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ARE APPARENT PRODUCTIVE SPILLOVERS A FIGMENT OF SPECIFICATION ERROR?

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Abstract

Using data on gross output for two-digit manufacturing industries, we find that an increase in the output of one manufacturing sector has little or no significant effect on the productivity of other sectors. Using value-added data, however, we confirm the results of previous studies which find that output spillovers instead appear large. We provide an explanation for these differences, showing why, with imperfect competition and increasing returns, the use of value-added data leads to a spurious finding of large apparent external effects.

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Does an increase in the output of one manufacturing sector significantly increase the productivity of other sectors? Caballero and Lyons (1990b, 1992) develop a simple and ingenious theoretical framework for detecting such externalities. Using industry data on value added as their measure of output, they establish the stylized fact that there appear to be strong productive spillovers from aggregate activity. In this paper, we provide an explanation for this stylized fact: with imperfect competition, value-added data predictably lead one to find large apparent externalities even when such externalities do not exist.

The Caballero-Lyons findings are commonly interpreted as evidence for true productive spillovers at the plant level. An increase in the activity of the automobile industry somehow increases the productivity (that is, shifts the production function) of, say, a chemical factory. This interpretation poses a challenge to economic theorists to articulate a well-specified model showing the mechanism through which this spillover operates. The idea of "thick markets externalities" is sometimes proposed, in which search costs are lower when activity is higher. It is difficult, however, to identify many examples of such thick-markets effects that affect specific manufacturing plants. Hence, the magnitudes involved seem too small to explain large spillovers. If the Caballero and Lyons findings reflect true productive spillovers, then we still await a compelling theoretical explanation of the mechanism.

Another feature of the value-added estimates is their uniform implication of strongly diminishing returns to scale at the level of the individual producer. This finding has not received as much attention as the finding of large external effects. This is a mistake. We argue that there are a number of reasons for rejecting the very low returns to scale estimated from value-added data. Most importantly, as we show, returns to scale as low as those estimated from value added imply that firms consistently sell output below marginal cost. This is not a sensible result, and it naturally casts doubt on the other result of value-added estimates, the finding of large external effects.

By contrast, our alternative interpretation of the Caballero-Lyons stylized fact suggests that there is no need for a theoretical model of spillovers and that firms do not in fact sell output below cost. We apply the same estimation methods to data on gross output in two-digit manufacturing industries, and find little evidence of productive spillovers to output but strong evidence that internal returns to scale are approximately constant. We then construct a measure of value-added output from our data, and like Caballero and Lyons, find large apparent spillovers and diminishing returns. We provide an explanation for these differences, showing why regressions with value-added data are misspecified and what form this misspecification should take. After correcting for these misspecifications, we again find, even with value-added data, that external effects are small or nonexistent and returns to scale are about constant.

The argument for why value-added data leads to bias is straightforward. Note first that real value added is not a natural measure of output, particularly for a firm- or industry-level study of production. Firms instead produce and sell gross output. Value added is an economic index number without physical interpretation. As an index number, value added is not atheoretic: its construction implicitly assumes competition and constant returns to scale.

Intuitively, real value added is like a partial Solow residual, constructed by taking gross output and subtracting the productive contribution of intermediate inputs. To do this, one must know or infer the marginal product of these intermediate inputs. Real value added is constructed assuming that this marginal product is observable from factor payments to intermediate goods. With imperfect competition, however, the marginal product exceeds the factor payments. Because of this, value added does not completely account for the productive contribution of intermediate inputs; thus, a value-added function is shifted by intermediate input use. Proxies for output externalities, such as aggregate inputs or aggregate output, are also reasonable proxies for the effect of the omitted intermediate inputs. Hence, with increasing returns and/or imperfect competition, value-added data can lead to a spurious finding of large apparent externalities. Given the positive correlation of aggregate and individual inputs, an incorrect finding of large externalities also biases down the estimate of internal returns to scale.

Our finding that there are no output spillovers suggests a sharp dichotomy between the mechanisms that might be responsible for long-run technological progress and those that are plausible sources of procyclical productivity or business cycle fluctuations. Over the long run, it is quite reasonable to think that R&D spillovers across industries, the transmission of technologies via trade, and the

development of new types of intermediate inputs play a crucial role in growth. There are a number of detailed, plausible models of these types of spillovers and their roles in long-run growth.[1] The assumption of R&D spillovers receives empirical support from the results of Jaffe (1986) and Griliches and Lichtenberg (1984). Indeed, one of the most interesting results of Bartelsman, Caballero, and Lyons (1993) is their finding that long run productivity growth in an industry is associated with increased activity in its upstream, input-producing, industries.

On the other hand, the literature explaining business cycles as a consequence of technological externalities has made relatively little progress since the seminal contribution of Diamond (1982). Hence the results of Caballero and Lyons have drawn a great deal of interest, because they seem to imply that large, high-frequency technological externalities exist regardless of their precise source.[2] However these results have also been the focus of much skepticism, since, as we noted before, it is difficult to see how spillovers of the size their estimates imply might work at business cycle frequencies. Our results indicate that the skepticism was justified: output spillovers do not in fact work at these frequencies. Thus, our results suggest that business cycle models based on "strategic complementarities" or multiple equilibria are, like the aggregate demand-driven models of Weitzman (1982) and Kiyotaki (1988), more plausibly based on pecuniary externalities than on short-run technological spillovers.

The paper is structured in five sections. In the first section we describe our method for detecting externalities. In Section II, we examine the biases that result from using value-added data. In Section III, we discuss the data we use. We present and discuss our empirical results in Section IV. We then conclude with a brief summary.

I. Method for Finding Externalities

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[1] See, for example, Romer (1987), Grossman and Helpman (1990), Ciccone and Matsuyama (1993).

[2] Based on the Caballero-Lyons findings, a number of papers have incorporated output externalities of this type into theoretical or empirical work. See, for example, Baxter and King (1990), and Braun and Evans (1991).

The theory behind our investigation follows closely the setup of Caballero and Lyons (1990a, 1990b, 1992). The discussion here parallels that found in their papers.

We begin with the following production function for an industry:

$$Y_t = F(K_t, L_t, M_t, Z_t, T_t). \tag{1}$$

Y is gross output (not value added). K and L are primary inputs of capital and labor, while M is intermediate inputs of energy and materials. Z is an externality index, and T is an index of the state of technology. We assume that the production function is homogeneous of degree γ in capital, labor, and intermediate goods. γ is thus the degree of internal returns to scale.

