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THE IMPORTANCE OF MEASUREMENT ERROR IN THE COST OF CAPITAL

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ABSTRACT

Conventional estimates of the impact of taxes on investment may be seriously biased by measurement error in the cost of capital. The existence and size of such error, however, has not been documented. Using panel data on different types of capital equipment, this paper provides direct evidence of measurement error in the tax component of the cost of capital, accounting for about 20 percent of the tax term's variance. Correcting for the error with IV estimation shows that taxes significantly affect both prices and investment and that conventional results may be off by as much as a factor of four.

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I. Introduction

Ever since the derivation of the marginal tax cost of capital established a way of quantifying the effect of tax policy on corporate investment (Hall and Jorgenson, 1969), there has been considerable applied interest in public economics as to how the tax term should be measured and a consequent fear that, in practice, it may be suffer from potentially severe measurement problems by neglecting a variety of important factors.[1] The existence of measurement problems in the tax term is more than just an interesting detail, however. It implies that estimates using the tax term will be biased toward zero. Since the voluminous empirical literature relating the cost of capital to investment has usually found that tax policy has a small impact on real investment, documenting the existence and size of measurement error in the tax term is a fundamental issue for determining whether tax policy matters for investment.[2] Despite its importance for both applied public finance and for the ongoing debate in the investment literature over price responsiveness, the basic task of establishing direct evidence on the role of measurement error in the tax term has not been attempted.

In this paper I do two things. First, I explicitly test for the presence of measurement error in the tax term and calculate the implied size of such error. Using the econometric methods of Griliches and Hausman (1986), I document that significant error does exist and that its variance makes up approximately 20 percent of the total variance of the tax term. Second, I examine how important the measurement error is for conventional estimates of investment. The results suggest that work using first-differenced data (which makes measurement problems worse) may underestimate the true effects of tax policy by as much as a factor of four. Re-estimating the equations with instruments or in ways that are less susceptible to measurement problems show significant effects of tax policy on both prices and investment.

The paper proceeds in several parts. Section II describes the problems with the conventional investment literature and the econometric approach to dealing with measurement error in panel data. Section III describes the data used in the study. Section IV presents the results establishing the existence of measurement error. Section V considers the implications for investment equations. The final section concludes.

II. THE INVESTMENT LITERATURE AND MEASUREMENT ERROR

A. The Standard Investment Problem

Conventional empirical work in the investment literature estimates the price elasticity of investment demand by regressing investment (a quantity) on a measure of the cost of capital (a price). A standard regression, starting from the neoclassical cost of capital, might be something like

$$\Delta \ln(K_t) = \alpha + \Delta \ln(TAX_t) + \Delta \ln(Y_t) + \varepsilon_t \qquad (1)$$

where K is the capital stock, TAX is the cost of capital, and Y is output. In other words, the percent change in capital for a firm depends on the percent change in the cost of capital and the percent change in output.[3]

Empirical estimates of equations such as (1) have tended to find quite small elasticities on the tax term. I present a representative regression here using disaggregated data by asset for 22 classes of equipment from 1963-1988. These data are described in more detail in section III. The dependent variable is the change in the log of the net capital stock for the asset-year and the explanatory variables are the change in the log tax cost of capital for the asset and the change in the log of real GDP lagged one year.[4]

$$\Delta \ln (K_{jt}) = \underset{(.005)}{.030} - \underset{(.061)}{.103}\, \Delta \ln (Tax_{jt}) + \underset{(.117)}{.382}\, \Delta \ln (GDP_{t-1}) + \gamma_{jt}.$$

$$n = 572,\ R^2 = .05$$

The coefficient on output is positive and significant suggesting that when the economy grows, firms invest. The coefficient on the tax cost of capital, however, is small and not significantly different from zero. A 10 percent investment tax credit at the mean corporate income tax rate would raise investment less than 2 percent. Taken at face value, this regression suggests that investment demand is not very responsive to tax policy.

The problem with interpreting this regression is the same one facing the conventional investment literature. If there is measurement error in the cost of capital, the coefficient on the tax term is biased toward zero so it is impossible to say if the low coefficient in this regression results from investment actually being unresponsive to prices or rather from poor measurement. Griliches and Hausman (1986), however, show that if one has panel data and if the error can be characterized by some simple random processes, then looking at the estimated effects of tax policy over different time periods can identify the importance of measurement error.

