Growing State Economies

How Taxes and Public Services Affect Private-Sector Performance

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EXECUTIVE SUMMARY

This report considers the evidence from the states—our "laboratories of democracy"— of the effect of public services on private-sector economic performance. The central question addressed in this report is whether public spending and taxes have important *indirect* effects on economic development—productivity, jobs, or output—over and above their *direct* effects on households.

The new empirical results reported here, which focus on the manufacturing sector, suggest that increases in state and local public services have positive and important effects on manufacturing productivity and output. However, state and local taxes have important but negative effects on private-sector performance. In most cases, increasing state taxes to finance increased public services will have little net effect on that state's economy. Alternatively, cutting spending on public welfare programs, and using the savings either to cut taxes or to expand public services, will boost a state's economy, but it will significantly reduce the incomes of the poor.

Large economic gains or losses are unlikely either from increasing taxes to finance increased public-services spending or from lowering taxes and reducing public-services spending. The merits of such policy changes depend on the specific programs in question.

This report also presents preliminary evidence that increases in state and local public services have positive and important "spillover effects" on the productivity and output of the manufacturing sector in neighboring states. These estimated spillover benefits are so large that some tax-financed increases in public-services spending may have regional or national economic benefits.

For state policy makers, the results imply that large economic gains or losses are unlikely either from increasing taxes to finance increased public-services spending or from lowering taxes and reducing public-services spending. The merits of such policy changes depend on the specific programs in question, not on any expectation of important, indirect economic effects on the state or local jurisdiction. In other words, from the standpoint of the state or local policy maker, public services should be provided as long as the state's citizens are getting their money's worth.

For national policy makers, the results suggest the need for a careful evaluation of whether specific public services have large spillover effects, because such spillover effects might justify national action to leverage state and local efforts. However, specific recommendations for national policy will require further research, since spillover effects are likely to vary greatly for particular policies and spending programs.

The new empirical results strongly support the traditional wisdom in

public finance that responsibility for financing assistance to the poor must be primarily assumed by the federal government. Left to their own devices, states may focus on economic development and neglect their responsibilities to their poorer residents. In a country with extensive mobility of population and business, only the federal government can serve the national interest in seeing that the poor are integrated into mainstream society.

INTRODUCTION

The economic effect of state and local government spending has been the object of a growing body of research. Governors and mayors want to know what they can do to increase their state or city's economic development. Politicians, the public, and the news media often debate whether cutting taxes or increasing spending is the best strategy for improving local economic development. Spending increases and tax cuts are often advanced under the rationale that they will bring significant collateral benefits for jobs and the economy over and above their specific benefits.

Spending increases and tax cuts are often advanced under the rationale that they will bring significant collateral benefits for jobs and the economy over and above their specific benefits.

A more encompassing motive for research on state and local government spending and economic development is to investigate how best to expand the national economy. Growth in real wages and income has slowed in recent years, in part because U.S. productivity growth—the growth of U.S. economic output compared to labor and other inputs—has been slow. Economists David Aschauer (1989a, b, c; 1990) and Alicia Munnell (1990a, b) argue that U.S. productivity growth is hampered by the slow growth of "public capital" (roads, mass transit, water and sewer systems, etc.). National data analyzed by these researchers show some correlation between public capital growth and U.S. productivity growth. If Aschauer and Munnell are correct, then increasing spending on public capital might boost U.S. productivity growth, thereby inducing more rapid increases in wages and incomes.

Evidence based on U.S. national data is limited for a number of reasons. One is that there is not a great deal of historical, statistical experience from which to draw. Another is that correlations in the national data between public capital and productivity could be the result of unknown external factors affecting both variables, or they could simply occur by accident.

State and local governments provide this debate with a natural "laboratory" in which many more observations are available.[1] Researchers have tried to use the diversity of experience in the states to examine how productivity is influenced by public-services spending, but they have yet to reach a consensus. This report makes a new attempt. In addition to using better data and statistical methods than previous research, this report addresses two key issues that have not been adequately considered to date:

(1) *The legitimacy of state and local government decisions about public services.* Even if state and local public spending increases productivity, this need not imply that spending should be increased. One could argue that state and local governments will independently make good decisions about taxes

and spending, weighing the negative effects of taxes against the positive effects of public spending on productivity and growth. This report considers whether state and local governments adequately evaluate the entirety of social benefits and costs resulting from their budgeting decisions. If not, there may be a case for reform that entails changes in the role of the federal government.

(2) *The cause-and-effect relationship between public spending and economic development.* Even if regional spending growth and productivity growth are correlated, it is difficult to prove that spending growth causes productivity growth. Strong productivity growth in a state may lead to increased spending on infrastructure, education, and other public services. This report presents new statistical evidence on whether increased public spending encourages economic development.

HOW PUBLIC SERVICES SPENDING CAN AFFECT STATE ECONOMIC DEVELOPMENT

State and local government spending can affect a local economy on the "demand side" or on the "supply side." On the demand side, government spending provides purchasing power that individuals and firms use to buy more goods and services. In turn, the sellers of these goods and services hire more employees to handle the expansion of demand, these new employees spend more, and so on. In a local economy, these demand-side effects are likely to occur more in businesses that serve the local market (e.g., fast-food restaurants) than in the area's "export-based" businesses. (By "export-based" businesses, regional economists mean businesses that sell their products to businesses and households elsewhere in the United States as well as to foreign countries.)

On the supply side, government spending might stimulate local economic activity by increasing business productivity or increasing the local area's attractiveness to workers.

On the supply side, government spending might stimulate local economic activity by increasing business productivity or increasing the local area's attractiveness to workers. If business productivity goes up, businesses can produce at lower unit costs. If the local area is more attractive to workers, local wage increases will be restrained. From lower business costs, additional production and new jobs in the local economy would be expected to follow.

These supply-side effects might increase business productivity in a wide variety of ways. The potential effects on business productivity of improving public physical capital have been widely discussed. A better road or water system directly supports the production process for business firms. A higher quality for any such input could enable business firms to produce more output for a given amount of labor and private capital. Unit costs of production could fall. In addition to directly raising business productivity, these lower unit production costs could attract additional private capital and increase labor demand in the state, further enlarging the state's economy.

Increasing physical public capital is not the only way in which public spending can accelerate a state's economic development. Public spending can also provide nontangible inputs to the business sector. Spending on higher education may provide research findings that help business firms improve their productivity. Many government agencies in one way or another also provide information that may be useful to the business community and may help improve business productivity.

Public-services spending may also increase business productivity by

improving the quality of labor. Elementary and secondary education, higher education, and health care spending can all improve the quality of labor skills.

Public-services spending can also provide benefits to a state's economic development that are not reflected in business productivity. An improvement in local amenities brought about by improved public services will increase the attractiveness of an area to workers. If the resulting increase in the labor supply puts downward pressure on local wages, local business expansion will be encouraged.

The crucial issue is how the supply-side benefits compare to the costs.

Many of these supply-side effects of public-services spending are likely to take place over a protracted period. It may also take time for an increase in public-services spending to actually increase public service quality. For example, an improvement in public school quality may take a while to be reflected in improved quality of the overall local labor force. Finally, because businesses will tend to stick with the locations and capital stock they already have, it will take some time for the business to fully respond to the incentives offered by greater local business productivity.

