ARE INVESTMENT INCENTIVES
BLUNTED BY CHANGES IN PRICES
OF CAPITAL GOODS?

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ABSTRACT

Recent research on business investment decisions suggests that real investment in plant and

equipment is quite sensitive to changes in the user cost of capital, pointing to the possibility that

long-run changes in tax policy may have a significant impact on an economy's capital stock. Indeed,

many countries have at times adopted investment tax incentives to stimulate investment. The

prevalence of investment incentives suggests that local policymakers believe that incentives are

effective in increasing investment at a reasonable cost in terms of lost revenue for a given increment

to investment. In this paper, we explore this issue by estimating the extent to which countries are

price-takers in the world market for capital goods. We find that most countries -- even the United

States -- likely currently face a highly elastic supply of capital goods, suggesting that the effect of

investment incentives on the price of investment goods is small. Hence efforts of long-run changes

in investment tax policy are likely to materialize in real investment rather than simply being

dissipated in changes in capital-goods prices.

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I.   INTRODUCTION

Most major developed countries have attempted to stimulate domestic fixed capital

formation at some point in the past twenty years with tax incentives for investment. Repeated

adjustment of these incentives suggests that policymakers believe that they can be effective, and

that they believe that investment incentives stimulate the quantity of fixed investment and not

just the price of investment goods. Indeed, Table 1 reveals the "tax wedge" for equipment

investment (defined as one minus the rate of investment tax credit minus the corporate tax rate

times the present value of depreciation deductions, all divided by one minus the corporate tax

rate)[1] has varied significantly in recent years. In almost all countries, research on business

investment decisions suggests that real investment is quite sensitive to changes in the user cost of

capital, pointing out the possibility that long-run changes in tax policy may have a significant

impact on the economy's capital stock (see, *e.g.*, the review in Hassett and Hubbard, 1997). For

example, Cummins, Hassett, and Hubbard (1994) conclude that the elasticity of fixed investment

with respect to the user cost of capital is about 0.6, a result confirmed in Caballero, Engel, and

Haltiwanger (1995). This empirical consensus regarding long-run effects of tax policy on

investment comes at a time when both economists and policy makers in industrialized countries

are paying more attention to determinants of long-term economic growth.

While recent empirical research suggests an emerging consensus that real fixed

investment is very responsive over the long run to changes in the tax treatment of investment

(through investment tax credits, depreciation allowances, and profit taxes), an important question

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[1] This is the familiar tax wedge in the Jorgensonian user cost of capital.

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remains for policymakers: In the short run, by how much do investment incentives raise the *price* of capital goods, as opposed to the *quantity* of capital goods. If, for example, the quantity of capital goods in the economy were fixed in the short run, the increase in demand for capital goods as a result of an investment incentive would bid up the price of machines, with no effect on output in the capital-goods sector. That is, the value of investment would increase, but not the quantity of new capital goods, the objective of the investment incentive. More generally, it may be that if domestic capital-goods sellers have market power, some portion of the demand stimulus provided by the investment incentive would raise domestic prices of capital goods. Alternatively, one might argue that domestic capital-goods manufacturers face a sharply upward-sloping marginal cost schedule for producing capital goods. In this case, a tax-induced increase in demand would increase the cost -- and price -- of capital goods.

By contrast, one scenario under which tax credits might have an especially large impact on the quantity of investment without dissipation in prices of investment goods is one in which firms' demand for capital is responsive to changes in the user cost of capital and in which capital goods are supplied perfectly elastically. It is implausible that the supply function for most individual capital-goods manufacturers is highly elastic, given their high capital intensity. The effective supply of capital goods to a *given domestic market* however, might well be elastic if the world market for capital goods is perfectly competitive and open. That is, even if U.S. manufacturers of capital goods faced rising marginal costs of production, an open world market for capital goods could provide imported capital goods with smaller or no capital goods price increases.

To date, there is little evidence concerning the openness of the world market for *capital*

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goods. The empirical literature on *financial* capital mobility begins with Feldstein and Horioka (1980), who first documented that there is nearly a one-for-one correlation between changes in national saving and domestic fixed investment; many subsequent studies have demonstrated the robustness of this finding.

Even if investment funds are apparently imperfectly mobile, that does not necessarily imply that capital goods are not very mobile, however. Indeed, for most of the past thirty years, the empirical literature relating taxes to investment through the user cost of capital has assumed that firms are price takers in the market for capital goods (see, *e.g.*, Jorgenson, 1963; Hall and Jorgenson, 1967; Auerbach and Hassett, 1991; and Cummins, Hassett, and Hubbard, 1994, 1996). This assumption simplifies empirical work significantly: If the price of capital goods were set in world markets, then one might ignore the response of the price of capital goods to tax policy, and estimation of the parameters of the investment demand function is not obscured by this source of simultaneity problems. Because these studies have explored the responsiveness of real investment under the perfectly-elastic-supply assumption, an upward-sloping supply curve would imply even higher elasticities of investment demand with respect to tax changes than are reported.

