077)	0.021	0.008	0.044	(0.085)	0.022	0.004
β_1	0.019	(0.009)	0.822	0.337	0.044	(0.018)	0.445	0.202	0.056	(0.013)	0.696	0.227
β_2	0.027	(0.009)	0.834	0.315	0.044	(0.013)	1.000	0.484	0.052	(0.008)	0.816	0.613
β_3	0.006	(0.007)	0.827	0.555	0.019	(0.011)	1.000	0.555	0.028	(0.009)	0.839	0.484
β_4	0.009	(0.007)	0.823	0.626	0.016	(0.008)	1.000	0.788	0.028	(0.007)	0.860	0.668
SUM (β)	**0.127**	**(0.069)**			**0.170**	**(0.077)**			**0.209**	**(0.072)**		
$CF_{i,t}/K_{i,t-1}$												
γ_0	0.404	(0.092)	0.041	0.002	0.491	(0.093)	0.021	0.002	0.492	(0.093)	0.048	0.001
γ_1	0.027	(0.021)	0.594	0.041	−0.033	(0.036)	0.201	0.031	−0.043	(0.038)	0.339	0.020
γ_2	0.031	(0.005)	0.663	0.715	0.024	(0.010)	1.000	0.401	0.012	(0.008)	0.661	0.407
γ_3	0.014	(0.005)	0.669	0.744	0.002	(0.008)	1.000	0.595	0.001	(0.007)	0.768	0.513
γ_4	0.008	(0.005)	0.671	0.604	0.002	(0.007)	1.000	0.819	0.002	(0.006)	0.817	0.706
SUM (γ)	**0.485**	**(0.074)**			**0.486**	**(0.071)**			**0.463**	**(0.049)**		
α_1	−0.112	(0.016)	0.869	0.811	−0.138	(0.035)	0.614	0.337	−0.145	(0.021)	0.803	0.656
α_2	−0.094	(0.018)	0.866	0.643	−0.101	(0.031)	1.000	0.419	−0.104	(0.023)	0.865	0.526
SUM (α)	**−0.207**	**(0.026)**			**−0.239**	**(0.060)**			**−0.249**	**(0.032)**		

[a] See notes to Table 3. The estimates of α_1 and α_2 at the bottom of the table are from a regression including only the first and second lags of $\Delta U_{i,t}/U_{i,t-1}$ and the contemporaneous and four lags of $\Delta S_{i,t}/S_{i,t-1}$ and $CF_{i,t}/K_{i,t-1}$.

either measure) is much higher for the lagged regressors than for the contemporaneous ones because the lagged regressors are simply transformations of the instruments. (In fact, with the first differences, lags beyond the first year are perfectly predicted by the instruments.) There is also support in Table 3 for Shea's criticism of first-stage R^2 statistics. The partial R^2 measure preferred by Shea is much lower than the first-stage R^2.

The IV point estimates of SUM (α) range from the first-difference value of -0.060 to the orthogonal deviations estimate of -0.557. These estimates imply that the UCE is likely negative, but in all cases the hypothesis is strongly rejected that the UCE is unity. Yet, the standard errors of the SUM (α) estimates are relatively large, both economically and statistically. One cannot even reject the hypothesis that SUM (α) is zero for the first-difference estimates. Moreover, the policy implications of a UCE near zero versus a UCE near one half are likely much different.

The somewhat broad range of point estimates and relatively large standard errors for SUM (α) in Table 3 could be due to inefficient estimation arising from including too many lags. This problem is especially likely to arise in IV when instruments are highly correlated with one another, thus compromising identification. Across the three IV regressions, the contemporaneous and third through sixth lag $\Delta U_t/U_{t-1}$ coefficients are almost always insignificantly different from zero.