Doing this search using quantities, as in the standard investment literature, might seem a natural place to begin but there is a problem with trying to document the existence of measurement error using only data on the quantity of investment. That problem is the potential existence of an upward sloping supply curve for capital (see Goolsbee 1998a; 1998b; 1999). With a rising supply price in the short run, the estimated elasticity in investment data may be small even without measurement error, simply because of simultaneity bias. In other words, tax policy may drive up prices in the short-run and this may reduce the amount that investment increases. By altering the short-run dynamics, such a situation will tend to confound the tests for measurement error that I describe below.

Although a rising supply curve may complicate estimation using quantities, it does mean that capital goods prices should be immediately responsive to tax policy. This makes prices a potentially good place to test for measurement error. I will, therefore, first document the existence of measurement error using regressions on capital goods prices and demonstrate how the problem can be fixed. After doing that, I will then examine how much the error affects conventional estimates of investment demand.

B. Detecting Measurement Error in Panel Data

The relationship of the tax cost of capital to the price of capital good j in year t can be written

$$\ln(P_{jt}) = \alpha_j + \beta(TAX_{jt}) + \Gamma' Z_j + \varepsilon_{jt}, \qquad (2)$$

where Tax is the log of the tax cost of capital for asset j in year t, P is the real price of the asset, and Z contains other control variables. An upward sloping supply curve means that β should be less than zero (i.e., lowering taxes raises prices). If there are problems associated with measuring the true tax term, however, so that the observed Tax differs from the true measure, Tax^*, by some amount v (i.e., $Tax = Tax^* + v$), this will tend to bias the estimated β from equation (2) toward zero. Econometric methods can be used, however, to distinguish a b that is small because of measurement error from a b which small in reality.[5]

Griliches and Hausman (1986) present the standard econometric methods for dealing with measurement error in panel data. First, they verify that under standard assumptions, first differencing data with measurement error makes the bias worse. They also show, however, that taking longer differences of the data such as the difference between time t and time t-2 or time t-3 will reduce the bias induced by measurement error. To see why, note that if the measurement error is not serially correlated, the estimated coefficient from a differenced regression of any length s will converge to a coefficient *B* which differs from the true b according to:

$$B_S = b\left[1 - \frac{2s_v^2}{Var(\Delta_S Tax)}\right], \qquad (3)$$

where s_v^2 is the variance of the measurement error and $Var(\Delta_S Tax)$ is the variance of the tax term differenced S years (e.g., for the conventional first difference, s=1). In standard cases, the variance of the tax term gets larger the longer is the difference so the bias shrinks.

This provides a simple test for measurement error. If measurement error is present then the coefficients on the tax terms from first differenced regressions should be smaller (in absolute value) than the coefficients from second differenced regressions. The second difference coefficients should be smaller than the third difference, and so on. As the difference gets longer, they should increase monotonically (in absolute value) toward the true b.

Further, Griliches and Hausman show that if measurement error exists, correcting for it in panel data does not require external instruments. Certain lagged levels of the tax term can serve as instruments for the differenced tax terms.[6] When estimated with IV, each of the equations should yield estimates of the same true b. In addition, I can use equation (3) for two equations differenced of different lengths of time to solve for the implied variance of the measurement error and the implied true coefficient. Doing so requires only the coefficients in the two equations and the variance of the two tax terms.[7]

III. DATA AND SPECIFICATION

The primary independent variable of interest is the tax cost of capital, (1-itc-tz)/(1-t), where itc is the investment tax credit, t is the corporate income tax, and z is the present value of depreciation allowances. These data come from Dale Jorgenson and the details of their calculation can be found in Jorgenson and Yun (1991). They cover 22 types of capital equipment for the 1963-1988 period. This is the variable used in conventional estimates to evaluate the effect of tax policy but may suffer from measurement error.

The tax term varies by asset as well as by year both because of different depreciation allowances by asset type and because the investment tax credit often varies by asset class. There is greater variation in the tax term over time than across assets, however. The standard deviation in the tax term across time for the median asset is .082 while the standard deviation across assets in the median year is .032.

The other component of the full cost of capital is the real interest rate plus the depreciation rate. Since the specification will be in logs, this term will enter additively. For the depreciation rate, I use the standard estimates by asset type from Jorgenson and Sullivan (1981). It is fixed over the sample. For the real interest rate I use the Baa bond rate minus the percent change in the GDP deflator, both of which are collected in the *Economic Report of the President*. These are the same for each asset but vary over time.