By contrast, demand-side effects on a state's economy from public spending or taxes are likely to occur quickly. But active state government policy to manipulate the demand side of a state's economy is seen by most economists as having limited benefit. Much of the augmented purchasing power from any state policy of stimulating demand would "leak out" of the state's economy, since local businesses and households would spend much of their added purchasing power on goods and services produced outside the state. Furthermore, increased state spending, without a tax increase, would often result in deficit spending, which is legally or politically restricted in almost all states. If a state spends more without borrowing more, any increased purchasing power that may result is roughly offset by decreased power elsewhere because of higher taxes or reduced public spending in other areas.

Public spending that can provide large "supply-side" benefits is of great public policy interest. The crucial issue is how the supply-side benefits compare to the costs. A public-spending and tax package that can increase business productivity by more than the tax cost, or that can make the local area more attractive to current and prospective residents, provides a strong argument for this policy.

RECENT TRENDS IN STATE AND LOCAL SPENDING

Before considering a formal statistical analysis of the relationship between state and local public services and economic development, we must deal with a common criticism: although state and local governments have in recent years, presumably, expanded enormously, U.S. productivity growth has been sluggish, leading some to suggest that state and local public services have small positive effects on private productivity, if any.

This impression of wild expansion of state and local governments is mistaken.

Allegations of the wastefulness of the 1980s' expansion of state and local public spending are widespread. For example, Stephen Moore of the Cato Institute argues that "[t]he root cause of the [state] budget crisis has been almost universally misdiagnosed by state lawmakers, economic analysts, and the media....[T]he primary culprit: a decade of runaway state government expenditures" (Moore 1991, 2). Andrew Bates, writing in *New Republic*, claims that "...[F]ar from being paragons of political courage, most of the recent state budget resolutions suggest no more backbone than those of the federal government. The states too are chronically unable to say no to powerful special-interest groups—in this case, to the public employee unions....[The states] in the worst shape are those whose fiscal policies left them particularly vulnerable to the recession. Instead of building up their reserves during the 1980s they went on a spending spree" (Bates 1991, 11).

This impression of wild expansion of state and local governments is mistaken. One source of this misconception is the failure to consider that prices and the economy have also grown. The most spectacular figures for state and local spending increases are those for nominal dollar increases, with no correction for inflation or for the size of the private economy, on which the need and demand for public services depends. Corrections for these factors make state and local spending trends look less dramatic, as will be shown.

Other more subtle factors also make spending increases a misleading indicator of public service trends. First, much of these spending increases are not for services that affect productivity but for transfers from one group in the population to another, such as payments to health care providers under Medicaid. In his recent analysis of state and local fiscal problems, Professor Edward Gramlich of the University of Michigan concluded that, "The main cause of declining state and local surpluses is found...in the large effects and explosive growth of health costs and related influences on the transfer sys-

tems of state and local governments" (Gramlich 1991, 274).

Second, the costs of the types of services provided by state and local governments are likely to rise faster than overall inflation. As Professor William Baumol of Princeton University and New York University pointed out in 1967, there are certain services "in which [the] quality [of the service] is judged directly in terms of [the] amount of labor" involved in producing the service, which makes it difficult to raise productivity by reducing the labor used per unit of output (Baumol 1967, 416). Baumol lists among these "stagnant" services "municipal government, education, the performing arts, restaurants, and leisure time activity." As wages go up in the economy, the costs of providing these services will go up accordingly. In contrast, for "progressive" goods and services in which it is easier to reduce labor input per unit of output, the cost and price effects of wage increases are reduced by these productivity improvements. Even if government spending is going up faster than average inflation, there may not be sufficient resources to maintain the quantity of public services.

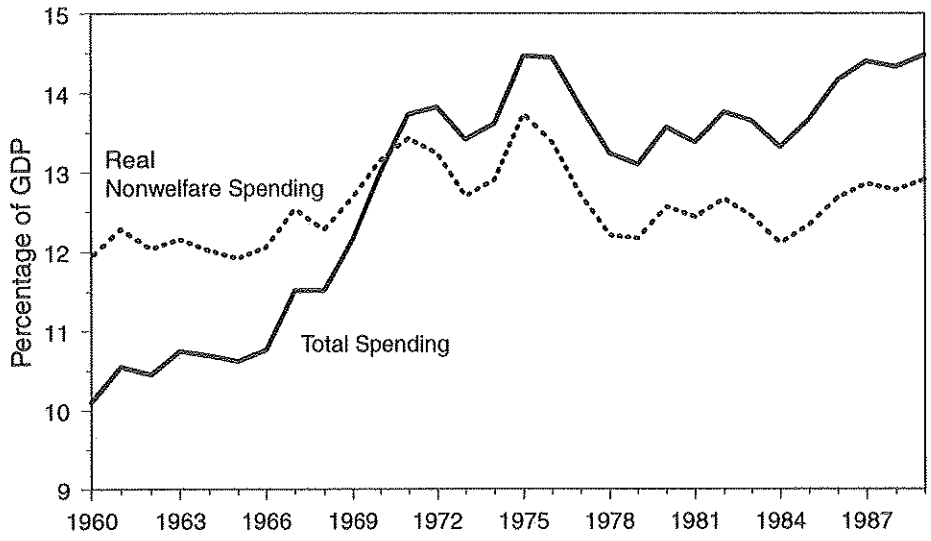
Most of the increase in state and local spending relative to the economy occurred in the 1960s and early 1970s. The increase since then has been far more gradual.

Third, social changes may have caused productivity declines in such state and local government services as education. For example, children may not be as "ready-to-learn" when they come to school. Causes may range from changing family structure to the influence of TV to gangs and drugs. Whatever the reason, these social changes make it more difficult for schools to do as good a job of educating children as in the past.

Figure 1 makes several rough adjustments to account for these factors and give a more accurate picture of trends in state and local public services. The "Total Spending" line shows the state and local spending as a percentage of gross domestic product (GDP), the total output of the U.S. economy. This calculation automatically corrects for overall inflation, population growth, and the increased demand for public services that occurs as economic activity expands and wages increase. As shown in the figure, most of the increase in state and local spending relative to the economy occurred in the 1960s and early 1970s. The increase since then has been far more gradual. Note that the major U.S. productivity growth slowdown began in 1973, around the time state and local spending growth slowed.

The second line in the figure focuses on state and local spending for nonwelfare purposes. Nonwelfare spending is then adjusted for changes in the prices of state and local public services (such prices, for the reasons outlined above, have gone up faster than overall prices[2]). The calculated real value of services is then expressed as a percentage of real GDP to get a "real

FIGURE 1
Trends in State and Local Government Spending, as a Percentage of GDP



Note: Calculations for real spending and real GDP use implicit price deflators from national income accounts. All figures compare spending throughout the United States for a specified fiscal year with the U.S. GDP for that same calendar year. All calculations in real dollars use a 1987 base year.

Source: Author's calculations.

services" trend line. This line shows much less change over time: real services increased somewhat in the late 1980s, but are still below their mid-1970s peak.[3]

None of this analysis disputes the need to reduce waste in state and local government spending. But additional state and local government spending may still be needed to improve services that increase private productivity. To see what types of government spending most affect productivity, we must compare the economic performance of different states or local areas. This comparison is the focus of the rest of this report.

PREVIOUS RESEARCH

Table 1 summarizes a number of studies conducted since 1985 that have examined how public services affect a state or local area's economic productivity. The results are sensitive to definitions of public services, choices of control variables, the selection of states or metropolitan statistical areas as the observed regions, and econometric methodology.