The results in Table 4 support the conjecture that more precise estimates can be obtained from a more parsimonious lag structure. Here we present regressions that include only the contemporaneous, first lag, and second lag of $\Delta U_t/U_{t-1}$. For SUM (α), the standard errors decline by a factor of at least 3, and the range of point estimates narrows substantially across the estimators. The Shea instrument relevance statistics rise markedly for the orthogonal deviations and mean difference estimators. The lower panel of Table 4 presents the coefficients on the first and second lag of $\Delta U_t/U_{t-1}$ from a regression that is similar to that presented in the upper panel but that excludes contemporaneous $\Delta U_t/U_{t-1}$, which remained insignificantly different from zero in the short-lag regression reported in the top panel of Table 4. All six SUM (α) estimates in Table 4 are negative and precisely estimated, ranging narrowly from -0.207 to -0.260.[22] While the panel has much information for generating instruments, the information is not unlimited. IV models

[22]To test the robustness of these results, we lagged the instruments an additional year. The results, which are robust to MA(1) errors, are very consistent with those in Table 4. The six SUM (α) estimates range from -0.181 to -0.223. An anonymous referee suggested that we estimate the model with only contemporaneous values of all the variables and lagged instruments. In these regressions the SUM (α) estimates range from -0.133 to -0.290 when cash flow is excluded from the regression and from -0.007 to -0.130 with cash flow included.

with long lags may have instruments that are not sufficiently ''relevant'' and may therefore lead to imprecise estimates.[23]

4.3. Summary: What is the user cost elasticity?

All our estimates of the UCE are negative. In our preferred parsimonious specification (Table 4) the UCE is precisely estimated. None of the UCE estimates in Tables 3 or 4 differ from -0.25 by more than two standard errors. These results are far away from the unitary value assumed in many studies based on Cobb-Douglas technologies. (Even the point estimates from the OLS or long-lag IV models, that have econometric problems, do not much exceed one half.) The comparatively low values of the estimated UCE we obtain may help explain why it has been so difficult for aggregate data studies to uncover the negative effect of user cost changes on investment predicted by theory. The negative effects exist but are small. Estimation of a UCE statistically different from zero therefore requires much more information than can be obtained from the limited variation available in aggregate data.

5. Comparisons to other micro-data research and additional results

Our results differ from the conclusions offered by several recent panel studies of investment and capital formation. In their survey, Hassett and Hubbard (1997, p. 375) conclude that recent empirical research on the sensitivity of investment to the user cost with micro data has resulted in substantial estimates of the UCE ranging from -0.5 to -1.0. This conclusion contrasts with our findings. While we obtain a negative and precisely estimated UCE from micro data, the value of the UCE is rather low. In this section, we compare our approach to recent research and try to reconcile differences.

Cummins et al. (1994, 1996) employ micro data at times of major tax reforms to estimate adjustment cost parameters in a q model and a cost-of-capital model

[23] We examined many different lag lengths for $\Delta U_t/U_{t-1}$ between two and six years. There was little evidence of significant $\Delta U_t/U_{t-1}$ effects beyond the second lag in regressions that included from three to six lags of $\Delta U_t/U_{t-1}$ (both with and without the insignificant contemporaneous value in the regression). The one exception is the fifth lag, in the orthogonal deviations and mean difference regressions. The bimodal lag pattern implied by a model with lags one, two, and five, however, is implausible. Also note that the Shea instrument relevance statistic for the fifth lag in the orthogonal deviations regression is relatively low and the fifth lag is unimportant in the other consistent regression employing first differences. (Recall that the mean-difference instrumental variable regression is inconsistent for a fixed number of time-series observations.) Although we consider the specification somewhat implausible, we ran regressions with lags one, two, and five (with and without the insignificant contemporaneous value). The UCE estimates were marginally larger than those in Table 4, ranging from -0.264 to -0.353, but all within two standard errors of -0.25.

based on Auerbach (1989). The authors are successful in obtaining more precisely estimated and economically reasonable adjustment cost parameters than have typically been found in previous empirical q models (most estimated with aggregate data). In this sense, this research supports the view that recent results support a larger sensitivity of investment to price variables, as summarized by q. The adjustment cost parameters estimated from the q model, however, do not give a UCE estimate, and these results are therefore not directly comparable to ours.