The other explanatory variables include the log of real GDP in the preceding year (also from the *Economic Report of the President*) and a variable to account from the Nixon price controls. The price controls began in August of 1971 and ended in April of 1974 so this variable 1/3 in 1971 and 1974 and 1 in 1972 and 1973.

The main dependent variable will be the log of the real price of a given asset type in a given year. This is measured using the deflators in *Fixed Reproducible Tangible Wealth 1925-1989* (U.S. Department of Commerce, 1993) divided by the GDP deflator. A few specifications will use the output price deflators for 85 capital goods producing industries (at the four digit SIC code level) as reported in the NBER Productivity Database (described in Bartelsman and Gray, 1996) as a comparison to the asset class data. The second major dependent variable will be the log of the net capital stock by asset type (in constant dollars). This is also reported in *Fixed Reproducible Tangible Wealth 1925-1989*.

The means and standard deviations for all of the variables used in are listed in table 1. For simplicity I list only the first differences.

The empirical specification, therefore, will look at the various changes over time in the logs of these variables. The specifications will be variants of

$$\Delta_S \ln(P_{it}) = \alpha + \beta(\Delta_S \ln(Tax_{jt})) + \gamma_1(\Delta_S \ln(r_t + \delta_j)) + \gamma_2(\Delta_S \ln(GDP_t)) + \gamma_3(\Delta_S \ln(C_t)) + \varepsilon_{jt},$$

where C is the price control variable and the S subscript on Δ is the length of the differencing. First differencing the data involves looking at the difference between time t and time t-1, second differencing the change between time t and t-2, and so on. In some specifications I will use the change in the log of the net capital stock as the dependent variable rather than the change in the log price.

IV. Results

A. The Existence Of Measurement Error

In table 2, I present the results from the regressions using data differenced of various lengths. The change in the real price is the dependent variable. The theory suggests that if measurement error is present, the coefficients should get larger (in absolute value) the longer is the differencing. The first column of table 2 presents a first differenced regression of changes in the log of prices on one year changes in the log of the tax term, the log interest plus depreciation rate, the log of real GDP, and the variable for the Nixon price controls. The results show a β very close to zero and not significant.[8] The magnitude of the coefficient indicates that a 10 percent ITC at the mean corporate tax rate would raise prices only 0.6 percent. Barring measurement error, this result would call into question the existence of an upward sloping supply curve. Coupled with the similar coefficients in conventional investment regressions, it would seem to support the idea that demand is not responsive to tax changes.

Column 2, however, moves to the "second" differenced regression (the change between year t and year t-2) which ought to reduce the measurement error bias and the coefficient on the tax term increases substantially, in absolute value. Now the 10 percent ITC raises prices almost 2.6 percent. Column (3) moves to the third difference and the effect rises to 3.8 percent. Column (4) moves to the fourth differenced regression of column (4), the effect rises to 4.3 percent. These increases are exactly the prediction of measurement error. The increased tax coefficients may even be biased downward by the tendency of the capital supply curve to become more elastic over time.[9]

B. The Size of the Error

Based on this evidence of the measurement error's existence, I can use the equation for the bias in the coefficient (equation 3) coupled with the estimates from any two differenced regressions to solve for the size of the measurement error (s_v^2) needed to generate the observed coefficients and to calculate the implied "true" coefficient b.[10] Taking the ratio of two equations differenced of lengths Y and Z and then solving for the variance of the error yields

$$s_v^2 = \frac{Var(Tax_Y)\left(\frac{B_Y}{B_Z} - 1\right)}{2\left(\frac{B_Y Var(Tax_Y)}{B_Z Var(Tax_Z)} - 1\right)}. \qquad (4)$$

Plugging s_v^2 into either equation then yields the implied true b.

With four different regressions there are six possible pairs of coefficients. Using each pair, I calculate the implied coefficients and variance of the error and list them in table 3. In theory, each pair should give the same true coefficient and the same variance of the measurement error. In practice, the six estimates of s_v^2 form a tight band with a mean of 0.00086 or 19 percent of the log tax term's variance. The implied b coefficients also form a relatively tight band with a mean of -.301.