We allow firms to have some degree of monopoly power in the goods markets, though we assume that they are price takers in factor markets. We assume also that firms act as if they face a sequence of one-period static problems; this abstracts, for example, from any considerations of dynamic monopoly or investment behavior. Under these assumptions, the first-order conditions for profit maximization imply that the elasticity of output with respect to any factor J equals a markup μ multiplied by the share of that input in total revenue, s_J:

$$\left[\frac{F_J J}{Y}\right] = \mu\left[\frac{P_J J}{PY}\right] = \mu s_J, \qquad J = K, L, M. \tag{2}$$

P is the price of gross output, F_J is the marginal product of input J, and P_J is the price of the Jth input as

perceived by the firm. Note that the price of capital, P_K, must be defined as the rental cost of capital.[3] It does not include possible profits, which generally are also payments to capital. With perfect competition, where $\mu = 1$, equation (2) states that the elasticity of output with respect to any input equals the input's share in revenue. Under imperfect competition, the elasticity of output exceeds the revenue share.

By definition, the sum of the output elasticities equals the degree of returns to scale γ. Combining this definition with the first-order conditions (2) implies that

$$\gamma = \mu\left[\frac{P_K K + P_L L + P_M M}{PY}\right]. \tag{3}$$

Rearranging equation (3), we find

$$\frac{\mu}{\gamma} = \frac{Revenue}{Costs} = 1 + \frac{Profits}{Costs}. \tag{4}$$

Thus, if the markup exceeds the degree of returns to scale, the firm or industry makes positive profits. If the markup equals the degree of returns to scale, the firm makes zero profits.

Note that unless the profit rate is constant, then it cannot be the case that both μ and γ are constant structural parameters. We assume that returns to scale γ is the structural parameter, and implicitly allow

[3] This procedure assumes that capital services are always rented in the quantity desired. If, however, capital is quasi-fixed (sunk in the short run) then the marginal product of capital does not equal its rental rate but rather its shadow rental. That is, the user cost of capital in the Hall-Jorgenson formula (equation (13)) should be multiplied not by the price of investment goods, which is the usual procedure, but by the shadow value of capital, marginal q. (The shadow rental also includes expected capital gains.) This problem is not significant, for two reasons. First, as argued, quasi-fixity of capital affects only the period-by-period computation of the input shares, not the growth rate of capital (or any other input). Since these shares are constant to a first-order Taylor approximation, any errors caused by failure to track the movements of the shares is likely to be small. Second, the mismeasurement of the rental rate of capital has its strongest effect on capital's share. But since the growth rate of capital is almost uncorrelated with the business cycle, errors in measuring capital's share are unlikely to cause significant biases in a study of cyclical productivity. Caballero and Lyons (1990) present simulations indicating that maximum biases from quasi-fixity are likely to be on the order of 3% of the estimated coefficients.

the markup to vary over time and over industries, as required by equation (3). Define c_{Ji} to be the share of input J in the total cost of industry i. From equations (2) and (3), the output elasticity with respect to input J is $\mu s_{Ji} = \gamma c_{Ji}$. Taking the logarithmic total differential of Y and substituting for the output elasticities, we find:

$$\begin{aligned} dy_i &= \gamma \cdot [c_{Ki} dk_i + c_{Li} dl_i + c_{Mi} dm_i] + dz_i + dt_i \\ &= \gamma \cdot dx_i + dz_i + dt_i . \end{aligned} \tag{5}$$

Lower-case letters represent logs of their upper-case counterparts, so all of the quantity variables in (5) are log differences, or growth rates. For convenience, we omit subscripts on the output elasticities, and we normalize the elasticities of output with respect to external effects Z and technology T to equal 1. dx_i is a cost-weighted sum of the growth rates of the various inputs. Intuitively, equation (5) says that the growth rate of output equals the growth of inputs multiplied by the degree of returns to scale, plus the contribution of productivity shocks. If there are constant returns and perfect competition--so the cost shares are the revenue shares--then equation (5) is just the standard equation defining the industry-level Solow residual.

If we use equation (5) to estimate returns to scale, it is necessary to model the externality term dz_i. Caballero and Lyons model dz_i by arguing that there are positive spillovers at any level of production that become internal at a higher level of aggregation. For example, if a firm's productivity is enhanced by the output of other firms within the industry, this effect is internal to the industry. But the productivity of a firm may also be enhanced by increases in output of other industries. These effects are external at the industry level.

Following their method, we investigate the importance of externalities across two-digit manufacturing industries. We then look for external effects in these industries from the one-digit level, aggregate manufacturing. Caballero and Lyons (1990a) and Bartelsman, Caballero, and Lyons (1993) advocate estimating the following regression:

$$dy_i = \gamma \cdot dx_i + \kappa \cdot dx + du_i . \tag{6}$$

dx is the aggregate input growth rate, where the growth rates of the inputs are appropriately weighted by their cost shares. That is, dx is the analogue, at the aggregate manufacturing level, of the sectoral input growth rates dx_i. This serves as a natural proxy for aggregate manufacturing output, dy. It mitigates the potential endogeneity problem, however, that arises from the direct influence of sectoral technology shocks on aggregate output.

Of course, there is still an endogeneity problem, coming from the probable correlation between sectoral technology shocks and sectoral input use: input use probably rises to take advantage of the higher level of productivity. So if one has suitable instruments, the correct procedure is to project the right-hand-side variables on instruments that can be argued to be exogenous with respect to shocks to technology.

II. The Biases from Using Value Added Data

The proper measure of output for estimating equations such as (6) is gross output. From the perspective of a firm or an industry, all inputs are symmetric: the firm faces the possibility of substituting among <u>all</u> factors of production--capital, labor, and intermediate inputs of energy and materials--to produce this gross output. Researchers such as Hall and Caballero and Lyons instead use data on value added in order to estimate markups, returns to scale, and external effects. Measures of real value added attempt in some way to subtract from gross output the productive contribution of intermediate goods. This generates a measure of "net output" which, it is hoped, depends only on primary inputs of capital and labor.