With some additional assumptions, however, we can roughly compare cost-of-capital results from Cummins et al. (1994) with those presented in Table 4. The regression used to obtain the estimates in their Table 9 has the form: $I/K = a + bU$, where I/K is the gross investment–capital ratio and U is a distributed lead of the level (not the percentage change) of the user cost, with preset weights that decline geometrically and sum to unity. Assuming that the intercept of this equation represents the geometric depreciation rate and subtracting the depreciation rate from both sides of this equation yields the percentage change in the capital stock (the net investment–capital ratio) on the left-hand side as a linear function of leads in the user cost level. For U.S. data, Cummins et al. (1994) report an average value for their user cost of about 25% and an average estimated value for b (the sum of the lead coefficients) of -0.66 across years of major tax reform. With these average values, a 1% permanent change in future user costs yields a 0.165% change in the capital stock ($.01 \times .25 \times -0.66 = -0.00165$). Thus, the implied UCE is -0.165, somewhat lower, but close to the range of results in Table 4, even though Cummins et al. (1994) employ a very different empirical approach.

The cost of capital regressions in Cummins et al. (1994) are for equipment investment rather than total capital spending. If structures are less price sensitive than equipment, one would expect an implied UCE from their method to be even closer to zero for total investment (which is the measure used in our study). Indeed, using the same econometric technique and similar data, Cummins and Hassett (1992) estimate separate equations for equipment and structures and find that equipment is more responsive to the user cost. Employing the same assumptions discussed above to derive UCEs implied by the Cummins and Hassett (1992) estimates yields -0.23 for equipment and -0.07 for structures. Thus these estimates also appear to be less than those we present in Table 4. Note that the Cummins and Hassett equipment UCE is roughly the same as the UCE we estimate for equipment and structures combined.[24] While this recent research has

[24]Using aggregate data and panel data for 15 classes of equipment assets, Clark (1993) measures variations in the user cost by variations in the investment tax credit (to attenuate measurement error), and reports UCE estimates ranging from -0.25 to his preferred estimate of -0.40. With the assumptions that the UCE for structures is lower by the same proportion as in the Cummins and Hassett study and that equipment's capital stock share is 0.55, Clark's UCE estimate for total investment would be approximately -0.27, very close to our preferred estimate of -0.25.

had more success in precisely estimating economically reasonable capital adjustment costs, the implied UCE estimates are small in magnitude, consistent with our findings and in contrast with the general conclusion in the Hassett and Hubbard (1997) survey.[25]

Cummins et al. (1994) argue that the sensitivity of investment to q or the user cost is best estimated in years of major tax reforms, to mitigate concerns about endogeneity and measurement error. They find that including years without major reforms lowers the response of investment to q and user cost changes. One of the major tax reforms they identified, 1986, is in our regression sample. To test whether our results are driven disproportionately by this year, and hence whether the estimates presented above may be biased toward zero because they are based on regressions that include years without major tax reforms, we use dummy variables to isolate the effects of 1986 on the SUM (α) in the regressions reported in Table 4. Unlike Cummins et al. (1994), we find that our UCE estimate is not much different in the major tax reform year than in the other years. Interactions between a 1986 dummy and $\Delta U_t/U_{t-1}$, $\Delta U_{t-1}/U_{t-2}$, and $\Delta U_{t-2}/U_{t-3}$ are virtually never significant, either individually or as a sum, in any of our regressions.[26]

Finally, we note that none of the investment results discussed in this section include cash flow. As we discussed in the previous section, cash flow has strong statistical effects in investment equations, and we find that its addition reduces the UCE estimates substantially. This difference in specification may account for some of the differences between our results and those of other micro studies.[27]

6. Implications for tax policy

As we have discussed in Section 1, the empirical UCE is a key parameter for policy analysis. It represents the long-run effect on the desired capital stock of policies that change the user cost of capital. In this section, we consider the

[25] Another extensive recent study of investment with micro data is Caballero et al. (1995). Using a very different method from ours they obtain a wide range of UCE estimates for equipment (from zero to -2.0 with a mean around -1.0) with plant-level data across different two-digit SIC industries. Rather than an investment equation, Caballero et al. (1995) estimate a co-integrating relation between the capital–output ratio and the user cost (assumed I(1)). This study maintains several different assumptions and uses different data than in our work, and a reconciliation is beyond the scope of the present paper.