Thus the error in measuring the tax term appears to be substantial and suggests that the coefficients from first-differenced regressions on prices may be off by as much as a factor of ten. As a comparison, it is useful to consider the implied size of the measurement error in other variables. The first thing apparent from the coefficients on output and the interest rate terms in table 2 is that they do not follow the monotonically increasing pattern predicted by measurement error. Further, even using the largest differences in the coefficients--first versus third difference for GDP growth and second versus third difference for the interest rate term--if I calculate the implied size of the measurement error using the same method as above, the implied variance of the measurement error in output is less than 0.3 percent of the variable's variance for the log of real GDP and about 2.5 percent for the log of the real interest and depreciation rates. In these data, both in the relative as well as the absolute sense, error in the tax term seems most fundamental.

C. Correcting for Measurement Error with Instrumental Variables

The implied true b coefficients calculated above suggest that the bias in first differenced regressions is substantial. One need not rely on such implicit calculations, however. Griliches and Hausman (1986) show that if the error is not serially correlated but the tax terms are, then various lagged levels of the tax term can serve as valid instruments. The instruments used here are the levels of the log tax term lagged up to five years as indicated at the bottom of each column. The results were generally quite robust to the choice of instruments and the first stage R^2 s are all above .3. Griliches and Hausman also show that by taking longer lags, it is possible to test for the presence of serial correlation in the measurement error using a Hausman test.

Table 4 shows that re-estimating the price regressions of table 2 but correcting for measurement error with instrumental variables yields results which are consistent across specifications and agree with the both the longer differenced results and the implied b's in table 3. In column (1a), I repeat the first differenced regression which, without instruments, showed almost no effect of taxes on prices. Now the coefficient is large, significant, and very close to the longer differenced results. A 10 percent ITC raises prices by 5.5 percent. Column (1b) tests for serial correlation in the measurement error by estimating the same equation as (1a) but using only the log tax terms lagged four and five years as instruments since they are still valid instruments under many types of serial correlation.[11] The coefficients on the tax term are similar and a Hausman test does not reject the hypothesis that they are identical.

Columns (2)-(4) then re-estimate each of the longer differenced regressions using the same IV approach as in (1a) and in every case the resulting tax coefficient is negative, large, and significant. None of them is significantly different from any other and they are all close to the longer differenced estimates of Table 2 and to the calculations of the implied true b's from table 3. They suggest that a 10 percent ITC raises prices between approximately 4 and 7 percent.

D. Sensitivity Analysis

Next, I provide some sensitivity analysis of the results presented. For brevity I will look at the first differenced specification and compare the results from an OLS regression (column 1) to those using IV (column 2) to test for measurement error. Each regression also includes the control variables of the baseline specification. In almost all cases, the results using longer differences rather than IV yielded similar evidence on the existence of error except in those cases mentioned.

First, the effects of tax policy need not be the same for all assets nor need the error itself need be the same size, so I evaluate the responses for certain types of assets. In doing so, I also deal with outliers since the most obvious source of outlying observations is computer equipment. The office computing equipment category contains all of the ten largest annual percentage price changes in the sample and might be only spuriously correlated with tax policy. Leaving out computers, as reported in the top row of table 5, reduces the apparent magnitude of the measurement error but it is still relatively important. The 10 percent ITC raises prices by approximately 2.5 percent when corrected for error versus 0.4 percent without the correction.

Regarding other asset types, Goolsbee (1998a) argues that upward sloping supply curves are most prevalent in heavy machinery and large transportation equipment so the price based tests may only be valid for them.[12] Restricting the sample to those asset types (presented in the next row of table 2) shows that error is important and that there is a significant impact from taxes once corrected for it. The 10 percent ITC raises prices 3.6 percent versus 0.7 percent without a correction. The third row then presents results from the regressions on the other asset types (excluding computers) and here there is no evidence that taxes affect prices with or without controlling for error. Both the effects are close to zero.

In the bottom panel of the table, I turn to the data on 85 different four-digit level SIC codes of capital goods producers in the NBER Productivity Database. The measure of prices here is the output deflator for these industries. I use these data because the number of industries is much larger than in the asset type data. They are not direct matches to the tax data, however, so they are not the preferred estimates. The fourth row of table 5 presents the SIC regressions and demonstrates that the first differenced data show a small but significant impact of changing taxes on the change in prices. The 10 percent ITC raises output prices about 1.1 percent. Correcting for measurement error using IV, however, raises this impact to 5.5 percent. The SIC code data is one place where the longer difference results are not the same as the IV results. The fourth difference regression for the SIC data did show a significant increase in the tax coefficient but only to -.13 rather than -.28 as in the IV results.