Unfortunately, attempts to estimate structural parameters from data on value added are in general misspecified. In this section, we explore the statistical properties of real value added when perfect competition and constant returns to scale do not hold. We show that in the presence of markups, intermediate inputs <u>directly</u> shift a value-added function. Moreover, even controlling for this omitted effect of intermediate inputs, estimates of the degree of returns to scale from value added in general provide biased estimates of the true gross-output returns to scale. Of particular relevance here, the misspecifications caused by using value-added data may show up as external effects. Hence, with value-added data, one cannot distinguish an apparent finding of externalities from a rejection of constant returns and competition.[4]

The preferred way to measure output net of materials use is to create a Divisia index.[5] The National Income and Product Accounts instead uses the method of double-deflation; occasionally, one also sees the method of single deflation. In Appendix I, we discuss the relationship between these measures and the Divisia-index measure; we show that single- and double-deflated measures of value added are subject to the same biases as the Divisia measure, plus an additional bias in each case.

In what follows, we omit industry subscripts for simplicity. Let s_M be the share of materials in revenue. The growth rate of the Divisia index of industry real-value added is then defined as

$$\frac{dV}{V} = dv = \frac{[dy - s_M dm]}{(1-s_M)}. \qquad (7)$$

The justification for this definition is easily seen if there is competition and constant returns. The numerator is like a partial Solow residual, subtracting the productive contribution of intermediate-input growth to output growth. The growth rate of value added should then be normalized so that if gross output and intermediate inputs each grow by, say, one percent, then value added also grows by one percent. This is accomplished by dividing through by a "gross-up" factor, the share of nominal value added in gross output, in the denominator.

[4] The argument below differs from that emphasized in the production function literature. It is well known from that literature that a value-added production function independent of intermediate inputs exists if and only if capital, labor, and technology are separable from intermediate inputs in the gross output production function (see, for example, Bruno (1978)). Our observations hold even if the production function is separable.

[5] See Sato (1977) or Arrow (1974).

These two adjustments are only correct, however, under constant returns to scale and competition. Value added is like a partial Solow residual, and Solow residuals do not correctly measure the productive contribution of inputs if constant returns and/or competition fails. In particular, with imperfect competition the output elasticity with respect to intermediate inputs exceeds their share in revenue.

We can express analytically the misspecification that arises from failures of constant returns and competition. If we take the logarithmic total differential of gross output and substitute into equation (7), we can write value-added growth as:

$$dv = \frac{1}{(1-s_M)}\left[\left(\frac{F_K K}{Y}\right)dk + \left(\frac{F_L L}{Y}\right)dl\right] + \frac{1}{(1-s_M)}\left[\left(\frac{F_M M}{Y}\right) - s_M\right]dm + \frac{1}{(1-s_M)}[dz + dt]. \qquad (8)$$

Substituting the first-order condition for intermediate input use, and combining capital and labor inputs into a cost-weighted index dx^v, we can write this as:

$$dv = \frac{1}{(1-s_M)}\left[\frac{F_K K + F_L L}{Y}\right]\left[\left(\frac{F_K K}{F_K K + F_L L}\right)dk + \left(\frac{F_L L}{F_K K + F_L L}\right)dl\right] + (\mu-1)\left(\frac{s_M}{1-s_M}\right)dm + \frac{1}{(1-s_M)}[dz + dt], \qquad (9)$$

or,

$$dv = \gamma \cdot \left[\frac{1-c_M}{1-s_M}\right]dx^v + (\mu-1)\left(\frac{s_M}{1-s_M}\right)dm + \frac{1}{(1-s_M)}[dz + dt]. \qquad (10)$$

Equation (10) provides the basis for our discussion of what it is that value added measures. The growth rate of the index of value added depends on returns to scale, markups, and intermediate input growth, in addition to technological progress and the growth of primary inputs.

If there is competition and constant returns to scale, then the growth rate of value added equals the growth rate of primary inputs plus technological progress. That is,

$$dv = dx^v + \frac{1}{(1-s_M)}[dz + dt]. \tag{11}$$

One implication of equation (11) is that, as Hall (1990) notes, under competition and constant returns the productivity residual calculated from value added is uncorrelated with any variable that neither causes productivity shifts, nor is caused by productivity shifts. In addition, dx^v can be calculated with either cost or revenue shares, since there are no profits.

With imperfect competition and non-constant returns, however, there are two sources of misspecification apparent in equation (10). The first misspecification is that in the presence of markups, the growth of materials and energy directly shift the value-added production function. Intuitively, value added is calculated by subtracting from gross output the contribution to production of intermediate goods, assuming that the elasticity of output with respect to materials equals its revenue share. With markups, the elasticity of output with respect to materials exceeds its revenue share. Hence, some of the productive contribution of energy and materials inputs is incorrectly attributed to value added. For example, with imperfect competition an energy-price shock, which causes energy use to fall, shifts the value-added function downwards.

This omitted growth in materials will appear as an external effect, since aggregate inputs of capital and labor serve as a proxy for intermediate input use in each sector. This seems plausible a priori, since part of the output of each sector is used as intermediate inputs in other sectors. The data support this conclusion as well. Using our data set, we find that aggregate inputs serve as a good proxy for sectoral intermediate input use, even after controlling for own-inputs.[6] Hence, by including aggregate inputs, Caballero and Lyons include a proxy for sectoral intermediate input use in their regressions.

The second misspecification is that the coefficient multiplying primary input growth dx^v in

[6] We regress dm_i on dx_i^v and dx^v. The coefficient on dx^v is 0.52, with a t-statistic of 12.

equation (10) is not, in general, the true degree of returns to scale. The reason for this is that the output elasticity with respect to primary factors equals the degree of returns to scale multiplied by the cost share going to primary factors, or $\gamma\cdot(1-c_M)$, whereas the calculation of value added "grosses up" this elasticity by $(1-s_M)$. This is the correct adjustment if there are no economic profits, so that the cost shares equal the revenue shares. In this case, the coefficient on primary input growth is the true degree of returns to scale. If there are economic profits, however, this divisor is too large. Hence, once we control for the omitted-variable bias from intermediate inputs, the coefficient on primary inputs provides a downward-biased estimate of the true degree of returns to scale.

It is unclear a priori what effect this second misspecification should have on estimated externalities. If $(1-c_M)/(1-s_M)$ is constant over time, then the coefficient on primary inputs is a constant structural parameter; after controlling for the omitted variable bias, this second misspecification has no effect on the estimate of external effects. If $(1-c_M)/(1-s_M)$ is strongly procyclical, on the other hand, this will bias the estimated externality term upwards. In our data, there appears to be little cyclical pattern to $(1-c_M)/(1-s_M)$. Hence, we expect this second misspecification to have relatively little effect on the estimated externality.