The assumption that firms are price-takers in the market for capital goods has not, until recently, been considered controversial, perhaps because imports and exports of capital goods make up a large proportion of world trade. For example, as can be seen in Figure 1, in the United States, imports of capital goods accounted for around ten percent of aggregate investment in the early 1970s, climbing to above 45 percent by 1995. Over the same time period, U.S. exports of capital goods experienced a similar boom. Such flows may be the natural outcome of

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competition in world markets. Protectionism toward capital goods is much different than protectionism toward butter. Countries who use trade barriers to restrict purchases of foreign capital goods run the risk of undermining the competitiveness of their firms.

Rather than rely on such stylized facts, Goolsbee (1998) recently explored the link between investment tax policy and capital-goods prices, and found that U.S. prices for capital goods appear to respond to investment incentives. His result has support in the empirical international economics literature, where "purchasing power parity" is often rejected (see, e.g., Bordo and Choudhri, 1976; Kravis and Lipsey, 1971, 1977, 1978; and Officer, 1986).

In this paper, we shed new light on this issue by identifying and testing equilibrium conditions that must hold if the world market for capital goods is open (in the sense that protectionist measures do not limit the responsiveness of capital-goods output to tax-induced shocks to the demand for capital goods). We first explore the extent to which the price of capital goods is similar across countries. Another way in which we pursue this issue is to address specifically the extent to which local tax policy affects prices of capital goods. If all countries are small relative to the world market for capital goods, then no local policy shock will have any impact on the world price for capital goods. If capital is mobile, all countries might increase their investment in the country adopting an investment incentive. In this case, the condition that a country be "small" translates into the condition that the total investment from all sources in a given country is small relative to the world output of capital goods; increases in investment incentives in any given country that stimulate domestic investment are assumed not to, at the same time, lead to higher prices for capital goods (see the review in Hassett and Hubbard, 1997). We consider this prediction both for capital-goods prices across countries and for a sample of

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capital goods (facing different tax treatment across assets and over time) for the United States.

The paper proceeds as follows. We describe our empirical approach and specific tests in Section II. In section III, we describe the data we use for our cross-country and cross-asset (for the United States) tests in our analysis. Our empirical results are presented in section IV. In Section V, we conclude and offer observations for the broader debate about effectiveness of tax policy toward investment.

II.   TESTING TAX EFFECTS ON CAPITAL-GOODS PRICES ACROSS COUNTRIES

We begin our analysis from the perspective of the "law of one price." For goods which are tradeable, the law of one price states that there is a single price of a tradeable good no matter where that good is produced, where the prices of the good in individual countries are stated in a common currency using market exchange rates. Putting aside aggregation related index-number problems, one can extend the law of one price for a single tradeable good to a class of tradeable goods.[2] In our case, we consider capital good and their prices.[3]

A sufficient condition for the law of one price for a group of tradeable goods to be valid is that markets be competitive (in the Chamberlinian sense). If the tradeable goods (or in our case capital goods) in a market are not literally identical, for the law of one price to be valid approximately, elasticities of substitution among goods in production and consumption must be

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[2]Extending the law of one price to all goods -- as opposed to just tradeable goods -- is more difficult; one must argue further that, for any given country, the price level of tradeable goods is equal to the price level for nontradeable goods.

[3] Evidence for the law of one price at the level of aggregate tradeable goods is mixed; see, for example, Kravis and Lipsey (1978) and Officer (1986).

high.[4]

If the market for capital goods is open and perfectly competitive, and transportation costs are negligible, then all countries should pay the same price for machines. Letting q be the price of capital goods in a common currency, t be the time index and i and j refer to any two arbitrary countries, then--assuming that all countries use the same bundle of capital goods--at each time, t, the following must hold:[5]

$$q_{i,t} = q_{j,t} \qquad \forall \ i \ , \ j \tag{1}$$

If equation (1) did not hold, arbitrage would be possible. In practice, exchange rate fluctuations, adjustment costs, and delivery expenses may cause observed prices to differ across countries at times. In this case, however, a "weak law of one price" holds:

$$q_{i,t} = \alpha + \beta * q_{j,t} + e_{i,t} , \tag{2}$$

where $\beta = 1$, ϵ is a white-noise error term, and α--which may reflect transportation costs--is small and varies from country to country. The error term most likely results from errors in the measurement of prices due to unobserved compositional shocks that vary country by country. Our first test, using capital-goods prices from many countries, will be based on equation (2).

Exploiting equation (2) as a test of the law of one price requires several assumptions, however. Under the null hypothesis, the equation holds across all countries and prices, so the

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[4] Market power for capital-goods producers can lead the law of one price to fail because of, for example, price discrimination across markets or strategic pricing decisions.

[5] See also the more general description in Officer (1986).

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hypothesis can be tested by choosing a "base" country j, with price as $q_{j,t}$ and constructing a

system of equations with a separate equation for each country other than the base country. Thus,

if we have N countries, we have a system of N-1 equations relating the capital-goods price for

each country to the capital-goods price for the base country. We then can estimate equation (2)

using standard panel data techniques, wherein we can allow for country effects in the model.

Under the null, each country takes the world price as given exogenously. As we discuss below,

our results are not sensitive either to the choice of common currency or the choice of country to

use as the base country.