The next bottom two rows of the table divide the SIC data into the machinery and transportation equipment industries and the "other" industries (again, excluding computers). Although there is a larger tax coefficient (in absolute value) of the machinery goods after correcting for measurement error than in the other industries, the difference is not very large in these data. Here, the results from either industry point toward the existence of some measurement error.

Next, in table 6, I present results that show the impact of other control variables and sample periods to emphasize the robustness of the results. The first row repeats the baseline specification with the controls for output, price controls, and the interest plus depreciation rates. In row 2, I show the results without any controls. The only explanatory variable is the change in the log tax term. Again there is clear evidence for measurement error of approximately the same size as in the baseline so the included controls are unlikely to be the source of a spurious correlation. In row 3, I restore the standard controls and add the change in the log of the real exchange rate in DM as calculated in Goolsbee (1998a). Though unreported for reasons of space, the coefficient on the exchange rate term was highly significant but did not reduce the implied importance of measurement error in the tax term. In fact, the measurement error implied in this set of equations appears to be even larger than in the baseline.

Once I add year dummies to the regressions (reported in the fourth row of the table) the evidence for measurement error becomes quite imprecise. These regressions are identified only off of the variation in the change in the log tax term across assets within a year. Although the point estimates suggest the presence of measurement error, they are not significant. Further, the results using longer differences (not reported) did not show any tendency for the coefficients to get farther from zero as the time period got longer. In other words, the evidence on measurement error essentially breaks down when cut at this fine a level of detail. Perhaps this is not surprising since the variation in the change in the log tax term after taking out year dummies is almost 30 times smaller than the overall variation in the tax term itself. There is just not a great deal of variation across assets but within years in the *changes* in the tax term.

Finally, in the fifth and sixth rows I use the SIC code data and divide the sample into two halves (1963 - 1975 and 1976 - 1988) in order to deal with the possibility that increasing international openness would eliminate the upward sloping supply curve in the latter half of the sample. I choose the SIC code data for this exercise because it has many more observations when split into the two samples. The data using the asset type data were similar but with larger standard errors. Interestingly, the evidence pointing to measurement error is, if anything, stronger in the later period than the earlier period. In the earlier period, there is a significant, negative coefficient in the first differenced data even without using instruments and the IV estimates do not rise much further. The later period does show evidence of measurement error. After the IV correction, the impact of taxes on prices appears to be slightly smaller but not significantly different from the one estimated in the earlier period.

V. THE IMPLICATIONS FOR THE QUANTITY OF INVESTMENT

The evidence, then, seems relatively robust that there is measurement error in the tax term and that it may be an important source of bias in conventional estimates. In this section I return to the central issue of how the existence of measurement error actually affects conventional estimates of investment demand. To do this, I use the data on the log of the net capital stock data by asset type.

Regressing a conventional investment equation with the same explanatory variables used in the price equations above, listed in here in column (1) of the top panel of table 7, again shows the apparently small effects of tax policy in the first differenced regression with a borderline significant coefficient of only -.11. This implies that a 10 percent ITC at the mean corporate tax rate in this specification raises total investment approximately 2.1 percent. Moving to the long differenced specification in column (2), the coefficient almost doubles to -.21 and is significant. The same ITC here raises investment more than 4.0 percent. Part of this larger effect after four years may, of course, reflect lags in the impact of tax policy on demand but in column (3), I use the IV methodology and estimate the true tax coefficient on the first differenced specification. The result shows a significant, negative coefficient even larger, in absolute value, than the one from the long differenced regression. In (3), a 10 percent ITC raises investment by 5.2 percent--around 2.5 times more than in the uncorrected regression.[13] This is the change in the real quantity of investment, i.e., holding prices fixed.

The results are even more pronounced excluding computers which, as mentioned before, have some rather serious outliers. These results are listed in the same columns but in the lower panel of the table. Here, the 10 percent ITC raises investment around 2 percent in the uncorrected first difference, 6.1 percent in the long differenced regression, and over 8.5 percent in the IV first differenced regression. The corrected effect is more than four times larger than the uncorrected.