In principle, we can estimate equation (10) directly. Non-linear estimation then provides estimates of internal returns to scale γ, the markup of price over marginal cost μ, and the size of any external effects. By estimating this equation, we can confirm the empirical importance of the two misspecifications we identify above.

It is inconsistent, however, to estimate this equation by constraining both the markup and the degree of returns to scale to be constant over time, as we discussed in Section I. As before, we eliminate this inconsistency by using equation (3) to solve out for μ, implicitly allowing the markup to vary over time and over industries. This gives the following equation:

$$dv = \gamma\cdot\left[\frac{1-c_M}{1-s_M}\right]dx^v + \left(\frac{\gamma c_M - s_M}{1-s_M}\right)dm + \frac{1}{(1-s_M)}[dz + dt]. \tag{12}$$

Equation (12) shows how to augment data on value added and primary inputs in order to correct for the effects of markups and increasing returns. In Section IV, we estimate this equation both with our value-added data, and with the NIPA data used by Caballero and Lyons, augmented with our data on materials use and the materials share in revenue and cost. This is of interest because the two sources of data have different methodological underpinnings. Hence, by estimating equation (12), we confirm the extent to which the misspecifications we identify explain results with their data, as well as with our own.

Note that Caballero and Lyons (1992) attempt to control for the flaws of value-added data without directly using data on materials use or the materials share. In their regressions they include as a right-hand-side variable either the quantity of energy used in the industry, or the price of oil relative to the price of industry output. We interpret the inclusion of the quantity of energy as a proxy for the omitted variable of sectoral intermediate input growth, and the inclusion of the relative price of oil as an attempt to control for the bias resulting from double-deflation (see Appendix I).[7] Of course, one wants to control for both failures of the NIPA data. To that extent, one would want to include both variables simultaneously rather than sequentially.

These are, however, quite imperfect ways to correct for the failures of value-added data. Energy use is only about five percent of a typical sector's use of intermediate goods, so including it does not correctly control for the omitted variable, which is total use of intermediate goods. Similarly, including the relative price of oil may partially proxy for the part of the double-deflation bias that depends on changes in relative prices. But again, the relevant price is the price of materials as a whole, not of oil alone. In any case, including the relative price of oil does not solve any of the problems we identify in equation (10).

We do not face the same problems as Caballero and Lyons in our estimation, because we do not use value-added data. As we discuss in the next section, we instead use data on gross output by 2-digit

[7] Caballero and Lyons's argument for including the price of oil seems to assume that their value-added data are single-deflated. We do not interpret the inclusion of the oil price this way, because their data is double-deflated, not single-deflated.

manufacturing industry. The discussion in this section provides an explanation for differences in results using gross-output data and using value-added data. In particular, it is apparent that the use of value-added data may lead to a spurious finding of external effects.

III. The Data

We use unpublished data provided by Dale Jorgenson and Barbara Fraumeni on inputs and outputs for 21 manufacturing industries of the U.S. economy, for the years 1953-1985. These data are at roughly the two-digit SIC level.[8] This data is part of Jorgenson's long-term research effort, with a number of coauthors, aimed at creating a complete set of national accounts for both inputs and outputs at the level of individual industrial sectors as well as the economy as a whole. The purpose of these accounts is to allow Jorgenson to allocate U.S. economic growth to its sources at the level of individual industries. Hence, these data seek to provide measures of output and inputs that are, to the extent possible, consistent with the economic theory of production.[9]

Inputs are separated into capital, labor, and intermediate inputs of energy and materials. Conceptually, Jorgenson divides labor input into hours worked, and average labor "quality." This quality adjustment takes account of differences in the sex, age, and educational composition of the labor force, assuming that observed wage differences reflect differences in relative marginal products. Similarly, Jorgenson constructs a quality- or composition-adjusted measure of capital input analogously to labor input, seeking to weight the input of different types of capital by their relative rental rates. We obtained the data on labor and capital input both with and without the quality adjustment. This allows us to confirm that none of our substantive results are affected by these adjustments.

[8] There are only 20 two-digit industries. In Jorgenson's data, the transport-equipment industry is divided into two sectors, "motor vehicles" and "other transport equipment."

[9] Appendix II contains an expanded description of the data. For a complete description, however, see Jorgenson, Gollop, and Fraumeni (1987) or Jorgenson (1990).

For our purposes, however, there are two crucial aspects of Jorgenson's data. First, sectoral output is measured as gross output, not value added. Second, the data include sectoral intermediate inputs in each year, compiled from the Bureau of Economic Analysis input-output tables. The input-output tables allow Jorgenson to compile complete intermediate inputs-- including energy, materials, and business services. These two aspects allow us to treat intermediate inputs symmetrically with primary inputs in modeling production. They also allow us to augment the data used by Caballero and Lyons, in order to correct the biases that arise from their use of value-added data.

The Caballero-Lyons data include measures of sectoral value added, from the National Income and Product Accounts. This is a double-deflated measure of value added, rather than the theoretically-preferable Divisia index. They also include data on sectoral labor hours, from NIPA, and data on sectoral net capital stocks, from the BEA. Neither measure of labor or capital input includes any adjustment for changes in composition, or quality, over time. (They are thus similar to Jorgenson's data without the quality adjustment, except that the capital stock incorporates different depreciation assumptions, and does not include land or inventories.)

As Hall (1986, 1988) notes, with imperfect competition we cannot assume that required payments to capital are simply a residual after all other factors are paid. To estimate the required payments to capital, we follow Hall and Jorgenson (1967), Hall (1986, 1988), and Caballero and Lyons, and compute a series for the user cost of capital r; the required payment for any type of capital is then $rP^K K$, where $P^K K$ is the current-dollar value of the stock of this type of capital. In each sector, we use data on the current value of the 50 types of capital, plus land and inventories, distinguished by the BEA in constructing the national product accounts. Hence, for each of these 52 assets, we compute the user cost of capital as

$$r_s = (\rho + \delta_s)\frac{(1 - ITC_s - \tau d_s)}{(1 - \tau)}, \quad s = 1 \ to \ 52. \qquad (13)$$

ρ is the required rate of return on capital. For each asset, δ_s is the depreciation rate.[10] ITC_s is the investment tax credit, and d_s is the present value of depreciation allowances. τ is the corporate tax rate. We assume that the required return ρ equals the dividend yield on the S&P 500. We obtained unpublished data on ITC_s, d_s, and τ from Dale Jorgenson.[11]

The derivations in Sections I and II are presented in continuous time. We approximate them in discrete time by replacing logarithmic derivatives with log differences, and using average shares in adjacent time periods in place of instantaneous shares. For example, in order to compare the results we find with Jorgenson's data more directly with the results from Caballero and Lyons' data, we calculate a series for real value added from Jorgenson's data. Letting $\bar{s}_M$ be the average value in periods t and t-1 of the share of intermediate inputs in revenue, we approximate equation (7) in discrete time as

$$\Delta Ln V_{it} = \left[\frac{1}{1-\bar{s}_M}\right]\left[\Delta Ln Y_{it} - \bar{s}_M \Delta Ln M_{it}\right]. \qquad (14)$$

This is the Tornquist approximation to the continuous-time Divisia index in equation (7).