[26] The largest effect is in the orthogonal deviations regression including contemporaneous $\Delta U_t/U_{t-1}$. The sum of the coefficients on the 1986 dummy interacted with the contemporaneous, first, and second lag of $\Delta U_t/U_{t-1}$ is 0.310 with a standard error of 0.150. Although this estimate is significant at the 5% level, note that its positive sign suggests a smaller UCE for 1986, in contrast with the findings of Cummins et al. (1994).

[27] Cummins et al. (1994, Table 10) include cash flow in additional cost-of-capital regressions. In tax-reform years, which they use to estimate the sensitivity of investment to the cost of capital, the addition of cash flow also tends to reduce the cost of capital coefficient, by an average of 12%.

implications of our econometric findings for evaluating several policy proposals. We must proceed with caution, however, in using non-structural results for policy analysis. Panel data allow us to develop a new test of the quantitative importance of the Lucas Critique and hence to test the sensitivity of our results to possible instability arising from the major policy changes in the Tax Reform Act of 1986. The next subsection discusses this test, which shows that the investment regressions in Table 4 remained quite stable over the tax reform period. We then use our preferred estimate of the UCE to evaluate the effect of three tax policy initiatives on capital formation: a cut in the capital gains tax rate, reinstatement of an investment tax credit, and adoption of a ‘‘flat tax.’’

6.1. The Lucas critique and the tax reform act of 1986

The Tax Reform Act of 1986 was a significant policy change that raised the user cost during our sample period and provides an opportunity to test the empirical importance of the Lucas Critique. If it were the case that the empirical UCE (which is not derived from a policy-invariant structural model) changed with the Tax Reform Act of 1986, one would expect to observe large residuals around the time of the policy change in our specification that maintains a uniform UCE over the sample. Because the user cost increases were, at least in part, anticipated prior to implementation we might expect systematic increases of investment in 1985, possibly 1986, relative to 1987 when the user cost rose.[28]

Including time dummies in the regressions (which obviously requires panel data) provides a test for the systematic changes in the investment–capital ratio. We include time dummies in the IV regressions with parsimonious lags and perform pair-wise equality tests on the 1985, 1986, and 1987 time dummy coefficients. We also test the joint equality of the time dummy coefficients for 1985, 1986, and 1987. The lowest *P* values we obtain from these tests are 0.241 for mean differences, 0.343 for first differences, and 0.165 for orthogonal deviations. The null hypothesis of stability over the tax reform period cannot be rejected. Moreover, the time dummy coefficient for 1987 is slightly higher than those for 1985 and 1986, further evidence against the view that the anticipation of tax reform led firms to intertemporally substitute investment from 1987 to 1985 or 1986.[29] These results help to mitigate concerns about the quantitative importance of the Lucas Critique in our context.

[28]The effective implementation dates varied for different parts of the Tax Reform Act of 1986.

[29]The range of UCE estimates from the instrumental variable regressions with time dummies was marginally higher in absolute value than those reported in Table 4: −0.231 for mean differences, −0.279 for first difference, and −0.326 for orthogonal deviations.

6.2. The effects of current tax initiatives

We follow a two-step process to estimate the effect of specific tax initiatives on the capital stock. First, we determine the effect of the tax change on the user cost of capital. Because user costs differ across firms, this calculation is performed at the firm level, and therefore requires micro data. The weighted average percentage change in the user cost for our sample is defined as:

$$\Delta U^{w}/U^{w} = \sum_{i} w_{i}(\Delta U_{i}/U_{i}) \tag{8}$$

where w_i is firm i's share of the total sample capital stock (K_i/K). Second, the percentage change in the aggregate capital stock for our sample (K) is estimated from:

$$\begin{aligned}\Delta K/K &= \frac{1}{K}\sum_{i} \Delta K_{i} = \sum_{i} \frac{\Delta K_{i}}{K_{i}}\frac{K_{i}}{K} = \sum_{i} \mathrm{SUM}(\alpha)\frac{\Delta U_{i}}{U_{i}} w_{i} \\ &= \mathrm{SUM}(\alpha)\Delta U^{w}/U^{w}\end{aligned} \tag{9}$$

While the Compustat sample may not perfectly represent the U.S. economy, its substantial coverage suggests that these estimates will be a good approximation to the aggregate effect of policies that change the user cost.