The error is quite important, then, for estimates of the impact of taxes on investment, just as they were for prices. Given the evidence that taxes affect both the prices and quantities of the capital goods, a final exercise of interest is to estimate a supply curve using the tax changes as instruments for demand. Since investment subsidies might also expand the supply of capital, as well as the demand, these estimates will provide an upper bound for the supply elasticity. In column (4) of both panels, I regress the change in the log of the net capital stock on the change in the log of real price of the capital good and instrument with the lagged tax terms. Using all the assets, reported in the top panel, the elasticity is estimated at around .9 with a 95 percent confidence interval ranging from 0 to 2. Excluding computers, as in the bottom panel, the elasticity is estimated at around 1.25 with a confidence interval from .3 to 2.1. These estimates are quite close to those found in Goolsbee (1998a) using SIC code level data.

V. CONCLUSION

Measurement error in the tax cost of capital is an important issue in applied corporate taxation but has received little systematic attention. The existence of such error lies at the center of recent debates over how responsive investment is to tax policy. Using panel data on the price of capital equipment by asset type, this paper has shown that there is substantial evidence of measurement error in the tax cost of capital.

First, the coefficients on the tax term increase monotonically the longer the differencing of the data. Second, the observed coefficients all imply a similar corrected coefficient and a measurement error of the same magnitude--around 20 percent of the tax term's variance. Third, when properly instrumented, all the equations give similar results and indicate that tax subsidies have significant impacts on capital goods prices. In many ways, the results provide a textbook example of measurement error in panel data.

The paper also shows that the measurement error may reduce the impact of tax policy on investment in conventional estimates by as much as a factor of 4. Correcting for measurement error shows substantial effects of tax policy on the amount of investment. The issues raised by this paper regarding the importance of measurement error in the analysis of tax policy toward investment may motivate work on the role of measurement error for the analysis of other areas.

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TABLE 1: SUMMARY STATISTICS
(Standard Deviation in Parentheses)

Variable	Mean
$\ln(Tax_t)$	.038 (.066)
$\ln(Tax_t)-\ln(Tax_{t-1})$	.001 (.043)
$\ln(p_t)-\ln(p_{t-1})$	-.007 (.045)
$\ln(K_t)-\ln(K_{t-1})$	.043 (.055)
$\ln(GDP_t)-\ln(GDP_{t-1})$	.032 (.024)
$\ln(r_t-\delta)-\ln(r_{t-1}-\delta)$	.008 (.079)
n	572

Notes: The sample is 1963-1988 for 22 different types of equipment. *Tax* is the tax component of the cost of capital for the asset in the year. *GDP* is real GDP in the year, $(r+d)$ the interest rate for the year minus the depreciation rate for the asset. K is the net capital stock for an asset type in real dollars, p the real price of the asset type.

TABLE 2: DIFFERENCED EQUATIONS ESTIMATED WITH OLS

(Standard Errors in Parentheses)

	(1) $\ln p_t - \ln p_{t-1}$	(2) $\ln p_t - \ln p_{t-2}$	(3) $\ln p_t - \ln p_{t-3}$	(4) $\ln p_t - \ln p_{t-4}$
Δ ln (*Tax*)	-.0347	-.1351	-.2009	-.2249
	(.0412)	(.0491)	(.0051)	(.0650)
Δ ln (*GDP*)	.1353	.1980	.3437	.2994
	(.1067)	(.1096)	(.1156)	(.1225)
Δ ln ($r+d$)	.0317	.0183	.0400	.0172
	(.0187)	(.0268)	(.0279)	(.0284)
Δ Price Controls	-.0690	-.0716	-.0752	-.0778
	(.0121)	(.0110)	(.0118)	(.0135)
n	572	572	572	572
R2	.10	.10	.09	.13

Notes: The dependent variable, listed at the top of each column, is the change in the log real price of a capital asset over the stated time period. The differencing of the independent variables (denoted by Δ) is of the same length as the dependent variable in the regression. The price control variable accounts for the Nixon price controls and is described in the text. The other variables are defined in the notes to table 1.

TABLE 3: IMPLIED VARIANCE OF MEASUREMENT ERROR AND TRUE b

Equations Used	s_v^2	b
4th and 1st Difference	.00071	-.287
4th and 2nd Difference	.00107	-.335
4th and 3rd Difference	.00077	-.294
3rd and 1st Difference	.00071	-.284
3rd and 2nd Difference	.00117	-.390
2nd and 1st Difference	.00067	-.216
Mean	.00085	-.301
Std. Deviation	.00022	.058

Notes: Results are the implied values of the parameters using the pairs of coefficients from table 2 as listed in the first column and the formula for the measurement error bias as described in the text.