As discussed in Section I, there is a clear argument for using instrumental variables in estimating equation (5). Good instruments are of course hard to find. We follow Hall (1988) and Ramey (1989), and use the political party of the President, the growth rate of real military spending, and the growth rate of the world dollar price of oil. We also include a lag of each of these variables. Of the three variables,

[10] The 50 types of depreciable capital, along with their estimated geometric depreciation rates, are listed in Jorgenson (1990), Table 3.6. Automobiles, for example, have a depreciation rate of 33 percent. Photocopy machines have a depreciation rate of 18 percent. Office buildings have a depreciation rate of 2.47 percent. We follow Jorgenson and assume that land and inventories do not depreciate.

[11] The typical industry appears to have an average postwar profit rate of about 5 percent in our estimates. We experimented with several alternative measures of the capital cost. For example, following Hall (1990) we assumed a constant depreciation rate of 12.7 percent for all assets. We also used data on non-asset-specific measures of ITC and d, obtained from Alan Auerbach. These adjustments had no important effect on our results. This is unsurprising, since they affect the overall estimated level of profits, but have little effect on the cyclical properties of the estimated cost-of-capital series.

the price of oil has the greatest correlation with the right-hand-side variables; it is also the most questionable. Hall (1990) argues in its favor, on the grounds that input prices do not shift production functions. It is not enough, however, that the production function at a point in time be unaffected. In estimating equation (6), we are explicitly focusing on how the production function changes over time; for this rate of change to be unaffected by input prices, we require that technological progress be Hicks-neutral. With this assumption, the price of oil is a valid instrument.

IV. Results and Discussion

In this section we present our major empirical results, and discuss their implications.

We reach three main conclusions. First, using gross-output data from 1953 to 1985, we find no evidence of spillovers. Second, using our constructed value-added data, we find large apparent spillovers. Thus, with different data, we reproduce the Caballero-Lyons stylized fact that there is a strong positive correlation between aggregate inputs and industry productivity calculated from value added. Third, by augmenting data on value added and primary inputs with data on intermediate inputs, we essentially recover the gross output results with value-added data, including the finding of no positive externalities. This is an important result because it shows that many of the failures of value added come from the source we identify: specification error caused by the existence of imperfect competition.

The results are presented in Tables 1-4. In all of the tables, we constrain internal returns γ and external spillovers κ to be the same in all 21 industries. Allowing internal returns γ to vary across industries barely affects our estimates of κ, the variable of primary interest here, so we do not report those results. In Table 1 we report the results using both the quality-adjusted and the non-quality-adjusted data. As we see, the two types of data give essentially identical results. Therefore, in the remaining tables we present only the results using the preferred quality-adjusted data. In no case are our conclusions altered by the quality adjustment.

We report all regressions both instrumented and uninstrumented. If one has complete confidence

in the instruments, then the instrumented results are clearly preferable. There are two potential shortcomings to the instruments we use, however. First, even if one has complete confidence in the exogeneity of the instruments, there may be small-sample problems. Nelson and Startz (1990) show that using exogenous instruments that fit poorly may lead to substantial small sample bias (where "small" may, in fact, be large relative to the data sets used by macroeconomists.) They consider a single-equation single-variable setup, but to us their work suggests a more general concern: we are trying to disentangle the effects of two highly correlated variables (industry and aggregate inputs) from their projections onto the same set of relatively poor instruments. Second, the only instrument with any significant correlation with inputs is the price of oil. And this is the most questionable instrument, because of the possibility that technological progress is not Hicks-neutral. Hence, for comparison, we also report the uninstrumented results.

All regressions include unreported industry-specific constants and dummy variables that allow a trend break in productivity growth after 1973. We include the post-73 dummy in response to recent work of Perron (1989) and others, who suggest that allowing for a one-time change in slope after 1973 provides a useful empirical specification for modelling trends. The post-73 dummies are generally highly significant, but none of our substantive conclusions depend on their presence.

Table 1 shows our main results. The dependent variable is the growth rate of gross output in each industry. The two columns on the left show the results when inputs are not adjusted for quality; the two columns on the right show the results when they are. The rows of the table show our estimates of the parameters in equation (10): internal returns γ, and output spillovers κ.

Focusing first on the estimate of returns to scale, we find essentially constant returns. Adjusting for quality, however, returns to scale are slightly smaller: instrumented, with quality-adjusted inputs, we estimate that γ is about 0.96. This is just smaller than 1 at the 95 percent level of significance: the confidence interval includes 0.99. Since the quality of inputs has grown over our sample period, taking this fact into account shows that some of the output growth incorrectly attributed to increasing returns is actually a consequence of increasing input quality.

Turning to κ, the parameter of primary interest here, in Table 1 we find no evidence of positive output spillovers across industries. In all cases we find that κ is essentially zero: the average κ is -0.02. In the one case where κ is positive it is insignificant, and in the one case where it is significant it is negative.

In Table 2, Panel A, we show the same regressions as in Table 1, but using our constructed value-added data as the measure of output. These results differ markedly from the gross-output results. This is particularly striking because the value-added data are constructed from the same data set that we used for the gross-output regressions. Hence, we control for the myriad small differences in sources and methods that make it difficult to compare results from different data sets.

Using these value-added data we find small estimates of γ, consistently on the order of 0.6. This accords with our prediction in Section II that the estimate of γ from a value-added regression is biased downward. For reasons we discuss below, estimates of internal returns on the order of 0.6 are too small to be believable. Turning to κ, the spillover parameter, we find that in our value-added data κ is large and significant--both t-statistics exceed 4. This contrasts with the results in Table 1, where κ was never both positive and significant.