Second, economic theory offers no unambiguous guidance about the source of the error

term. As a consequence, for sensitivity analysis, we estimate equation (2) under a number of

alternative specifications that are consistent with equation (1). Toward this end, we estimate

equation (2) with and without taking logarithms of the variables, and with and without country

fixed effects.[6]

As we noted in the introduction, there is an alternative way to test for the validity of our

hypothesis that capital-goods prices are equivalent across countries. The price-taking assumption

provides a test of effects of investment incentives on capital-goods prices; (*i.e.*, the product of the

corporate income tax and the present value of depreciation allowances on an investment of one

currency unit.) Thus we can evaluate the orthogonality condition:

$$q_t \perp [TAX_j, E\ TAX_l], \quad \forall (\ j \leq t, \quad l > t\ ), \tag{3}$$

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[6] We also estimated our equations in first-differences; estimates were similar to those from the country fixed-effects runs, and we exclude them here.

where *TAX* is one minus the sum of the rate of investment tax credit and the value of depreciation allowances divided by one minus the tax rate, and E is the expectations operator, conditional on the information set at time t. This condition is two-sided. If firms construct their expectations of future tax variables using only data available to the econometrician at time t, and the time-series process for *TAX* follows some stationary, autoregressive process, then a test of equation (3) would be a standard Granger-causality test -- assuming, of course, that the policy variable is independent of world-business-cycle variables that might be expected to interact with the world price level. Because even current-period shocks must be of no value in predicting price movements, we perform this test below with both current and lagged tax variables. Because tax policy may be related to world economic conditions, failure to find any effect of local policy on prices would be strong evidence of open markets.

We conduct this second test in two ways. First, using a multi-country dataset, we explore whether a local investment tax wedge change in any of our countries affects the prices paid for capital goods in that country. If the short run supply curve for capital goods is upward sloping, then *TAX* should have a significant negative coefficient. For our second test, we use disaggregated data on asset-specific investment prices and tax variables for the United States to explore whether U.S. tax policy has had any effect on the extra price paid for investment. We provide this second test because a conclusion that the United States is too small to affect the world price of capital goods is especially meaningful, given the relative size of the United States, which, as we show in Figure 2, accounts for roughly 21 percent of world investment in 1992.

The capital-goods price deflators are nonstationary; given the low dimensionality of the cross-section in our sample, this nonstationarity may lead to spurious inferences. This presents

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no problem in our tests for the equality of capital-goods prices under the null hypothesis, but may be problematic when we analyze tax terms. To control for this possibility, we first-difference the capital-goods price variables in order to make them stationary before constructing our panel estimates of the responsiveness of prices to changes in local tax policy. In our final set of estimates, which focus only on U.S. data, the time period is sufficiently long enough for us to estimate time-series models, as well as to pool the data in order to allow us to address issues of nonstationarity and cointegration directly.

Finally, we do not make the claim here that we identify the supply curve for capital goods with our second set of tests; indeed it is difficult to conceive of instrumental variables that would make separate identification of the demand and supply curves possible. One would expect that the estimated tax coefficients in our test would also be zero -- irrespective of the slope the supply curve -- if demand does not respond to tax variables. However, if one accepts findings that observed *real* investment responds to tax variables, (as *e.g.*, Cummins, Hassett, and Hubbard, 1994, 1996), then our zero estimated coefficient is perhaps best explained by a highly elastic local supply curve. One might nonetheless argue that the price does not respond to investment tax credits because both the supply and demand curves shift outward when investment incentives are introduced. In any case, these identification issues highlight the important complementarity between our two sets of tests. If the evidence is strong that the law of one price holds for capital goods, then this helps us interpret the result that local tax variables do not impact on local capital-goods prices.

III. THE DATA

We use two data sets in our various tests. The first includes deflators for producers'

durable equipment and investment incentives for twelve countries: Australia, Denmark, France,

Germany, Ireland, Italy, Japan, the Netherlands, Switzerland, Sweden, the United Kingdom, and

the United States. We focus only on industrialized countries because our test is only valid under

the assumption that the deflator used is measuring a similar concept of investment in each

country. Among industrialized countries, we restrict the sample to this set of countries because

of data availability and because of our desire to have a long and balanced panel.

The capital-goods price data we use are taken from the OECD publication *National*

Income Accounts, and cover the sample period from 1971 to 1992. The price deflators for

producers' durable equipment are collected by the OECD from the individual countries. This

data collection process entails both a benefit and a cost for our study. The benefit is that the

independent calculation of the series lowers the risk that the law of one price will hold by

construction. The cost is that the exact definition of each deflator may be different, which may

invalidate our test. The latter may be especially important when comparing price deflators to

those in the United States. The United States is the only country that attempts to adjust its price

deflators hedonically for technological improvement. Because of this adjustment, the price

deflator for the United States, which includes a substantial adjustment for the evolution of

computer prices, may not align well with the others, which are closer to transaction prices. As a

check for this possibility, we will provide results both with and without the United States, but

any anomalous results for the United States should be interpreted with caution.

Price deflators are normalized for each country to equal unity in 1987. We convert

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deflators into dollar terms using data on bilateral exchange rates. We then renormalize the new common-currency price deflators to equal unity for each country in 1987, effectively imposing the law of one price in that year. The logarithmic results, which relegate these normalizations to the constant term, provide a check on whether the particular normalization we choose influences the results.