Studies do not consistently find positive effects of public services on productivity.

These studies do not consistently find positive effects of public services on productivity. Why has research in this area failed to reach a consensus? Holtz-Eakin has argued that some studies mistakenly fail to include controls for state-specific "fixed effects" on private-sector productivity. The idea is that there may be many important state attributes influencing private-sector productivity that are unmeasured or mismeasured, such as the state's raw materials, weather, access to markets, cultural makeup, etc. If these fixed effects are not included in the statistical analysis, the estimated effects of public-sector capital on private-sector productivity may be biased. Holtz-Eakin argues that the estimated positive effects of public capital on private-sector productivity disappear when state fixed effects are included in the estimation procedure.

Holtz-Eakin's hypothesis fails to fully explain all the disparate results. As noted in the table, a number of studies controlling for fixed effects found some positive effects on productivity by some public services (Eisner 1991; Moomaw and Williams 1991; Morrison and Schwartz 1991; Eberts 1990; Beeson and Husted 1989). However, other studies controlling for fixed effects found no statistically significant positive effects of public services on private-sector productivity (Holtz-Eakin 1992; Hulten and Schwab 1991; Beeson 1987).

Three methodological problems may be contributing to this lack of consensus:

Current productivity measures are not necessarily a good indicator of the efficiency of a local economy.

Productivity is extraordinarily difficult to measure. In many studies, the productivity variable could be more of an echo of random noise than anything that is really going on in the economy.[4]

In addition, business productivity measures will not capture an important possible indirect effect of public-services spending: it can increase the supply capacity of the local economy through increasing the attractiveness

of an area to workers. A local economy in which more workers wish to live, even if wages are somewhat lower, has increased its productive capacity as surely as an economy in which business has increased its productivity.

Rather than confine our attention to productivity, it is useful to consider better-measured indicators of the health of the local economy. One is the growth performance of a regional economy's industries that export their products outside of the region. Any estimated positive effects of public services on the growth of a region's export industries are likely to be due to the positive effects of public services on the productivity of local businesses or on the local area's attractiveness to workers. In contrast, the effects on nonexport industries of public spending on public services may be due to demand influences—public spending may affect the magnitude of local demand, causing growth in these industries to change. As noted previously, the supply-side effects of public spending are more policy-relevant than the demand-side effects.

Manufacturing industries, which generally export their products outside the state or metropolitan area in which they are produced, tend to have their growth positively affected by state and local public services.

There is a sizable literature, reviewed in Bartik (1991a), on how public services affect regional growth. **Table 2** summarizes some of the results of these studies; they show that manufacturing industries, which generally export their products outside the state or metropolitan area in which they are produced, tend to have their growth positively affected by state and local public services.[5] There is a greater consensus in the regional growth research literature than in the regional productivity research literature that public services have positive effects; this consensus may occur, in part, because the growth of an industry's output or employment is easier to measure than its productivity level or growth rate. In addition, growth measures capture the economic development effects of making an area more attractive to workers, whereas productivity measures do not.

Many studies consider narrow definitions of public services.

It would be ridiculous to assume that the only public service that matters to private productivity is total physical public capital. Surely the effects on private-sector productivity of highways differ from the effects of government office buildings. Also, one would think that public services that affect human capital—education and health services—could have effects on private-sector productivity at least as important as highways or water and sewers.

If a study looks only at the effects of total physical public capital, it may miss the effects of highways or education spending. Note that, in Table 1, studies that control for fixed effects but consider a wider variety of public services often find positive effects on private productivity of some of these

TABLE 1
Recent Studies of the Effects on Productivity of State and Local Public Services

Study	Dependent Variable	Types of Public Capital/Service Variables Included	Fixed Region Effect Controls	Other Aspects of Methodology	Results
Garcia-Mila and McGuire (1992)	Real GSP, 1970 to 1983, annual observations	Hghway capital per square mile, education expenditures (combined K-12 and post-secondary), median years of schooling	No	Regression, estimates of production function; capital and public service variables lagged	Both highways and education have significant effects, but education's effects are stronger
Holtz-Eakin (1992)	Real private GSP, 1969-1986, annual observations; also sum of private GSP for regions	Total public capital	Yes	Regression estimates of production functions	Public capital insignificant when allow for fixed or random state or regional effects
Eisner (1991)	Real GSP, 1970 - 1986, annual observations	Total public capital; also public capital disaggregated to highways, water and sewers, and "other"	Yes	Regression estimates of production functions	Total capital insignificant in fixed-effect model; highways and water/sewer significantly positive, other public capital significantly negative
Hulten and Schwab (1991)	Estimated growth in multifactor productivity, by Census regions, 1970-86, annual observations, based on regional outputs, inputs, and input factor shares	Total public capital; public capital broken down into roads, water and sewer, and other	Yes, implicitly	Regression analysis of changes in regional MFP, and how affected by changes in public capital; private capital included in this regression	Public capital variable always insignificant
Moomaw and Williams (1991)	Average TFP growth in manufacturing by state over 1954-76 period, calculated as difference between real value-added growth and input growth weighted by factor shares	Level and percentage change in proportion of manufacturing workers with high school education; interstate highways per square mile in state	Yes, implicitly	Regression estimates of total factor productivity growth	Both education and highways have positive effects, although not statistically significant in all specifications
Morrison and Schwartz (1991)	System of equations, including input/output ratios, and marginal cost/price of output, for manufacturing, state data, 1970 to 1987, annual observations	Sum of road and water and sewer capital	Yes	Regression analysis of how manufacturing input mix and marginal cost affected by public capital, holding production and nonproduction wages, energy prices, and private capital constant; estimated separately for four Census regions	Public capital lowers manufacturing costs; comparison with calculated social cost of public capital suggests that manufacturing cost reductions from public capital exceed its social costs for South, and are about equal to social costs for other regions; results sensitive to inclusion of energy prices and wages of two types of labor

TABLE 1 *(cont.)*
Recent Studies of the Effects on Productivity of State and Local Public Services

Study	Dependent Variable	Types of Public Capital/ Service Variables Included	Fixed Region Effect Controls	Other Aspects of Methodology	Results
Munnel (1990)	Real GSP, 1970 to 1986, annual observations	Total public capital, also public capital dis-aggregated to roads, water and sewer, and "other"	No	Regression estimates of production function	Total public capital significant; when disaggregated, only raods and water and sewer capital are significant
Eberts (1990)	Estimated total factor productivity growth in manufacturing, in 36 metropolitan areas, over two periods: 1965-73, 1973-77. TFP calculated using adjusted manufacturing value added and factor shares of labor and capital. Also value-added growth, 1965-73, 1973-77	Total public capital	Yes	Regression estimates of determinants of TFP; regression estimates of determinants of output growth, with controls for labor and private capital growth	Public capital significant in explaining TFP growth, 1965-73, when include control for age of housing in MSA; public capital insignificant in all other regressions
Beeson and Husted (1989)	Average manufacturing productive efficiency by state over 1959-73 period, calculated from preliminary regression of manufacturing value added on labor and capital inputs	Percent of population with high school education	Yes, in part	Regression estimates of determinants of a state's average productive efficiency; preliminary estimates of average production efficiency allow for unobserved state random effects, rejects state fixed effects	States with a higher percentage of high school educated tend on average to be more productive, controlling for industry mix, unionization, and urbanization
Costa, Ellson, and Martin (1987)	Value added by state, manufacturing and all sectors, 1972	Total public capital	No	Regression estimates of production function	Public capital has positive and significant effects for "mean state"; effect of public capital on productivity declines as public capital increases, holding other inputs constant
Beeson (1987)	Average manufacturing total factor productivity growth by state, from 1959 -73, calculated from preliminary estimates of production function for manufacturing value added	Average years of education	Yes, because focuses on changes	Regression estimates of determinants of TFP growth	Education has positive effect, but not statistically significant
Eberts (1986)	Real value added in manufacturing for 38 MSAs, 1958 to 1978, annual observations	Total public capital stock; also sum of public roads and water treatment and supply capital stock	No	Regression estimates of production function	Public capital stock has significantly positive effect at means