To estimate the firm-specific percentage decline in the user cost as the result of the recent reduction of the top marginal capital gains tax rate from 28% to 19.8%, we follow the approach of Fazzari and Herzon (1996), who use assumptions about corporate financial structure that are representative for the U.S. economy.[30] Weighting these percentage changes by the firm capital shares from 1991, the final year in our sample, yields a weighted average reduction in the user cost ($\Delta U^{w}/U^{w}$) of 1.89%. The estimated impact of this change on the long-run capital stock is given in the first column of Table 5 for a UCE of -0.25, consistent with our

[30]These assumptions include the following: firms pay 50% of their income as dividends and 50% as capital gains; 30% of new investment is financed with debt and 70% with equity; the real required rate of return on equity is 6%; and expected inflation is 3%. For the results reported here, each firm's percentage decline in the user cost is determined as follows. The user cost can be expressed as the product of components representing relative prices (P_i), corporate taxes (T_i), and a required rate of return (R_i) that includes depreciation and the tax-adjusted opportunity cost of funds r that the firm must attain to compensate its investors: $U_i = P_i^{*}T_i^{*}R_i$ and $R_i = r + \delta_i$. The capital gains tax rate affects r, and the percentage change in the user cost from a capital gains tax rate cut can be expressed as $\Delta U_i/U_i = \Delta r/(r + \delta_i)$. The term $(\mathrm{r}+\delta_{\mathrm{i}})$ is taken from our micro data. Fazzari and Herzon's estimates imply that r will fall by 7.42% from a base of 4.53% after the capital gains tax rate cut, which implies that Δr equals .0742 * .0453. Note that δ, which is ignored in many studies, plays a large role in determining $\Delta U/U$. If δ is set to zero, the percentage change in the user cost triples. In our calculations, and in contrast with Fazzari and Herzon, we have not adjusted the capital gains tax rate for the expected holding period of assets. Thus our figures are an upper bound on the impact of cutting the capital gains tax rate.

Table 5
Policy Effects[a]

	Capital gains tax rate cut	10% Investment tax credit	Flat tax
User cost elasticity:	−0.25	−0.25	−0.25
$\Delta U/U$	−1.89%	−14.25%	−14.15%
$\Delta K/K$	+0.47%	+3.56%	+3.54%
$\Delta Y/Y$	+0.14%	+1.07%	+1.06%

[a] The $\Delta U/U$ row shows the estimated percentage decline in the user cost of capital which is a weighted average of estimated firm-specific percentage changes in the user cost. The weights reflect each firm's share of capital in the data sample. The $\Delta U/U$ for the capital gains tax is based on Fazzari and Herzon (1996), as described in the text. The user cost decline for the investment tax credit assumes a 10% credit for both equipment and structures. The decline for the flat tax policy is based on the authors' calculations as described in the text. The $\Delta K/K$ row shows the percentage change in the long-run capital stock as a result of the user cost decline given a user cost elasticity of −0.25. The $\Delta Y/Y$ is the long run percentage change in output as a result of the increase in the capital stock assuming a 0.3 elasticity of output with respect to the capital stock.

regression results. This policy yields only about half a percentage point increase in the long-run capital stock. Assuming a typical output elasticity with respect to capital of 0.3, the capital gains tax cut is predicted to have an impact on the level of output of only 0.14%.

Reinstating the investment tax credit to 10% would have a more substantial impact.[31] We compute that this change would lower the user cost by 14.25%. With a UCE of −0.25 and a output–capital elasticity of 0.3, this change raises the long-run capital stock by 3.56% and output by 1.07%.