Our international tax data (presented in Table 1) cover the years from 1981 to 1992, and are taken from Cummins, Harris, and Hassett (1995). In theory, orthogonality of domestic tax parameters extends to all domestic tax variables, including domestic depreciation rules as well as investment tax credits, so we use the total tax wedge for equipment investment as our tax variable. In order to focus purely on tax effects, we construct the present value of depreciation deductions for each country other than the United States by assuming that the depreciation rate is .14 and the real discount rate is .04. For the United States, we take the tax wedge from Goolsbee (1998).

Our second data set contains information on prices and the tax treatment of 22 different types of investment assets only for the United States, for the years 1958-1989. For comparability to previous work, we use the price and tax variables also used by Goolsbee (1998).

IV. INCENTIVES AND CAPITAL-GOODS PRICES: FINDINGS

This section reports our results in three subsections. In the first we report results from the capital-goods price regressions described in section II. In the second we describe cross-country evidence on the effects of local tax shocks on capital-goods prices. In the third, we focus on results for the disaggregated data from the United States.

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A. The Law of One Price for Capital-Goods Prices

Table 2 reports the results of pooled estimation of equation (2) for the countries in our sample. The first row presents the results for all countries. The estimated coefficient on the price of capital goods in the base country (Denmark) is approximately unity, with a small standard error. The next two models reported first exclude the United States, and second focus on only the European countries in our sample. For both of these models, the estimated coefficient on the base-country capital-goods price deflator is very close to unity, again with a small standard error. The next row reports the results for the United States alone. This time the estimated coefficient is significantly less than unity indicating either that parity does not hold for the United States in fact, or that the U.S. deflator's hedonic adjustment invalidates the test for the United States. The next four models modify the first specifications by using the logarithm of, rather than the level of, the dependent and independent variables. The results are qualitatively similar to the levels specifications, with a precisely estimated coefficient on the base-country capital-goods price variable close to unity. [7]

Table 3 reports the full-sample results of estimating equation (2) with country fixed effects; for brevity we only report the results from using the variables in logarithms. We cannot reject the hypothesis that capital-goods prices in all countries are equal. Table 4 repeats the estimation using two other countries -- Italy and the Netherlands -- as the base country. We find essentially the same results, with parity holding for all countries except the United States, when

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[7] The residuals in the pooled runs excluding the United States do not reveal high serial correlation, whereas the U.S. residual does. These data are available from the authors on request.

we use the Netherlands or Italy as the base country. [8]

We cannot say whether the low estimated value of coefficient for the United States reflects the fact that the U.S. market is not "small and open," or whether it reflects the fact that the U.S. capital-goods price deflator includes a hedonic adjustment not included in the others. If the hedonic adjustment is important, then this test is not appropriate for the U.S. data. The second test we proposed in section II potentially sheds light on this issue, however. If the United States is a large player in the world market for capital goods, then investment tax policy in the United States may affect the prices paid by other countries for capital goods.

B. Local Effects of Policy: Evidence

In Table 5, we provide the results of our tests for impacts of local tax policy on the prices paid for capital goods. The dependent variable is the log difference of the capital-goods price variable. The first row reports the result when we do not control for fixed effects. The estimated coefficient on the tax variable is almost exactly zero. The subsequent rows indicate that allowing for a lagged response does not produce a significant tax effect, nor does controlling for fixed effects. Thus Table 5 does not contradict the conclusions implied by the previous three tables. There is no evidence that investment policy in individual countries has a significant impact on the price paid for capital goods in that country.

It is possible, however, that these results give a false impression. Because our approach employs a pooled, panel-data estimator, it implicitly assumes that the effect of local tax policy on

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[8] The results only change significantly when the United States is used as the base country.

the price paid for investment goods in the United States is the same as the effect of local tax

policy on the price paid for investment goods in France. It may be that the plausibly zero

coefficients in smaller countries "drown out" the impact of U.S. changes. Given the limitations

of the data used in this subsection, exploring results on a country-by-country basis is not feasible.

In the next subsection, we turn to results of the same specifications using a much larger

disaggregated U.S. asset-class data set that allows us to explore the time-series responsiveness of

the price variables to the tax variables more precisely.

C. U.S. Tax Policy and Capital-Goods Prices

In this section, we continue to investigate the impact of local tax policy on capital-goods

prices, but this time we focus only on U.S. data. Our tax variable is an asset-specific measure of

the tax variable that enters the traditional user cost calculation, one minus the ITC minus the

present value of depreciation deductions times the corporate tax rate, all divided by one minus

the corporate tax rate ("TAX" in the tables). If the supply curve for capital goods were upward-

sloping, the estimated coefficient on this variable should be negative. In this data set, the longer

time period -- 29 years -- allows us to explore the impact on each asset separately, although we

will investigate the results of pooled regressions in subsequent tables. In Table 6, we provide

summary statistics that describe the time-series properties of each series. In the first and second

columns, we report augmented Dickey-Fuller statistics for the null hypothesis that each series is

non-stationary of order one ($I(1)$). For the tests we report, the Dickey-Fuller regression included

two lags of the change in the dependent variable, along with a constant term. The five-percent

MacKinnon critical values are reported in the last row of the table. Each of the price and tax

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variables is nonstationary, with no Dickey-Fuller statistic approaching the MacKinnon critical value. (Though not reported, the results are not changed if we include a trend term in the Dickey-Fuller regression.)