TABLE 2
Summary of Results from Recent Studies of the Effects on Growth of State and Local Public Services

	% of Studies With at Least One Positive and Statistically Significant Public Service Variable Coefficient
All inter-area studies	60% (30 studies)
All studies with results for manufacturing	61% (23 studies)

Notes: These results are adapted from Table 2.5 and Appendix 2.3 in Bartik (1991a), although in some cases the original studies were re-examined to determine the effects for manufacturing. Studies included were all studies since 1979 that have focused on state or metropolitan area growth. Studies focusing on intrametropolitan growth patterns were excluded from the analysis. Statistical significance was judged based on a 95% one-tail test. The first row report results based on the most aggregate measure of local growth that is used in the study. The second row reports results only for studies that used some type of manufacturing growth as a dependent variable.

public services (Eisner 1991; Moomaw and Williams 1991; Beeson and Husted 1989).

Analyses of public services and private-sector productivity may be biased by unobserved factors that cause state productivity growth trends to diverge.

Holtz-Eakin (1992) appropriately argues that states may differ in unobserved or poorly measured ways that cause differences in productivity *levels*. But it is also true that states may differ in unobserved or poorly measured ways that cause differences in productivity *growth*. Economic developers or business persons familiar with different regional economies will often refer to a region's "entrepreneurial climate," measuring the features that affect how rapidly the area adopts innovations. Entrepreneurial climate may reflect local cultural attitudes toward innovation, the structure of local financial markets, or the structure and competition of local industry. All of these aspects of a regional economy are difficult to measure and will affect productivity growth. Failure to control for these region-specific effects on productivity growth may bias estimates of the effects of public services on productivity.

The new research conducted for this report will try to address these three problems. There is a fourth problem with previous research, however, that is not addressed here and that may bias results. None of the studies in Table 1 have adequate controls for the quality of state and local public services. The overwhelming majority of studies use public spending in some way as a proxy for public service quality. That convenient approach will also be followed in this report. The problem with using public spending instead of public service quality is that it may underestimate the true effects on private-sector productivity of improving public service quality.[6] This bias should be kept in mind when reviewing any research on public services and productivity.

NEW RESEARCH ON PUBLIC SERVICES AND ECONOMIC DEVELOPMENT

This section highlights key results of research on how state and local public services affect both productivity and output growth in the manufacturing sector of a state's economy. This report focuses on manufacturing, for two reasons: manufacturing output data is more reliable than output data for other sectors of a state's economy, and manufacturing is a key export sector whose growth should be affected by anything that influences a state's productivity. (See the appendix for more details on the methodology used as well as other results.)

This analysis attempts to address some of the problems that have plagued previous research on state and local public services and productivity.

This analysis attempts to address some of the problems that have plagued previous research on state and local public services and productivity. First, it considers effects of state and local public services on a state's manufacturing output as well as its manufacturing productivity. If public services in fact have positive effects on productivity in a state's export industry, capital and labor should flow to that state's export industries. These capital and labor flows, along with the increases in productivity, should result in greater public-services effects on export industry output than on productivity. These effects on output should be easier to detect than effects on productivity, even if productivity were perfectly measured. In addition, if productivity is mismeasured for some reason, effects on the output of export industries may be detectable while there is too much "noise" in the productivity data to tell what is going on. Finally, output measures may show whether public services make an area more attractive to additional labor supply, whereas productivity measures will not.

Second, this analysis focuses on public spending rather than public capital. It looks at five categories: elementary and secondary education spending, higher education spending, highway and roads spending, health spending, and all other nonwelfare spending. This broader focus recognizes that economic-development effects may occur from many public services, not just public capital. In addition, public spending is easier to measure than public capital; the latter requires assumptions about depreciation, retirement, and usage rates.[7]

Third, the estimates here take account of the effects of unobserved state characteristics on state productivity or output growth. As mentioned above, states may have different entrepreneurial climates that affect productivity and output growth. In addition, we know that states in the Sunbelt, over

most of the post-World War II period, have had faster growth for reasons that might be impossible to fully explain. Without some controls for unobserved state characteristics that affect state manufacturing output growth, any characteristic of the South—for example, higher per capita consumption of iced tea—will tend to be a good predictor of stronger state output growth. The true estimated effects of public services on output or productivity growth may be biased.

Because this new research allows for state effects on growth, it differs from other recent studies in the question it addresses. Some of the recent productivity studies ask a simple question: do states with higher public-capital levels have higher productivity levels? Studies that control for fixed state effects on productivity or output levels ask another question: do states with higher growth of public services have higher productivity or output growth? But studies such as this one, which allow for state effects on productivity growth or output growth, ask a more sophisticated question: do states have faster productivity or output growth in periods after they have increased public-services spending than in periods of smaller increases? This research question is more complicated to understand and estimate, but is less likely to be biased by unexplained differences across states.

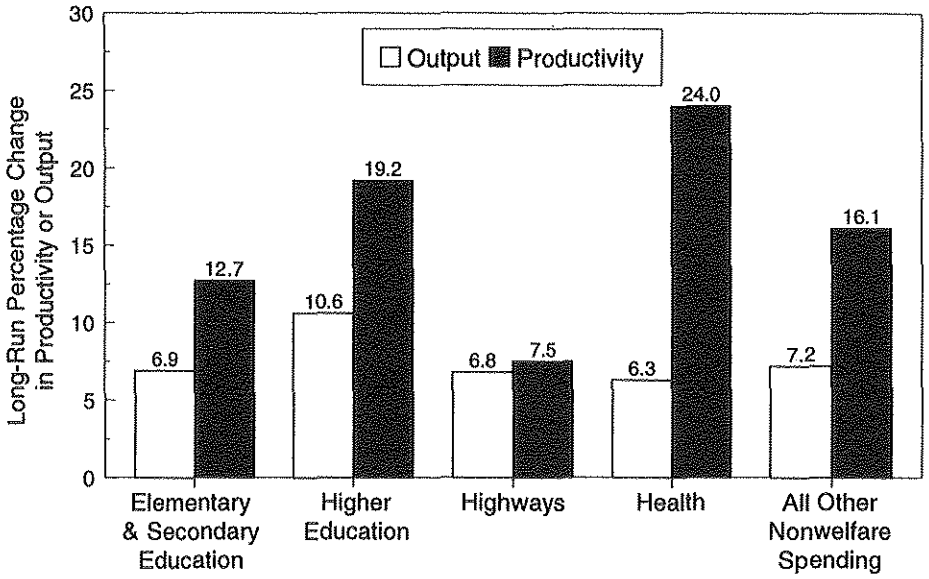
Do states have faster productivity or output growth in periods after they have increased public-services spending than in periods of smaller increases?