In Table 2, Panel B, we run the same regressions with the Caballero-Lyons data, from NIPA and BEA.[12] The results are qualitatively similar to those in Panel A, although the estimates of κ with NIPA data are larger than with our value-added data. This difference may arise from the different method by which the NIPA value-added data are constructed, an issue we discuss in Appendix I.

Together, Tables 1 and 2 demonstrate empirically the pattern of results we predicted on theoretical grounds in Section II: estimates of returns to scale are biased down and estimates of external effects are biased up, relative to their true gross-output values, when the estimation is done with value-added data. We now confirm that our explanation for the failure of value added is the correct one. Equation (12)

[12] We thank Ricardo Caballero, who generously provided the data to us. These regressions do not correspond exactly to the regressions of Caballero and Lyons (1992) because we do not include the sectoral oil price or energy use as a right-hand-side variable.

specifies the exact manner in which value added should fail. This equation predicts that the major source of the difference in results is the omission of materials growth as a right-hand-side variable. If our explanation is correct, then by estimating equation (12) we should get exactly the same γ's as in Table 1 and κ's that are about twice as large.[13] (The reason that κ should be larger is given in equations (10) or (12): with value added, the effect of the externality is scaled up by a factor of $1/(1-s_M)$. In the data, s_M is on the order of 0.5-0.6 over our sample period.)

We estimated equation (12) by non-linear least squares using both our value-added data and the NIPA data. Table 3 reports our results. For our data, they conform very well to our predictions. In the case of the instrumented regression the parameter estimates are just what we expect: γ is almost identical to its Table 1 counterpart and κ is multiplied by a factor of about 2.5. The uninstrumented results are also very close.

Applying our corrections to the NIPA data, however, does not give exactly the results of Table 1. It is nevertheless clear that the failures we identify do apply to the NIPA data. The changes in the parameter estimates are very similar: γ rises significantly and κ falls significantly. In the case of the instrumented results, even with the NIPA data, κ becomes insignificantly different from zero, while γ increases from 0.8 to 1.1 (significantly increasing returns to scale). Thus, we attribute the (instrumented) results of Caballero and Lyons (1992) to the specification error we discussed in Section II. When we do not instrument the NIPA data, however, κ is reduced by a third relative to its value in Table 2, yet it remains large. Why the instrumented and uninstrumented results are so different remains a puzzle.

In Section II, we identified two separate misspecifications with value-added data. Here we ask which of these misspecifications has a greater empirical effect. In Table 4A, we correct for the first problem only: the fact that dx^v should be multiplied by $(1-c_M)/(1-s_M)$. In Table 4B we correct for the second problem only: we include the omitted materials growth term as required in equation (12). Comparing the results with those of Table 3 shows that the omission of the materials term is quantitatively

[13] We are indebted to an anonymous referee for making this point.

more important. This fits our theoretical prediction, since profit rates are relatively small, and $(1-c_M)/(1-s_M)$ is not very cyclical. This conclusion holds for both data sets.

We view our findings as methodologically significant. They show not only that gross-output data gives results quite different from results with value added; they also confirm empirically our predictions about why value added is inappropriate. But our methodological findings should not overshadow the significance of our substantive results. We find that internal returns to scale are roughly constant, but certainly not strongly decreasing as the value-added results suggest. Most interestingly, we find no evidence of positive, contemporaneous spillovers from aggregate activity.

That value-added estimates give strongly diminishing returns has not received as much attention as the finding of spillovers, but it gives yet another strong reason to be wary of these results.[14] Most plant and engineering studies find returns to scale essentially constant.. For example, Griliches and Ringstad (1971) find γ to be on the order of 1.03-1.05. Baily, Hulten, and Campbell (1992) use plant-level data and find that returns to scale are roughly constant (with more estimates slightly below 1 than above).[15] As Griliches and Ringstad observe, essentially constant returns are needed to rationalize the observation that there is often a large dispersion of establishment sizes within a given industry. Estimates of γ from value added, like the ones we report above, imply that firms should want to have a huge number of plants, each operating at a minuscule scale.

We have assumed that there are no fixed costs, but fixed costs do not suffice to resolve this paradox. With fixed costs and perfect competition, each firm will operate on average at the minimum of the average cost curve, the point where returns to scale are locally constant. With Chamberlinian

[14] Hall (1990) uses value-added data, and finds increasing returns. In a companion paper, Basu and Fernald (1993), we argue that this arises from the omitted-variable bias we identified in Section II. Since Hall's regressions do not include an aggregate activity variable, in his case the specification error biases up the estimated returns to scale.

[15] They use a sample of plants from 23 four-digit industries in the Longitudinal Research Database for the last four census years (1972, 77, 82, 87) . "If anything," they write, "there is some sign of decreasing returns, especially in the later years, but a vote for constant returns at the plant level looks like a pretty good bet (p. 235)."

monopolistic competition firms operate where there are locally increasing returns, i.e., on the downward-sloping portion of the average cost curve. Some industrial-organization models of entry deterrence predict that firms should carry excess capacity (Bulow, Geanakoplos, and Klemperer, 1985): this again predicts that firms operate at a point of locally increasing returns. We are not aware of any model with fixed costs that predicts that firms will on average produce at a point where there are diminishing returns to scale (as distinguished from increasing marginal cost).

Apart from these considerations, a γ that is significantly less than 1 is internally inconsistent with the assumed monopolistic-competition framework (or, indeed, with any form of profit maximization). Note from equation that unless (4) profit rates are huge (which in the data they are not), a γ of 0.6 or even 0.8 implies that the markup of price over marginal cost, μ, is less than 1 -- i.e. firms sell output below marginal cost. This conclusion is independent of any possible externalities and gives perhaps the strongest reason for doubting these low estimates of γ.

Our results with gross-output data, however, seem quite reasonable. In terms of economic significance we find constant returns to scale. With the profit rates we calculate, this implies a small but significant markup: prices are on the order of 5 percent above marginal cost. This calculation is, however, sensitive to an accurate computation of profit rates. The profit rates we compute are particularly low (sometimes negative) in the 1970s. Here it is possible that our required payments to capital are too high because we do not account for capital losses on existing plant and equipment following the oil price shocks.[16] As an alternative strategy, we estimate equation (10) by NLLS to get a direct estimate of the markup, μ. (Assuming that both γ and μ are constant is equivalent to assuming that the true profit rate is a constant.) We find μ to be about 1.15, implying prices about 15 percent higher than marginal costs. Hence the implied markup and profit rates from these two procedures range between 5 and 15 percent: a strong indication of imperfect competition, but small enough to be plausible.