Given that each of our variables is nonstationary, one would expect them to be cointegrated if the long-run supply curve for capital goods were upward-sloping. In the third column, we report the cointegrating regressions for each variable. There is no evidence that any of the error terms in the cointegrating equations are stationary.

While the evidence based on cointegration tests suggest that there is no long-run relationship between investment tax variables and capital-goods prices, it may be that prices respond in the very short run to tax policy, but that ultimately, supply response reverses the short-run supply effect. To investigate this possibility, we performed Granger-causality tests of the null hypothesis that the first-difference of the capital-goods price is not Granger-caused by the first difference of the tax variable for each of our assets, with the results reported in the fourth column of Table 6. Again, the evidence that taxes affect prices is very weak. In only one case (Agricultural Machinery) is there clear evidence of Granger-causality, about the number of rejections one would expect given the size of our tests. Thus the evidence confirms our earlier conclusion that there is no relationship between capital-goods prices and taxes, and the world market for capital goods appears to be open.

As we noted earlier, Goolsbee (1998) offers evidence for the United States that investment incentives have a statistically significant and economically important effect on prices, with prices increasing whenever investment tax credits were introduced. In Table II of his paper, he reports estimated tax coefficients for the 22 assets in our Table 6 -- using the same data we use

-- that almost always find a significant role for tax policy in explaining prices. Indeed, Goolsbee argues that an ITC of 10 percent in the United States would increase equipment goods prices by seven percent, with prices remaining higher (relative to the no-incentive case) for a very long period of time -- possibly even *forever*. It is useful to put this result in perspective. If the elasticity of U.S. real investment with respect to the user cost is -0.6, introducing an ITC of 10 percent (thereby reducing the user cost of capital by about 10 percent) would increase U.S. investment demand by about 6 percent. Because the United States accounts for about 21 percent of world investment demand, world investment demand would rise by about 0.21*0.06, or about 1.2 percent. Hence, according to Goolsbee's estimate, an increase in world demand for capital goods of about 1.2 percent increases the price of capital goods by seven percent, perhaps forever. If capacity utilization around the world were about equal to that in the United States (0.81 in 1997), this would represent an increase in world demand about equal to six percent of *unutilized* world productive capacity.

Fortunately, we can explain why Goolsbee reaches such a different conclusion from that which we reach here. Our Tables 7 and 8 help identify the causes of the disagreement. In the first column of Table 7, we report the results of pooling each of the assets together (ignoring for a moment issues of nonstationarity) and regressing the level of price on the level of the tax variable (as well as on a dummy variable for the Nixon price controls, a time trend, and the level of real GDP).[9] Following Goolsbee, we also employ in these two tables, a correction for

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[9]Goolsbee also included exchange rates in the equation, data which he also kindly supplied to us. We found that including these variables did not alter the results substantively, but did complicate the issues of nonstationarity. We opted to exclude them in the interest of brevity. Regressions including the exchange rate data are available upon request.

second-order serial correlation. Here, as in Goolsbee's Table II, the estimated tax coefficient is negative and statistically significantly different from zero, indicating that prices of capital goods rise when tax policy favors business fixed investment.

Because in this dataset the cross-sectional dimension is approximately equal to the time-series dimension, the pooled regression may reflect well-known spurious regression problems. There are two signals that this might be the case. First, there is significant residual serial correlation, indicating that the single second-order correction applied to each series does not allow adequately for heterogeneity. Second, the estimated coefficients do not tell a consistent story. For example, the estimated coefficient on GDP, the only variable Goolsbee chose to include in log-differenced form, is negative. Because expenditures on capital goods are cyclical, one would expect that capital-goods prices would rise when GDP rises if the supply curve for capital were upward-sloping (because the increase in GDP signals an increase in the demand for capital goods).[10] The second column of Table 7 repeats this exercise, this time transforming the variables so that they are stationary prior to pooling the observations. In this case, the estimated coefficient on the tax variable is zero, and the residuals are much closer to white noise. This serial correlation is so decisive in the second test because the tax and capital-goods price variables are not cointegrated, while the cross-country capital-goods price series appear to be (although the short sample periods should make one wary of statements about cointegration.)

Because the results in Table 7 revealed significant heterogeneity across assets, we repeat the comparison runs, asset by asset in Table 8. The first column shows that the estimated tax

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[10]Goolsbee also reports a negative estimated coefficient for GDP growth.

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coefficient is often negative and statistically significant in level specifications, but that the Durbin-Watson statistics are often fairly close to zero. In differences, the Durbin-Watson statistics are close to two, and the estimated tax coefficients are nowhere negative and statistically significant. Column 3 repeats Goolsbee's specification in levels, this time allowing the $AR(2)$ correction to vary across assets. The estimated tax coefficient now looks quite similar to those from the first-differenced specifications. For completeness, the final column of Table 8 reports the results for the first-differenced specification with the $AR(2)$ correction. Again, the estimated tax coefficients are always zero.