TABLE 4: DIFFERENCED EQUATIONS ESTIMATED WITH IV

(Standard Errors in Parentheses)

	(1a) $\ln p_t - \ln p_{t-1}$	(1b) $\ln p_t - \ln p_{t-1}$	(2) $\ln p_t - \ln p_{t-2}$	(3) $\ln p_t - \ln p_{t-3}$	(4) $\ln p_t - \ln p_{t-4}$
$\Delta \ln (Tax)$	-.2885 (.1013)	-.3645 (.1411)	-.1988 (.0738)	-.2052 (.0711)	-.2441 (.0780)
Others	GDP $r+d$ Price Contr	GDP $r+d$ Price Contr	GDP $r+d$ Price Contr	GDP $r+d$ Price Contr	GDP $r+d$ Price Contr
n	572	572	572	572	572
Lagged Yrs for Instruments	t-2 to t-5	t-4 to t-5	t-1 and t-3 to t-5	t-1 to t-2 and t-4 to t-5	t-1 to t-3 and t-5

Notes: The dependent variable for each regression is listed at the top of the column. The tax term is differenced between the same time periods as the dependent variable. Each equation is estimated using instrumental variables where the differenced tax term is instrumented with the lagged levels of the tax term listed at the bottom of the column. The other included variables are described in the note to table 2. Their coefficients are not listed to save space.

TABLE 5: TAX COEFFICIENTS BY ASSET TYPE

(Standard Errors in Parentheses)

	(1) OLS $\ln p_t - \ln p_{t-1}$	(3) IV $\ln p_t - \ln p_{t-1}$	Other Vars
No Computers n = 546	-.0207 (.0265)	-.1364 (.0620)	GDP $r+d$ Price controls
Machinery & Transp. n = 312	-.0410 (.0414)	-.1878 (.0934)	GDP $r+d$ Price controls
Other Equipment n = 234	.0023 (.0321)	-.0313 (.0705)	GDP $r+d$ Price controls
SIC Code Data n = 2210	-.0589 (.0174)	-.2876 (.0391)	GDP $r+d$ Price controls
SIC--Mach. & Trans. n = 936	-.0836 (.0211)	-.2693 (.0506)	GDP $r+d$ Price controls
SIC--Other Eq. n = 1170	-.0432 (.0199)	-.1937 (.0439)	GDP $r+d$ Price controls

Notes: Each row restricts the sample as described in the first column. The dependent variable for each regression is listed at the top of the column. Each cell represents a separate regression. Only the coefficient on the tax term is given to save space. The other included variables are listed in the final column. Regressions listed in column (1) are estimated using OLS. Regressions listed in column (2) are estimated using instrumental variables as described in the notes to table 4.

TABLE 6: TAX COEFFICIENT SENSITIVITY

(Standard Errors in Parentheses)

	(1) OLS $\ln p_t - \ln p_{t-1}$	(2) IV $\ln p_t - \ln p_{t-1}$	Other Vars
Baseline n = 572	-.0347 (.0412)	-.2885 (.1013)	GDP $r+d$ Price controls
No Controls n = 572	.0514 (.0352)	-.2517 (.1129)	None
Exchange Rate n = 572	-.0970 (.0503)	-.4671 (.1375)	GDP $r+d$ Price controls Exchange Rate
Year Dummies n = 572	-.0934 (.1131)	-.4251 (.4603)	$r+d$ Year Dummies
SIC Data: 1963 - 1975 n = 1105	-.1593 (.0297)	-.1613 (.0408)	GDP $r+d$ Price controls
SIC Data: 1976 - 1988 n = 1105	.0173 (.0214)	-.1243 (.0475)	GDP $r+d$

Notes: Each row restricts the sample or modifies the controls as described in the first column. The dependent variable for each regression is listed at the top of the column. Each cell represents a separate regression. Only the coefficient on the tax term is given to save space. The other included variables are listed in the last column. Regressions listed in column (1) are estimated using OLS. Regressions listed in column (2) are estimated using instrumental variables as described in the notes to table 4.