Turning to the part of Caballero and Lyons's findings that has excited a great deal of interest in

[16] This is consistent with the observation that the stock market was depressed but investment was high following the oil price shocks. See the discussion in note 2.

the literature, we find no evidence of positive externalities from contemporaneous activity in manufacturing to the production functions of firms within a specific industry. We have presented evidence that results to the contrary are a consequence of specification error.

What accounts for the differences between our results and those of Bartelsman, Caballero, and Lyons (1993)? They, too, use gross-output data, albeit at the level of four-digit industries, and find evidence of externalities from aggregate input use of about 0.12. An earlier (1991) version of their paper indicates that most of the externalities they estimate come from other two-digit industries, not from within the same industry group. Hence, the differences between our results and theirs is not explained simply by externalities at the four-digit level that are internalized at the two-digit level.

One possible explanation is that their findings of externalities arise from shortcomings in their data, which are incomplete in several ways. First, their measure of intermediate inputs does not include business services. These services have become increasingly important over time, so their omission is potentially serious. Second, their data on output and inputs are collected from establishments, not companies. Hence, these data include only inputs and factor payments in plants actually producing output. All of the other inputs used by a firm are not counted. Crucially, these omitted inputs are those typically associated with fixed costs of operation, such as the overhead labor and capital used by an administrative headquarters or a research laboratory. Since fixed costs are one plausible source of increasing returns, omitting fixed inputs is likely to bias down their estimates of internal returns. Since aggregate activity is correlated with the average productivity of fixed inputs, the omission would show up as an apparent externality. That is, when output rises, firms have lower average costs. If this fact is not taken into account via correct measurement of the internal inputs of the firm, the reduction in average cost is perceived as an external effect.

Another possible explanation for the difference in results is that the sample periods differ. Bartelsman, Caballero, and Lyons estimate their regressions over the period 1959-86. Our data only goes up to 1985; but for comparison, we estimated our basic gross-output regressions over the period 1959-85. The uninstrumented regression continues to show no evidence of external effects. Instrumenting, however,

the estimate of κ is 0.08 with a standard error of 0.02. This is relatively close to the results of Bartelsman, Caballero, and Lyons.

A finding of contemporaneous spillovers of 0.10 or smaller, whose presence is highly sensitive to the type of data, the estimation technique, and the precise sample period, is probably not very significant economically. For this reason, we conclude that internal returns to scale are about constant, and external effects from contemporaneous aggregate activity are probably not present.

Conclusion.

In this paper, we provide an explanation for the stylized fact that regressions using value-added data in two-digit manufacturing appear to imply large productive spillovers to output. We show why, in the presence of imperfect competition, the use of value-added data will likely cause one to find large apparent externalities, even if such externalities do not in fact exist.

Using theoretically-preferable gross-output data, we find no evidence of output spillovers. When we construct value-added data, however, we find large apparent spillovers. We then augment data on value added and primary inputs with data on intermediate inputs in the way our theoretical specification requires; we again find that spillovers are non-existent. This confirms our conjecture that the misspecifications of value added we identify as a matter of theory are actually responsible for the differences in results.

The specification errors we identify have an important implication: value-added data is not appropriate for estimating structural parameters, except with competition and constant returns. Hence, our critique applies equally to the work of Hall (1986, 1988, 1990), who uses the same data as Caballero and Lyons in order to estimate markups and returns to scale. In a related paper (Basu and Fernald 1993), we argue that Hall's finding of increasing returns arises from the misspecification we identified in Section II: his estimates of returns to scale are inflated due to omitted variable bias from ignoring the effects of intermediate input growth. As we show in this paper, Hall's data suggests that returns to scale are close to constant. We do, however, find evidence of small, though significant, markups of price over marginal cost.

Our results paint a very different picture of the economy than the one suggested by previous empirical work and incorporated into recent theoretical papers. We find essentially constant returns, no short-run spillovers, and small markups of price over marginal cost. Our results suggest that business cycle models cannot plausibly be based on increasing returns or productive externalities.

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Appendix I: Single- and Double-Deflated Value Added

In Section II, we discussed the statistical properties of a Divisia index of real value added. This assumes that the weights on output and intermediate inputs are adjusted continuously. The NIPA data used by Hall, Caballero and Lyons, and others instead uses the method of double-deflation to estimate real value added: gross output and intermediate inputs are deflated separately. If we normalize the base-year prices of gross output and intermediate inputs to one, the double-deflated estimate of real value added, V_i^{DD}, is

$$V^{DD} = Y - M. \qquad \text{(A-1)}$$

Let n equal M/Y, the share of output going to materials in base-year prices. Differentiating equation (15), we find that the growth rate of double-deflated value added is:

$$dv^{DD} = \frac{1}{1-n}dy - \frac{n}{1-n}dm\ . \qquad \text{(A-2)}$$

Note that the difference between the double-deflated index (16) and the Divisia index (7) is the weights used to subtract materials growth from output growth. The double-deflated index calculates the weights using constant base-year prices, whereas the Divisia index calculates the weights using current prices. With some algebraic manipulation, we can show that the growth rate of the double-deflated index of value added equals the growth of the Divisia index, plus an additional term:

$$dv^{DD} = dv + \frac{n}{(1-n)(1-s_M)}\left(1 - \frac{P_M}{P}\right)(dy-dm). \qquad \text{(A-3)}$$

The second term in equation (17) is the double-deflation bias. As Bruno (1978) and Bruno and Sachs (1985) point out, this bias term disappears in two special cases. First, it disappears if intermediate inputs

grow at the same rate as gross output. For this to be true in general, the production technology must be Leontief between intermediate inputs and value added, so that intermediate inputs are used in fixed proportion to output. Second, it disappears if the price of intermediate inputs relative to the price of output is always equal to one. This is the case if relative prices are constant over time; it is also the case if these base-year prices are allowed to change each period, so that the double-deflated value-added index becomes essentially the Divisia, shifting-weight index.