In our third policy scenario, we analyze the flat tax, which would allow firms to ''expense'' investment and would drive the tax component of the user cost measure to unity.[32] We calculate the tax component for the final year in the sample (1991) for each firm and compute the percentage change in the firm's user cost that would result if this tax component went to unity. The weighted average of these percentage changes (with 1991 capital shares as weights) is −14.15% (remarkably close to the change we compute for a 10% investment tax credit). We

[31] Note that this a substantial subsidy relative to historical experience because it gives a full credit to both equipment and structures.

[32] The flat tax would have another effect on the user cost that we do not measure in this exercise. Interest payments would no longer be deductible for corporate tax purposes. This change would raise the user cost, holding pre-tax interest rates constant. Whether pre-tax interest rates rise or fall is debatable. Hall (1996) and Toder (1995) conclude that pre-tax rates will decline, while Feldstein (1995) suggests the opposite. Even if pre-tax rates fall, it is unlikely that this channel will be sufficient to lower the net-of-tax interest rate. For these reasons, we believe the effect of eliminating the corporate interest expense deduction is not substantial. The calculations presented in Table 5 should be viewed as an upper bound on the magnitude of the overall stimulus.

use this figure to estimate the impact of the Hall and Rabushka (1995) flat tax proposal on the long-run capital stock and output. With our UCE estimate of −0.25, the capital stock is predicted to increase by 3.5% and the long-run level of output by 1.1%. In his simulation of the Hall-Rabushka flat tax, Auerbach (1996, Table 2.3, column 2) finds that output per capita increases by 8.4% in the long run.[33] This result is based on a unitary UCE implicit in the Cobb-Douglas production function. Our results are also less than a third of the increase predicted by Hall and Rabushka due to the increase in the capital stock alone.[34]

7. Conclusion

This study investigates the empirical user cost elasticity of capital formation with an extensive micro dataset. The estimated UCE depends to some extent on the specification and econometric technique employed. Accounting for financing constraints and simultaneity results in a range of UCE estimates from −0.06 to −0.56. Employing a parsimonious lag specification suggested by various regression diagnostics leads to a precisely estimated UCE of approximately −0.25. This value is consistent across several panel-data estimation techniques that control for firm fixed effects, including the new Arellano and Bover (1995) orthogonal deviations transformation that generates substantially lower standard errors. Our results imply that higher user costs do indeed reduce capital formation. Our UCE estimate, however, is much lower than the value of unity frequently assumed in applied research.

It has been suggested that a modest response of capital formation to its user cost is somehow inconsistent with the neoclassical view of the firm. This conclusion, however, depends on how one defines the ''neoclassical'' view. There is no logical inconsistency between a low UCE and the hypothesis that profit-maximizing firms respond to price incentives. The low UCE suggests simply that substitution possibilities are limited by the firms' production technologies. It is the case, however, that a low UCE implies that price incentives have a quantitatively smaller impact on capital formation than many economists often assume. For example, our finding suggests that quantitative models that rely heavily on prices to allocate capital – especially those in the real business cycle tradition – may be

[33] Auerbach's estimate reflects general equilibrium effects not accounted for in our analysis. In particular, interest rates fall by 1.4 percentage points in Auerbach's simulations, whereas our calculations hold the interest rate fixed.

[34] This calculation is based on the mid-point of the 2 to 4% output increase range that Hall and Rabushka (1995, p. 87) predict over 7 years. Because Hall and Rabushka assume a 0.25 elasticity of output with respect to capital, a 3% output increase translates into a 12% increase in capital, which can be compared to our figures in Table 5. Hall and Rabushka also argue that the flat tax would increase the efficiency of the capital stock resulting in further increases in output. We cannot assess this prediction in our framework that focuses on the overall quantity of capital.

misspecified. Our modest UCE estimate also implies a correspondingly modest effect of interest rates on investment, weakening the traditional monetary transmission mechanism. Finally, the effects of policy initiatives to stimulate capital formation by cutting taxes are attenuated by a low UCE. Cutting the capital gains tax rate from 28 to 19.8% would raise the long-run capital stock by only a trivial amount with a UCE in the range of our estimates. A low UCE reduces the benefits of an investment tax credit relative to its tax cost. Replacing the current tax system by a flat tax would increase the long-run capital stock by about 3.5%, much less than is claimed by flat-tax proponents. There may be good reasons for supporting these tax policies, and thus for shifting the burden of taxation away from upper-income taxpayers. But a substantial increase in the capital stock is not one of them.