TABLE 7: TAXES AND THE QUANTITY OF INVESTMENT
(Standard Errors in Parentheses)

	(1) OLS $\ln K_t - \ln K_{t-1}$	(2) OLS $\ln K_t - \ln K_{t-4}$	(3) IV $\ln K_t - \ln K_{t-1}$	(4) IV $\ln K_t - \ln K_{t-1}$	Other Controls
<u>All Assets: n=572</u>					
$\Delta \ln (Tax)$	-.1129	-.2069	-.2704		GDP
	(.0634)	(.1138)	(.1215)		$r+d$
					Price controls
$\Delta \ln (P)$				.8704	
				(.4968)	
<u>No Computers: n=546</u>					
$\Delta \ln (Tax)$	-.1282	-.3165	-.4470		GDP
	(.0517)	(.0966)	(.1060)		$r+d$
					Price controls
$\Delta \ln (P)$				1.221	
				(.450)	

Notes: The dependent variable, listed at the top of each column, is the change in the log of the net real capital stock of a capital asset type over the stated time period. The differencing of the independent variables (denoted by Δ) is of the same length as the dependent variable in the regression. Each cell represents a separate regression. The other controls listed in each regression is listed in the final column. Their coefficients are not listed to save space. Regressions in columns (1) and (2) are estimated using OLS. Regressions in columns (3) and (4) are estimated using IV as described in the notes to table 4.

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[1] For an overview of some of the details in these calculations see Jorgenson and Yun (1991) or Gravelle (1994). Some of the more prominent measurement problems include the impact of expected future tax reforms (Auerbach and Hines, 1988), tax loss asymmetries and the alternative minimum tax (Lyon, 1990; Auerbach and Altschuler, 1990), inflation and the tax code (Auerbach, 1979; Feldstein and Summers, 1979; Abel, 1981) and the sheer complexity of actual tax laws (Feldstein, 1982; Ballantine, 1986). The potential inadequacy of the stylized marginal tax cost of capital has lead some to favor effective-tax-rate measures of taxes on capital income (see Feldstein, 1982 or Fullerton 1984; 1986)

[2] See Chirinko (1993) for a survey of the conventional investment literature. In their survey of the investment literature, Hassett and Hubbard (1997) argue that finding ways to deal with measurement error in fundamental variables (not just taxes) characterizes much of the recent literature on investment including Abel and Blanchard (1986), Cummins, Hassett and Hubbard (1994; 1996), Caballero (1994), Gilchrist and Himmelberg (1995), and others. Chirinko et al. (1999) question the importance of measurement error.

[3] This specification assumes that the rates of interest and depreciation are constant and thus are included in the constant term. It abstracts away from lags, and so on, for the sake of simplicity.

[4] The tax term is (1-itc-tz)/(1-t) where t is the corporate tax rate, itc is the investment tax credit, and z is the present value of depreciation allowances. More detail can be found in the data section below or in Goolsbee (1998a).

[5] Note that measurement error in the price term on the left hand side of the regression, though almost certainly present, does not affect the coefficient on the tax term but will instead go in the basic error term of the regression.

[6] See Griliches and Hausman (1986) for a discussion of the choice of instruments under various conditions on the measurement error.

[7] These conclusions rely on the assumption that the measurement error follows the same process for each of the types of capital. Any error in one particular asset that remains constant over time will not harm the results since differencing will eliminate it. If every asset has a completely different measurement error process, however, this essentially eliminates the benefit of having panel data and the Griliches and Hausman results will not apply.

[8] This result replicates Hassett and Hubbard (1998) who find little evidence of a price effect in differenced data for the U.S. As the results below will indicate, however, the bias from measurement error in this case is quite severe. The true impact on prices is substantial.

[9] This would tend to lead the price effects to fall over time. On the other hand, the existence of a lagged demand response to policy changes might lead the price impact to rise in the years following the change. Given the relatively consistent estimates of the size of the error in the results below, these considerations may not be very important in practice.

[10] Because the differenced equations include more than just the tax variable, the proper variance term in the denominator of equation (3) is the variance of the residual from a regression of the differenced tax term on the other independent variables.

[11] See Griliches and Hausman (1986) for discussion.

[12] The asset classes are engines, tractors, agricultural machinery, construction machinery, mining machinery, metalworking machinery, special industrial machinery, general industrial machinery, trucks, aircraft, ships, and railroad equipment.

[13] The estimates are smaller but not unlike the recent work on investment of Cummins, Hassett, and Hubbard (1994).