While we can reproduce the Goolsbee results with his data and specification, we conclude that one can only find a "statistically significant" coefficient on the tax variable if one accounts inadequately for the high level of serial correlation in the variables and the absence of cointegration between taxes and prices. We put significance in quotations because the regressions' nonstationary errors make traditional critical values inapplicable. We do not believe that the asset-class data for the United States support the notion that investment incentives generate large short-run price increases for capital goods. The case is not yet closed; additional empirical work in this area should use more disaggregated data (*e.g.*, studying the response of stock prices of capital-goods-supplying firms to the announcement of investment incentives).

V.   CONCLUSION AND IMPLICATIONS

The results from our three sets of tests, employing different data sets, tell a consistent story. The price paid for capital goods in any one country in the world is approximately equal to the (common-currency) price paid in every other country, and this conclusion holds even if an

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individual country has adopted tax policy to encourage investment locally. Moreover, this result

holds even for the United States. This suggests that the effects of investment tax policy have not

been muted by sharply upward-sloping supply schedules for capital goods, and that empirical

work that assumes that each country is a price-taker in the world market for capital goods is

relying on an assumption that is consistent with the broad patterns in the movements of

international capital-goods prices. Given other evidence of strong demand responses to tax

policy reviewed earlier, we conclude that investment incentives are likely to have potent short-

run and long-run effects on fixed capital formation. Moreover, for the data we examine, this

result holds even for the United States. We should note however, recent investment research

points out pitfalls in basing conclusions on evidence from time-series regressions such as those

reported here. Endogeneity of policy and other problems create empirical difficulties, again

making desirable empirical analysis of more disaggregated data.

Care must be taken in making inferences about tax policy based on our results. For

example, while our results are consistent with the idea we have expressed many times[11] that long-

run changes in tax policy toward investment can be expected to have large and predictable effects

on investment,[12] we do not mean to imply that temporary or uncertain incentives are desirable

from a policy perspective. In particular, the fact that explicitly temporary incentives can have a

large short-run impact on investment does not make them desirable policies *per se*. In the

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[11] See Cummins, Hassett, and Hubbard (1994, 1996) and Hassett and Hubbard (1997).

[12] More generally, we see no reason that other policy sources of investment stimulus would have
large effects on prices of capital goods. For example, Cohen, Hassett, and Hubbard (1998) argue that
even modest reductions in the rate of inflation in countries with tax systems distorted by inflation
can provide a potent long-run investment stimulus.

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presence of uncertainty and costs of adjusting the capital stock, we doubt that policymakers can time investment incentives sufficiently well to carry out effective stabilization policy. In addition, the use of temporary incentives magnifies uncertainty in business capital budgeting, making it more difficult for companies to forecast the path of the user cost of capital.

Finally, our evidence does not necessarily imply that tax policy *should* be used to stimulate business investment. While it is instructive to ask how effective investment incentives are at increasing the fixed capital stock, another important question remains: What is the social value of the increase in the fixed capital stock? It is well known that competitive economies do not necessarily converge to the "correct" capital stock. Our reading of the literature applying "golden rule" models and more general models of dynamic efficiency is that both capital stock and cash flow data for the United States suggest that, by increasing the stock of equipment capital, investment incentives have positive social returns (see the review in Hassett and Hubbard, 1997).[13] However, because existing analytical models for this purpose are derived for a closed economy, it is difficult to extend the comparison to domestic versus foreign fixed capital in much more open economies. We view this as an important topic for future research.

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[13] To the extent that equipment investment generates externalities in the growth process (as in DeLong and Summers, 1991) this point holds *a fortiori*. Auerbach, Hassett, and Oliner (1994) dispute the DeLong and Summers evidence on externalities.

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TABLE 1
Tax Wedge for Equipment Investment, 1981-1992
(Sample of Industrialized Countries)

	1981	1982	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992
Australia	0.856	0.856	0.856	0.856	0.856	1.214	1.214	1.142	1.142	1.142	1.142	1.142
Canada	1.072	1.072	1.066	1.060	1.072	1.072	1.099	1.093	1.143	1.143	1.143	1.143
Denmark	1.148	1.148	1.148	1.148	1.222	1.222	1.222	1.222	1.222	1.148	1.136	1.136
France	1.022	0.922	1.222	1.222	1.222	1.182	1.182	1.161	1.142	1.131	1.114	1.114
Germany	1.283	1.283	1.283	1.283	1.283	1.283	1.283	1.283	1.283	1.222	1.240	1.240
Ireland	1.182	1.222	1.222	1.222	1.222	1.222	1.222	1.197	1.168	1.168	1.148	1.148
Italy	1.127	1.156	1.156	1.192	1.192	1.192	1.192	1.192	1.192	1.192	1.203	1.274
Japan	1.161	1.161	1.161	1.170	1.170	1.170	1.161	1.161	1.148	1.133	1.139	1.139
Netherlands	0.974	0.974	0.974	0.948	0.948	0.945	0.945	1.161	1.120	1.120	1.120	1.120
Sweden	1.069	1.069	1.069	1.241	1.241	1.241	1.241	1.241	1.241	1.148	1.095	1.095
Switzerland	1.024	1.024	1.024	1.024	1.024	1.024	1.024	1.024	1.024	1.024	1.024	1.024
UK	1.241	1.241	1.241	1.182	1.148	1.120	1.120	1.120	1.120	1.120	1.109	1.109
USA	0.942	0.941	0.960	0.965	0.955	1.083	1.097	1.076	1.076	1.076	1.076	1.076

Source: Cummins, Harris, and Hassett (1995, Tables 7.1,7.2), and Goolsbee (1998) (for the last row).