Results

Figure 2 presents these new estimates of the long-run effects of different types of state and local public spending on a state's manufacturing productivity and manufacturing output. The estimates are based on state data from 1958 to 1989.

The effects shown are for a permanent increase in a particular category of public spending of 1% of state income. Since the object is to isolate the effect of the spending increase on productivity and output, a mock experiment has been constructed that precludes events that could independently bias the result. It is assumed that the spending increase does not unbalance the state's budget and that the way the increase is financed affects productivity and output. For this reason, a reduction in welfare spending is assumed to be the source of the spending increase. The effects on manufacturing productivity and output of cutting welfare spending should be small; the effects in Figure 2 probably reflect the positive effects of increased public services, not the effects of cutting welfare spending. One could argue that welfare spending may mistakenly appear to negatively affect manufacturing output and productivity if lower manufacturing output leads to higher welfare spend-

FIGURE 2
Long-Run Effects of Increases in State and Local Public Spending on Manufacturing Productivity and Output



Note: Figure shows the long-run percentage change in a state's manufacturing productivity and output due to increasing different types of state and local public spending by 1% of the state's personal income. Long-run effects came from specification with 11 lagged years in all state and local fiscal variables. The increases in state and local public-services spending is assumed to be offset by reduced welfare spending. See appendix for more details. Standard errors of estimated effects on productivity and growth are as follows: elementary and secondary education (3.36, 4.76); higher education (3.96, 5.55); highways (3.15, 4.09); health (5.19, 7.52); all other (3.29, 4.74). All estimated coefficients are statistically significant at 0.95, except for health spending's effect on output.

Source: Author's calculations.

ing. This issue of the direction of causation will be addressed later. It should be emphasized that this exercise does not necessarily support the policy decision to shift resources away from antipoverty efforts in a state's budget. This is discussed below as well.

The public service effects reported in Figure 2 are large enough to be considered in policy decisions, but they are not implausibly large. Average state and local public spending in each of these categories ranges from 1% to 5% of state income. A spending increase of 1% of state income in any category represents a sizable percentage increase, typically adding at least 20% to what the state and local governments are already spending in that category.[8]

The relative estimated effects of these public services on productivity and output are reasonably consistent with the hypothesis that state and local

public services affect manufacturing productivity. Effects of most public services on manufacturing output are generally greater than effects on manufacturing productivity (only marginally so, in the case of highway spending), indicating that increases in these public services attract labor and capital to a state's manufacturing sector.

Why are these results more positive about the effects of public services than some previous studies? The differences could be the result of a variety of methodological and data differences, such as the focus on manufacturing, the way in which public services are measured, or the controls for unobserved state trends. Some preliminary regressions, described in the appendix, indicate that the controls for unobserved trends may be particularly important. Without such controls, one frequently gets nonsensical results in which public services have negative effects and taxes have positive effects.

PUBLIC POLICY IMPLICATIONS OF THE NEW RESULTS

This new research provides support for the belief that state and local public services have positive effects on business productivity and output. But this finding does not necessarily imply that public-services spending should be increased or that welfare spending should be reduced. Increased public spending will usually be financed by higher taxes. Increased taxes may reduce a state's output and productivity. The benefits of greater public services must be compared with the costs of higher taxes. Presumably, state and local governments try to make this comparison. In a related vein, reducing welfare spending deprives some state residents of income.

The benefits of greater public services must be compared with the costs of higher taxes.

State and Local Taxes and Public-Services Spending

This new research's estimated models can be used to calculate the effects of increases in public services that are financed by tax increases. The estimated models include a number of possible financing sources, including two tax financing categories: property taxes and all other taxes. Increased taxes used to finance increased welfare spending are estimated to have statistically significant negative effects on a state's manufacturing output and productivity. As noted above, however, it seems unlikely that welfare spending in itself has any direct effects on manufacturing, so the negative effects on the state's manufacturing sector are probably due to the disincentive effect of taxation and not to any reaction against welfare. As shown in the full set of results in the appendix, the negative effects of increased property taxes are significantly less (in absolute value) than the negative effects of other types of taxes. Based on these estimated effects, there are two cases in which tax-financed increases in public services would have statistically significant positive effects. An increase in higher education spending of 1% of a state's personal income, financed by a property tax increase, would increase state manufacturing output in the long run by 8.3%[9]; a similar boost in health spending would increase state manufacturing output in the long run by 13.0%. In one case, tax-financed increases in public-services spending would have statistically significant negative effects: an increase in roads spending of 1% of a state's personal income financed by other taxes (not property taxes) would reduce state manufacturing output in the long run by 10.0%. All other

tax-financed increases in public services spending fail to have statistically significant effects on state manufacturing output or productivity, negative or positive.

The implications of these estimates for state and local fiscal policies are modest. The use of property taxes to finance higher education or health spending is theoretically possible, but highly unlikely. The estimates do not indicate that either a low-tax, low-services strategy or a high-tax, high-services strategy will always be the best for promoting a state's economic development. Some tax-financed public service increases will help development and some will hurt; in many cases taxes and public services will probably have offsetting influences on development.

Some tax-financed public service increases will help development and some will hurt; in many cases taxes and public services will probably have offsetting influences on development.

The results contradict some of the more extreme claims of supply-side economists. For example, Stephen Moore of the Cato Institute argues that, "There is now an emerging consensus among public finance economists, substantiated by solid research, that states with low or declining tax rates are economically outperforming states with high or rising tax burdens" (Moore 1992, 6). Neither the findings here nor the literature reviewed in the previous section support Moore's argument.

Welfare Spending and State Economic Development

The results might appear to be consistent with the position that cutting welfare spending will benefit the economy, since cutting such spending and using the savings either to cut taxes or increase spending on public services is estimated to increase a state's manufacturing output and productivity. But any policy on welfare spending should also consider the social benefits of such spending.

From a state perspective, it must be recognized that, while economic development is important, state and local policy makers have other goals. In particular, policy makers are interested in poverty and economic equality. Economic development may increase the earnings of workers in poor families, but this income may be more than offset by the reductions in welfare spending.

In other papers, Bartik (1991b; 1994)[10] analyzes the effects of local employment growth on the well-being of various income groups. These benefits due to jobs are, not surprisingly, increased labor earnings; some benefits also are due to increased real estate values. **Table 3** uses these calculations, figures on welfare receipt by income group, and the estimates

TABLE 3
Distributional Effects of Across-the-Board Cut in Taxes by 1% of State's Income, Financed by Cuts in Welfare Spending

Income Group	Gross Labor Market and Housing Market Benefits, As % of Income	Gains From Lower Personal Taxes as % of Income	Costs of Cutting Welfare, as % of Income	Net Dollar Benefits, as % of Income (= Sum of Other Columns)
Lowest Income Quintile	12.4%	1.0%	-16.5%	-3.1%
2nd	7.2	0.7	-2.4	5.5
3rd	6.8	0.6	-0.4	7.0
4th	7.0	0.6	-0.1	7.5
Highest Income Quintile	3.6	0.7	-0.0	4.3
Overall Average	5.7	0.7	-1.0	5.4