The relative price of intermediate inputs (especially energy) is certainly not constant over time, and NIPA adjusts the base-year prices approximately every ten years. Hence, the only case in which double-deflated value added provides an unbiased measure of productivity growth is if intermediate inputs are used in fixed proportions to output. This is not only a strong assumption to impose, it is rejected by the data.[17]

An additional method sometimes used (see, for example, Bartelsman, Caballero, an Lyons 1991) to create an index of real value added is the single-deflation method: nominal value added is deflated by the gross-output deflator. That is,

$$V^{SD} = \frac{(P \cdot Y - P_M \cdot M)}{P} = Y - \left(\frac{P_M}{P}\right)M \qquad \text{(A-4)}$$

Differentiating equation (18), we find that the growth rate of the single-deflated index of value added again equals the growth of the Divisia index, plus an additional term:

$$dv^{SD} = dv + \left(\frac{s_M}{1-s_M}\right)\frac{d(P_M/P)}{(P_M/P)}. \qquad \text{(A-5)}$$

It is clear from equation (19) that single-deflated measures of value added are directly shifted by changes

[17] See, for example, Jorgenson, Gollop and Fraumeni (1987).

in the relative prices of intermediate inputs.

Hence, both single-deflated and double-deflated measures of real value added suffer from the same misspecifications as the Divisia index of value added. There is then an additional misspecification in each case.

Appendix II: Data Description

This appendix contains a brief description of the Jorgenson data set, and the underlying source data. See Jorgenson, Gollop, and Fraumeni (1987) or Jorgenson (1990) for a complete description.

From the perspective of a firm or industry, the proper measure of output is gross output, so that intermediate inputs are treated symmetrically with primary inputs. Gross output by industry, in current and constant prices, comes from the Office of Economic Growth of the Bureau of Labor Statistics.

Hours worked by industry comes from NIPA. Apart from data revisions, this unadjusted data is the same as that used by Caballero and Lyons. From the point of view of a producer, however, the proper measure of labor input is not merely labor hours: firms also care about the relative productivity of different workers. Jorgenson uses data from household surveys to disaggregate total hours into hours worked by different types of workers, distinguished by demographic variables such as sex, age, and education. Jorgenson then assumes that workers are paid proportionately to the value of their marginal products. This assumption allows him to calculate labor input as essentially a weighted sum of the hours worked by different types or workers, weighting by relative wage rates.

The BEA provides the industry investment data used to create stock estimates--in current and constant prices-- for 27 categories of producer durables and 23 categories of nonresidential structures. These stock estimates are calculated using the perpetual-inventory method, assuming that capital depreciates geometrically. Jorgenson also includes industry data on the stock of inventories and land. A simple industry capital stock measure, with no adjustments for quality, is an unweighted sum of the stocks of all types of capital. As an input measure, this is analogous to labor hours. Capital input from the point of view of a producer, however, should weight the different types of capital by relative productivities. Just as we need wage rates to calculate labor input, we require rental rates to calculate capital input. These rental rates are not directly observed. With either constant returns and competition or with monopolistic competition, however, total payments to capital are observed as property compensation, a residual after all other factors have been compensated. Jorgenson uses this to back out the implied rental rates for each type of capital, based on knowledge of the stock of each type of capital and its depreciation rate, as well as tax parameters such as the corporate income tax, depreciation allowances, and investment tax credits.

The data on intermediate inputs of energy and materials is constructed from the BEA input-output tables. These data appear to be the most difficult to construct. Conceptually, the task is straightforward. For each year, payments to intermediate factors are the difference between nominal gross output and nominal value-added; we want to divide this nominal intermediate input into indices of price and quantity. The gross-output price deflators give the price of each individual input, so creating an appropriate overall deflator simply requires the correct weights for each price. This requires consistent annual input-output tables. Unfortunately, the BEA compiles comprehensive input-output tables only about every five years. In Jorgenson's data, the benchmark tables are adjusted to make industry definitions consistent over time, and are then aggregated to the 35-industry level. These benchmarks are converted to shares of industry output, and then these shares are interpolated from benchmark to benchmark. This gives an estimated input-output table for each year.[18] As long as the weights change only slowly (which in fact they appear to do), this allows Jorgenson to create an appropriate annual price deflator for intermediate inputs.

[18] Wilcoxen (1988) discusses in detail the steps required for all of these calculations.

TABLE 1
GROSS-OUTPUT RESULTS

Parameter	Quality adjusted		Non-quality adjusted	
	SUR	3SLS	SUR	3SLS
γ	1.00 (0.009)	0.96 (0.014)	1.02 (0.009)	1.00 (0.013)
κ	-0.02 (0.011)	0.01 (0.016)	-0.035 (0.011)	-0.025 (0.015)

Standard errors in parentheses. Sample period is 1953-1985.

TABLE 2
VALUE-ADDED RESULTS

Parameter	Panel A: Divisia Value Added		Panel B: NIPA Value Added	
	SUR	3SLS	SUR	3SLS
γ	0.61 (0.029)	0.62 (0.050)	0.63 (0.025)	0.85 (0.042)
κ	0.16 (0.037)	0.51 (0.10)	0.55 (0.036)	0.63 (0.077)

Standard errors in parentheses. Sample period is 1953-1985.

TABLE 3
CORRECTED VALUE-ADDED RESULTS

Parameter	Panel A: Divisia Value Added		Panel B: NIPA Value Added	
	SUR	3SLS	SUR	3SLS
γ	0.98 (0.010)	0.96 (0.014)	0.97 (0.01)	1.09 (0.012)
κ	-0.08 (0.030)	0.023 (0.042)	0.35 (0.034)	0.04 (0.061)

Standard errors in parentheses. Sample period is 1953-1985.

TABLE 4A
VALUE-ADDED RESULTS, WITH $(1-c_M)/(1-s_M)$ CORRECTION

Parameter	Panel A: Divisia Value Added		Panel B: NIPA Value Added	
	SUR	3SLS	SUR	3SLS
γ	0.66 (0.030)	0.69 (0.047)	0.68 (0.026)	0.96 (0.045)
κ	0.10 (0.031)	0.17 (0.054)	0.61 (0.034)	0.39 (0.058)

Standard errors in parentheses. Sample period is 1953-1985.

TABLE 4B
VALUE-ADDED RESULTS, WITH CORRECTION FOR OMITTED dm TERM

Parameter	Panel A: Divisia Value Added		Panel B: NIPA Value Added	
	SUR	3SLS	SUR	3SLS
γ	0.97 (0.010)	0.94 (0.016)	0.98 (0.006)	1.04 (0.012)
κ	-0.07 (0.029)	0.034 (0.052)	0.32 (0.026)	0.18 (0.069)

Standard errors in parentheses. Sample period is 1953-1985.