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Appendix A. Data and R^2 definitions

The accounting data are from the Compustat Industrial Database maintained by Standard and Poor are described below. The Data Resources, Inc. (DRI) data used to construct industry-specific user costs are described in Section 2.

Investment

Capital expenditure (on property, plant, or equipment) from firms' uses of funds statement. Nominal investment is deflated by a weighted average of capital asset price indexes from DRI with weights determined by two-digit S.I.C. industry asset usage. (See Section 2 for more information.)

Sales

Gross sales during the year reduced by cash discounts, trade discounts, and returned sales or allowances to customers. Nominal sales Figs. from Compustat are deflated by industry-specific output price indexes provided by DRI.

Cash flow

Cash flow is the sum of several variables from Compustat. It includes:

1. Income before extraordinary items;
2. Depreciation and amortization;
3. Deferred Taxes;
4. Equity in net loss (earnings); and
5. Extraordinary items and discontinued operations.

The first two components of cash flow (income and depreciation) are seldom missing from firms' income statements. If the a firm reports a missing value for either one of these variables, we produce a missing value for cash flow. The last three items, however, are missing a greater percentage of the time. We assume that when they are missing, their values are economically insignificant, and we set them to zero.

The replacement value of capital

The capital stock appears in the denominator of our dependent variable. The book values of gross or net property, plant, and equipment may severely understate the current value of the capital, especially in periods of high inflation. Salinger and Summers (1983) present an algorithm for approximating the current replacement value of capital using accounting data such as that supplied by Compustat. We modified the original algorithm to make it more useful in approximating capital stocks for a wider variety of firms.

The basic idea behind the algorithm is to build iteratively a replacement value series using three steps. First, take the previous year's value and inflate it in proportion to aggregate inflation to obtain the capital stock's replacement value today in the absence of other changes. Second, add the value of the current year's investment, and third, account for capital lost to depreciation. The resulting nominal capital stock is then deflated as described above for investment. The details of this calculation appear in an extended appendix available from the authors.

Definition of R^2

To maintain comparability in the R^2 statistic across models with firm-specific intercepts, we compute R^2 in Table 2 as follows. For the models that include lags of the percentage change in the user cost and the percentage change in sales, R^2 is defined with regression residuals ($e_{i,t}$) from:

$$e_{i,t} = (I/K)_{i,t} - \hat{\phi}_i - \sum_{h=0}^{6} \hat{\alpha}_h \frac{\Delta U_{i,t-h}}{U_{i,t-h-1}} - \sum_{h=0}^{4} \hat{\beta}_h \frac{\Delta S_{i,t-h}}{S_{i,t-h-1}} \tag{A.1}$$

where $\hat{\alpha}_h$ and $\hat{\beta}_h$ are regression coefficients. The estimated firm-specific intercept is given by:

$$\hat{\phi}_i = (1/T) \sum_{t=1}^{T} \left((I/K)_{i,t} - \sum_{h=0}^{6} \hat{\alpha}_h \frac{\Delta U_{i,t-h}}{U_{i,t-h-1}} - \sum_{h=0}^{4} \hat{\beta}_h \frac{\Delta S_{i,t-h}}{S_{i,t-h-1}} \right) \tag{A.2}$$

where T is the number of years in the panel. This definition of the residuals gives the conventional R^2 for the mean-difference estimator. For the first-difference estimators, this definition may result in R^2 statistics that do not necessarily rise when additional variables are added to the regression model. We use this definition of R^2, appropriately modified to account for the addition of the cash flow-capital ratio in the regression equation, for all the OLS fixed effects regressions reported in the paper.

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