Notes: Table based on estimates from appendix and on estimates in Bartik (1991b). Across-the-board cut in taxes of 1% is equal to 10.0% cut in both property and non-property taxes (cut in property taxes of 0.36% of personal income, non-property taxes of 0.64% of personal income). Using coefficients in appendix, predicted increase in manufacturing output is 15.1%. If overall employment also increases by 15.1%, gross benefit column in Table 5 of Bartik (1991b) is used to calculate gross benefit here. Calculation adjusts gross benefits down by (1/1.12) to reflect CPS underreporting of income; implicit assumption is that non-CPS reported income would not, on net, change due to employment growth, which seems reasonable as much of nonreported income is nonlabor income. Benefits of reduced taxes are allocated across income groups based on tax cost column in Bartik (1991b). This allocation in turn is based on Pechman (1985; variant 3b on p. 61). The personal share (versus business share) of state and local taxes is assumed to be 66.1% (U.S. ACIR, 63). Welfare costs are allocated across income groups based on transfer cost column in Bartik (1991b). This in turn is based on tabulation from CPS of income and transfer payments by quintile.

from this project of how taxes affect manufacturing output, to simulate the effects on various income groups of cutting welfare spending by 1% of a state's personal income and using the savings to cut property and other taxes.[11] This welfare cut/tax cut policy would increase the income of the top four-fifths of the population, but the bottom fifth would suffer a significant net income loss.[12] Thus, the rising tide of economic development in this scenario is not sufficient to lift all boats.

Left to its own devices, a state government might approve of a welfare cut/tax cut policy that helped the top 80% at the expense of the poorest 20%. But for a number of reasons, the national government might see things quite differently.

First, a state's inclination to be generous to the poor could be tempered

by the fear that such a policy might create a "welfare magnet." Second, a state's reluctance to help the poor could be fed by an expectation that other states would not take commensurate responsibility, even if no "magnet" effect were contemplated. Third, sympathy for the poor could spill over state boundaries, even if all states were equally generous, so the total aid desired by potential donors through the nation for each needy person could exceed the aid desired by the residents of that state. Fourth, the benefits of tax cuts are to some extent a zero-sum game among the states. A state's gain comes at some expense for others, so the national benefit of uncoordinated state tax cuts is less than the sum of expected individual state benefits. The estimates of this paper suggest that only half of the effect on manufacturing output of lower taxes is due to greater productivity; the other half is due to increases in the state's employment and capital, which will largely come from other states.[13]

The conventional wisdom in public finance—that the national government in a federal system should be responsible for income distribution policy—still applies.

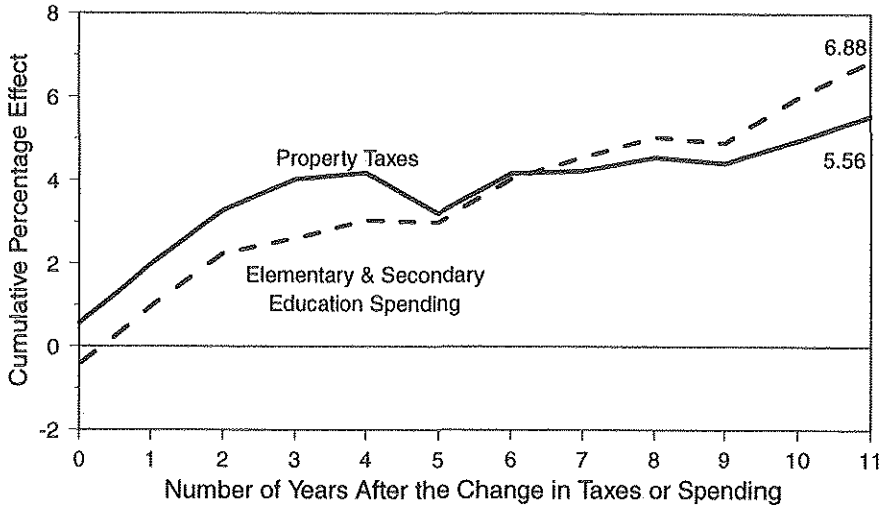
State and local governments are not well-situated to deal with "externalities" in welfare policy. The conventional wisdom in public finance—that the national government in a federal system should be responsible for income distribution policy—still applies. Increasing tax rates to finance increased welfare payments is against the self-interest—narrowly defined—of many and perhaps most of the voters of a state. In addition to the negative effects of increased tax rates on economic development, there is evidence that increased welfare payments may have long-term effects on the migration patterns of the poor (Gramlich and Laren 1984; Blank 1988).

Short-Term Versus Long-Term Thinking About State Fiscal Policies

The estimation procedure used here allows the output and productivity effects of state and local taxes and spending to vary over time for up to 11 years after some change in policy.[14] This allows an assessment of whether the net effects of tax-financed increases in public spending vary depending on whether policy makers focus on the short or long term.

The estimates indicate that the negative effects of increased taxes occur sooner than the positive effects of increased public-services spending. **Figures 3, 4, 5**, and **6** present some examples of this pattern for property taxes and public school spending (Figures 3 and 4) and for non-property taxes and higher education spending (Figures 5 and 6).

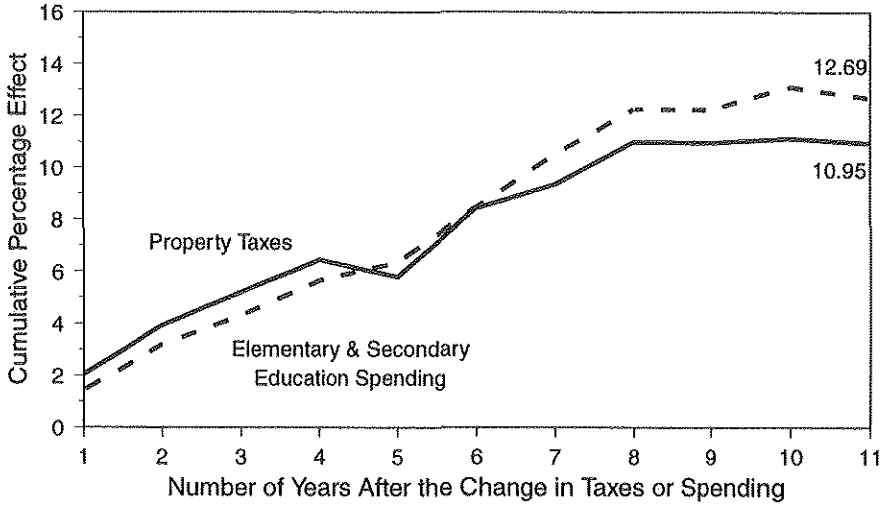
FIGURE 3
Comparison of Effects on State's Manufacturing Productivity of Reductions in Property Taxes and Increases in Elementary & Secondary Education Spending



Note: Figure compares the value of cumulative percentage effect on state's manufacturing productivity of a reduction in property taxes of 1% of state personal income and an increase in elementary and secondary education spending of 1% of state personal income. The difference between the two effects is the net effect of increasing property taxes to finance increased elementary and secondary education. See the appendix for more details on these estimated effects.

Source: Author's calculations.