TABLE 2

Test of Equality of Capital-Goods Prices
(Cross-National Panel Data, Levels)

Dependent Variable: Country-Specific Capital-Goods Price Deflator

No Country Fixed Effects

Sample	Constant	q(Denmark)	No. Obs.	R^2
All	0.009	0.990	264	0.893
	(0.015)	(0.008)		
All except U.S.	-0.019	1.028	242	0.920
	(0.014)	(0.009)		
Europe	0.013	1.029	198	0.96
	(0.009)	(0.012)		
U.S.	.313	.680	22	0.65
	(.080)	(.091)		
All[1]	0.012	1.025	264	0.83
	(0.011)	(0.036)		
All except U.S.[1]	0.012	1.055	242	0.85
	(0.010)	(0.037)		
Europe[1]	0.028	0.987	198	0.96
	(0.006)	(0.013)		
U.S.[1]	0.011	0.692	22	0.75
	(0.035)	(0.060)		

Note: Heteroskedasticity consistent standard errors are in parentheses.

[1] Variables are in natural logarithms.

TABLE 3
Tests of Equality of Capital Goods Prices
(Cross-National Panel Data, First Differences)

Country Fixed Effects
(all variables are in logs)

Sample	q(base country)	No. Obs.	R^2
All	1.025	264	0.91
	(0.023)		
All except U.S.	1.025	264	0.93
	(0.023)		
Europe	0.987	198	0.97
	(0.011)		

Note: Heteroskedasticity consistent standard errors are in parentheses.

TABLE 4
Test of Equality of Capital-Goods Prices
(Cross-National Panel Data, Other Base Countries)
(all variables in logs)

Sample	Base Country	Constant	q(base country)	No. Obs.	R^2
All	Netherlands	-0.006	1.040	264	0.836
		(0.010)	(0.037)		
All except U.S.	Netherlands	-0.007	1.070	242	0.85
		(0.010)	(0.038)		
Europe	Netherlands	0.011	1.002	198	0.965
		(0.006)	(0.013)		
U.S.	Netherlands	0.002	0.711	22	0.77
		(0.035)	(0.056)		
All	Italy	0.008	1.012	264	0.838
		(0.010)	(0.034)		
All except U.S.	Italy	0.007	1.041	242	0.850
Europe	Italy	0.022	0.972	198	0.960
		(0.006)	(0.014)		
U.S.	Italy	0.016	0.703	22	0.801

Note: Heteroskedasticity consistent standard errors are in parentheses.

TABLE 5

Investment Incentives and the Prices of Capital Goods
(Cross-National Panel Data)

Dependent Variable: Country-Specific Log Difference of the Capital-Goods Price Deflator

Fixed Effects?	Constant	Trend	Tax	Tax lagged	No. Obs.	R^2
no	-21.95 (3.738)	0.011 (0.002)	0.00006 (0.0002)		144	0.116
no	-14.761 (4.138)	0.007 (0.002)	-.095 (0.117)	-0.091 (-0.116)	132	0.056
yes	--	0.011 (0.002)	-.004 (0.053)	--	144	0.130
yes	--	0.008 (0.002)	-.110 (0.120)	0.115 (0.129)	132	0.084

Notes: Heteroskedasticity standard errors are in parentheses.

TABLE 6			
U.S. Data on Capital-Goods Prices and Investment Incentives, Descriptive Statistics			
	Dickey-Fuller Statistics		*Dickey-Fuller Statistics*	*F statistic (p-value): Price not Granger caused by Tax*

	Price	Tax	Cointegrated?		
Furniture	-2.16	-1.70	-2.03	0.79	(0.46)
Fabricated Metals	-2.45	-1.53	-2.46	0.14	(0.87)
Engines	-1.36	-1.66	-2.23	0.62	(0.55)
Tractors	-0.96	-1.52	-0.90	2.29	(0.13)
Agricultural Machinery	-1.71	-1.70	-1.89	2.85**	(0.01)
Construction Machinery	-0.80	-1.68	-1.22	0.88	(0.43)
Mining Machinery	-1.16	-1.69	-1.14	0.52	(0.60)
Metalwork Machinery	-1.57	-1.72	-2.27	2.43	(0.11)
Special Industrial Machinery	-0.44	-1.75	-1.22	2.41	(0.11)
General Industrial Machinery	-1.31	-1.70	-2.34	1.77	(0.19)
Office and Computer	-0.20	-1.76	0.38	0.33	(0.72)
Service Industry Machinery	-1.19	-1.74	-1.26	0.26	(0.78)
Electrical Dist.	-2.25	-1.58	-2.18	0.11	(0.89)
Communication	-0.44	-1.58	-1.06	1.16	(0.33)
Electrical Equipment	-1.94	-1.58	-2.01	1.07	(0.36)
Trucks and Buses	-2.29	-1.69	-2.09	1.82	(0.18)
Autos	-2.02	-1.59	-0.95	0.62	(0.55)
Aircraft	-2.33	-1.43	-2.46	0.64	(0.54)
Ships	-1.47	-1.71	-1.59	0.84	(0.44)
Railroad Equipment	-1.56	-1.65	-1.91	0.38	(0.69)
Instruments	0.31	-1.63	-0.53	0.36	(0.70)
Other Equipment	-1.02	-1.76	-1.68	0.29	(0.75)
Note: MacKinnon 5% critical value	-2.97	-2.97	-3.58		