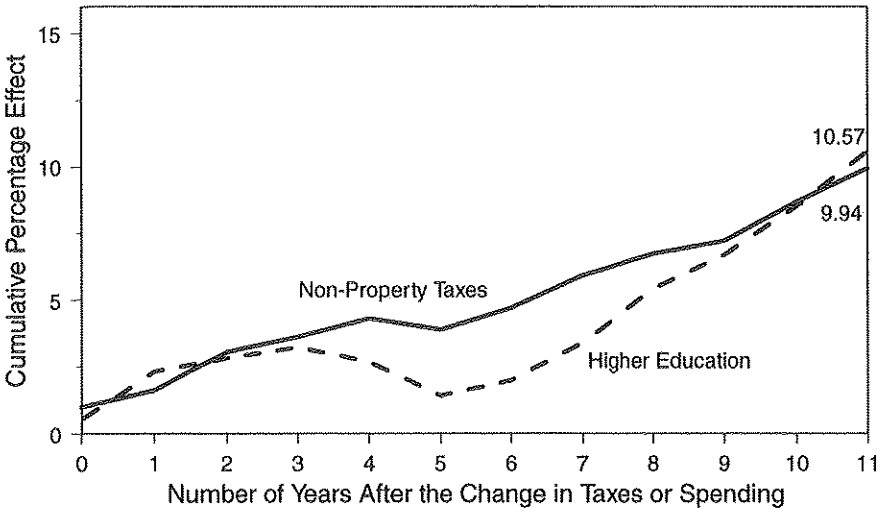
FIGURE 4
Comparison of Effects on State's Manufacturing Output of Reductions in Property Taxes and Increases in Elementary & Secondary Education Spending



Note: Figure compares the cumulative percentage effects on manufacturing output of a reduction in property taxes of 1% of state personal income and an increase in elementary and secondary education spending of 1% of state personal income. See the appendix for more details on these estimated effects.

Source: Author's calculations.

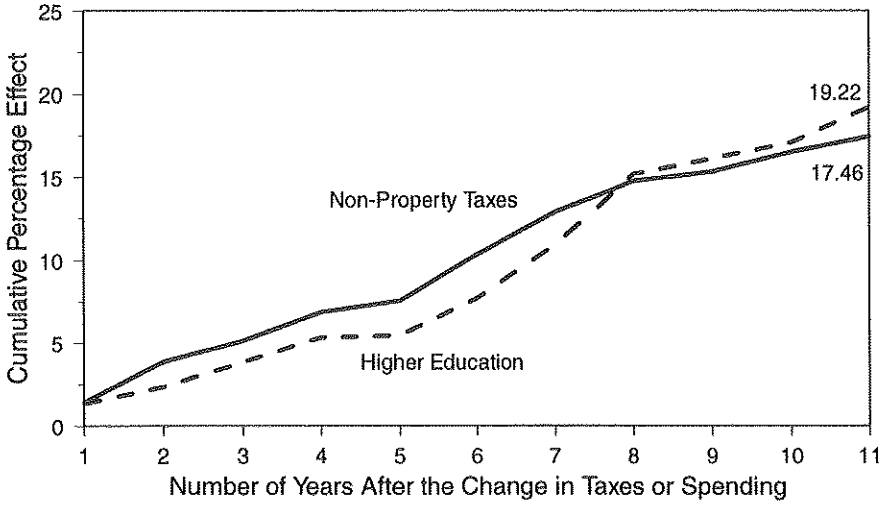
FIGURE 5
Comparison of Effects on State's Manufacturing Productivity of Reductions in Non-Property Taxes and Increases in Higher Education Spending



Note: Figure compares cumulative percentage effect on a state's manufacturing productivity of a reduction in non-property taxes of 1% of state personal income and an increase in higher education spending of 1% of state personal income. Spending effect minus tax effect is the net effect of increasing non-property taxes to finance higher education spending. See the appendix for more details on these estimated effects.

Source: Author's calculations.

FIGURE 6
Comparison of Effects on State's Manufacturing Output of Reductions in Non-Property Taxes and Increases in Higher Education Spending



Note: Figure compares cumulative percentage effect on a state's manufacturing output of a reduction in non-property taxes of 1% of state personal income and an increase in higher education spending of 1% of state personal income. Spending effect minus tax effect is the net effect of increasing non-property taxes to finance increased higher education spending. See the appendix for more details on these estimated effects.

Source: Author's calculations.

Spillover Effects

Officials in one state are likely to consider only the benefits and costs that accrue to the state itself in making fiscal decisions. These fiscal decisions could be disadvantageous from a regional or national perspective.

A state's fiscal decisions may cause three types of spillover effects to other states: incentive spillovers, demand spillovers, and public good spillovers. Incentive spillovers occur when an increase in the fiscal attractiveness of one state steals business away from neighboring states. Businesses often restrict their attention to location in one particular region in the United States to ensure access to raw materials, suppliers, or markets. If a particular state in that region reduces its taxes or increases its public services, businesses have an added incentive to locate in that state rather than elsewhere in the region. This incentive spillover results in positive effects of tax increases in neighboring states on that state's manufacturing output and negative effects of public-services increases.

Spillover effects encourage inefficient government decision making.

Demand spillovers occur when fiscally induced increases in business activity in one state result in increases in demand for the output of that state's neighbors. More business activity in a state will increase consumer demand and demand for the output of business suppliers. Both types of increased demand will boost economic activity in neighboring states. This type of spillover results in negative effects of tax increases in neighboring states on that state's manufacturing output and positive effects of public-services increases.

Public good spillovers occur when the public services in one state are directly or indirectly used by businesses or residents of nearby states. One state's roads may improve the access to markets and supplies of businesses in neighboring states, helping reduce these businesses' costs. A better-educated, healthier labor force in one state may be available for migration to nearby states, resulting in higher productivity and lower business costs. This type of spillover results in positive effects of public-services increases in neighboring states on a state's manufacturing output and productivity.

Spillover effects encourage inefficient government decision making under two conditions. First, the net spillover effect of some feasible package of tax and spending changes must be significantly positive. Second, as a result of these spillover effects there must be a package of regionwide changes in fiscal policy that would significantly increase the region's economic development. Even though this fiscal policy package would be in the region's interest, it might not be in the interests of an individual state because of the significant spillover benefits. Furthermore, if this fiscal policy package is in

the regional interest, it presumably is in the national interest. The positive spillover effects at the regional level would presumably be even greater at the national level.

A simple measure of spillover effects is used in this work. The average tax and public spending rates of all states bordering a given state are included as independent variables in the equations explaining a state's manufacturing output and productivity growth.[15] Tax rates and public spending rates of a state's neighbors are measured similarly to the state's own tax and spending rates, that is, as a percentage of the personal income of the state in which the taxes and spending occur.

The results suggest that a state's public-services spending causes positive spillovers for its neighbors.

The results suggest that a state's public-services spending causes positive spillovers for its neighbors. For each of the five types of public-services spending in this empirical analysis—elementary and secondary education, higher education, roads, health, and other nonwelfare spending—spending in neighboring states caused a state's manufacturing output to significantly increase. In the case of manufacturing productivity growth, spending in neighboring states on elementary and secondary education, roads, or miscellaneous nonwelfare spending had significantly positive effects.

In many cases, a regionwide increase in taxes and public-services spending is estimated to have statistically significant, positive effects on regional economic development. A regionwide increase in property taxes to fund an increase in elementary and secondary education, roads, health, or miscellaneous nonwelfare spending would significantly increase overall regional manufacturing output. A regionwide increase in miscellaneous nonwelfare spending, funded by either increased property taxes or non-property taxes, would increase regional manufacturing productivity.