Source: Authors' calculations.

	Dependent Variable	
Independent Variable	Price	Change in Price
Constant	0.800 (0.014)	-0.013 (0.002)
TAX	-0.190 (0.013)	---
Δ TAX	---	-0.001 (0.008)
GDP	-0.001 (0.0004)	
Δ GDP		-0.003 (0.0002)
Nixon dummy	-0.066 (0.005)	-0.004 (0.002)
Time trend	-0.010 (0.0003)	0.001 (0.0001)
Number of observations	616	594

TABLE 7
Investment Incentives and the Prices of Capital Goods, U.S. Assets

Note: Standard errors are in parentheses.

TABLE 8

Investment Incentives and the Prices of Capital Goods, U.S. Asset Values
(Asset by Asset)

	Levels Estimated Coefficient (standard error) *Durbin-Watson statistic*	**Differences** Estimated Coefficient (standard error) *Durbin-Watson statistic*	**Levels + AR(2)** Estimated Coefficient (standard error)	**Differences + AR(2)** Estimated Coefficient (standard error)
Furniture	-0.05 (0.09) 0.95	0.18 (0.10) 2.48	-0.01 (0.20)	0.18 (0.11)
Fabricated Metals	-0.37 (0.11) 0.64	-0.03 (0.12) 1.62	-0.29 (0.11)	-0.002 (0.10)
Engines	-0.48 (0.19) 0.87	-0.02 (0.14) 1.38	-0.21 (0.23)	-0.15 (0.17)
Tractors	-0.35 (0.12) 1.21	0.19 (0.19) 2.38	-0.25 (0.17)	0.29 (0.19)
Agricultural Machinery	-0.42 (0.11) 1.38	0.11 (0.13) 2.19	-0.49 (0.12)	0.20 (0.12)
Construction Machinery	-0.33 (0.10) 0.95	0.08 (0.09) 1.25	-0.01 (0.14)	0.10 (0.10)
Mining Machinery	-0.82 (0.19) 0.70	-0.20 (0.16) 1.23	-0.29 (0.51)	-0.22 (0.17)
Metalwork Machinery	-0.22 (0.09) 0.99	0.12 (0.07) 1.55	-0.004 (0.08)	0.17 (0.11)
Special Industrial Machinery	-0.08 (0.07) 0.87	0.16 (0.09) 1.64	0.07 (0.08)	0.16 (0.12)

TABLE 8

(continued)

General Industrial Machinery	-0.19 (0.08) 0.79	0.04 (0.08) 0.89	-0.05 (0.10)	0.04 (0.07)
Office and Computer	-1.31 (1.42) 0.23	-0.04 (0.44) 2.02	0.14 (0.41)	-0.17 (0.49)
Service Industry Machinery	-0.03 (0.05) 1.19	0.07 (0.07) 1.98	-0.06 (0.05)	0.08 (0.09)
Electrical Dist.	-0.07 (0.08) 0.58	-0.06 (0.08) 1.21	-0.13 (0.06)	-0.08 (0.09)
Communication	0.21 (0.10) 0.75	-0.05 (0.09) 0.93	0.002 (0.10)	-0.02 (0.09)
Electrical Equipment	-0.38 (0.15) 0.65	0.09 (0.11) 2.10	-0.21 (0.14)	0.09 (0.14)
Trucks and Buses	0.08 (0.22) 0.41	0.08 (0.17) 2.13	-0.14 (0.15)	0.06 (0.19)
Automobiles	0.62 (0.11) 1.08	0.33 (0.17) 2.21	0.31 (0.17)	0.40 (0.19)
Aircraft	-0.38 (0.10) 0.92	-0.03 (0.12) 2.13	-0.15 (0.15)	0.06 (0.12)
Ships	-0.23 (0.11) 0.76	0.02 (0.10) 2.53	-0.05 (0.10)	0.08 (0.10)
Railroad Equipment	-0.55 (0.20) 0.43	0.05 (0.12) 1.32	-0.01 (0.10)	0.04 (0.11)

TABLE 8

(continued)

Instruments	0.02 (0.14) 0.24	0.11 (0.07) 1.23	0.15 (0.06)	0.14 (0.08)
Other Equipment	-0.07 (0.07) 0.92	0.12 (0.09) 1.82	-0.002 (0.07)	0.16 (0.11)

Note: Standard errors are in parentheses.