Further study is needed about spillover effects before any policy recommendations can be made. (As discussed in the appendix, there may be other explanations for these strong correlations between one state's economic fortunes and its neighbors' fiscal policies.) But these results certainly suggest that, in thinking about state and local fiscal policy, it is worthwhile to consider what kind of regional and national benefits might accrue from one state's policies.

Generalizing the Results

All the empirical results reported here are for the effects of taxes and public spending on the manufacturing sector. A complete evaluation of optimal state and local fiscal decisions would also have to consider the effects on the

nonmanufacturing business sector and the household sector. This evaluation would require further research focused on the benefits and costs of state and local fiscal decisions for these groups, including spillover effects from one state to another as well as long-run and short-run effects.

The results dispel some common misconceptions about state and local fiscal policy. It is often thought that state and local governments extract resources from the business sector for the benefit of the household sector: for instance, the most prominent state and local public service is education, which directly benefits households rather than businesses. In debating appropriate levels of state and local taxes and public spending, one sometimes hears the argument that a given tax and spending increase might be a good idea except for the possible negative consequences for the state's economic development. The results reported here suggest that, for at least one key part of the business sector—manufacturing—the benefits from increased public spending are of about the same magnitude as the taxes needed to finance the spending, even if spillover effects are ignored. Perhaps concern over the negative economic-development consequences of tax-financed increases in public-services spending is overblown, and these policies can be evaluated in a common-sense manner that asks whether the benefits of these policies are worth the costs.

For at least one key part of the business sector—manufacturing—the benefits from increased public spending are of about the same magnitude as the taxes needed to finance the spending.

THE CAUSALITY ISSUE

These new empirical results, like the results of previous research, are subject to the "causality critique": a statistical correlation between two variables does not prove that one of them causes the other. The correlation between increased public-services spending or between reduced taxes and increased manufacturing output or productivity does not show that changes in these fiscal variables caused the changes in manufacturing. Instead, increased manufacturing output may cause increases in public-services spending and reductions in tax rates.

This report's statistical procedures minimize the possibility of spurious correlations by focusing on how past changes in fiscal variables explain current changes in manufacturing output and productivity.

This report's statistical procedures minimize the possibility of spurious correlations by focusing on how past changes in fiscal variables explain current changes in manufacturing output and productivity. Current manufacturing output or productivity cannot be the cause of fiscal policies in the past. In addition, the analysis controls for unobserved trends in a state's manufacturing output or productivity that are fixed over the estimation period. Ignoring these unobserved trends could lead to misleading correlations among trends in the fiscal variables and in manufacturing output or productivity.

No perfect solution to the causality critique is available for most economics research, but there are two imperfect but practical approaches that can be used: Granger causality tests and instrumental variable techniques. These two approaches were applied to analyze whether fiscal variables cause changes in state manufacturing output. In the case of manufacturing productivity, causality is harder to analyze because manufacturing output, employment, private capital, and the fiscal variables could all potentially cause each other. An analysis of causation in the manufacturing productivity case is left to future research.

The idea behind Granger causality tests is straightforward. In the present case, the test is whether past changes in state and local fiscal variables are estimated to have significant effects on the current growth of manufacturing output after past growth in manufacturing output is controlled for. If this is so, then state and local fiscal variables "Granger cause" manufacturing output. By controlling for past manufacturing output growth, any spurious correlation between manufacturing growth and changes in fiscal variables is presumably prevented. The test focuses on whether variations in state fiscal policies that cannot be predicted by past manufacturing growth lead to changes in current manufacturing growth.[16]

Applying this test reveals that effects of past state fiscal variables on

current manufacturing growth are highly significant. The probability of this correlation occurring by chance is less than 0.5%.[17]

This result is consistent with the only previous attempt to test whether state and local public services "Granger cause" growth: an analysis of the relationship between public and private investment in U.S. metropolitan statistical areas (Eberts and Fogarty 1987). Separate Granger causality tests were applied to each of 40 MSAs. In 21, the authors found evidence that public investment "causes" private manufacturing investment. In only eight MSAs did they find statistically significant evidence that manufacturing investment "caused" public investment.[18]

If the assumptions underlying the approach are valid, instrumental-variable estimates will reveal the true causal relationship between state and local fiscal variables and manufacturing output growth.

The instrumental variable approach tries to deal with the issue of causation by looking only at *some* changes in state and local fiscal variables: those that are likely not to be caused by changes in manufacturing output. These changes in state and local fiscal variables are those predicted by "instrumental variables," which are simply variables that are thought to be good predictors of changes in fiscal variables but are not correlated with manufacturing output growth.

Because the instrumental-variables approach throws away information—only some changes in fiscal variables are examined—the resulting estimates tend to be relatively imprecise. But if the assumptions underlying the approach are valid, instrumental-variable estimates will reveal the true causal relationship between state and local fiscal variables and manufacturing output growth.

Two types of instrumental variables were used for state and local fiscal variables:

Changes in the state political climate.

These variables included changes in political party of the governor, changes in party control of the state legislature, and changes in tax and spending limitations in the state.[19] Such changes in political climate will change state and local fiscal policies, but it is unlikely that state manufacturing output growth will have strong effects on which party controls the state or on whether tax or spending limitations are enacted.[20] Because changes in political climate may take some time to affect fiscal policies, the set of instrumental variables also included two lagged years of changes in these variables. To reflect the intensification of partisan differences in recent years, the analysis allowed for some change over time in the effects of political party control on state and local fiscal policy. Finally, to reflect the more conservative tendencies of Southern Democrats, the analysis allowed party control of the

governorship to have less effect in the South than in other regions.[21]

Changes in federal intergovernmental grants.

For each state, a calculation was made of the change in intergovernmental grants as a percentage of state income that would be likely to occur if both intergovernmental grants and state income had grown at the national average. This variable is larger for states with a greater dependence on intergovernmental grants. This predicted change is positive for much of the late 1960s and early 1970s and negative for much of the late 1970s and early 1980s, reflecting shifts in federal grants policy.[22] Several lagged years in this instrumental variable were also included in predicting changes in state and local fiscal policies.

Overall, the statistical tests are mildly supportive of the notion that state and local fiscal variables have true causal effects on state manufacturing output.

When looking at changes in fiscal variables brought about by political party shifts or intergovernmental grant shifts, the estimated effects of public services on manufacturing output tend to be somewhat greater than in the previous estimates, which looked at all changes in fiscal variables. (The appendix presents the details of the estimates.) These instrumental variable estimates are extremely imprecise, however, so the possibility that the instrumental variable estimates are no different than the original estimates cannot be ruled out.

Overall, the statistical tests are mildly supportive of the notion that state and local fiscal variables have true causal effects on state manufacturing output.

CONCLUSION

This report presents new statistical evidence that state and local public services have significant effects on state manufacturing productivity and output. These effects are large enough that an increase in taxes, if used to finance an increase in public-services spending, is unlikely to have *significant negative* effects upon a state's economic development.

Whether these results imply that state and local governments should spend more on public services is another issue. The estimated effects on a state's manufacturing sector of fiscal policy changes that do not affect the budget's overall balance appear to be small. Whether a given increase in taxes and public-services spending is desirable is likely to be determined by how it affects the household sector, an issue this report leaves to the voters. This report merely provides some reassurance that states can make fiscal policy decisions without fear of the economic-development consequences.

The findings support the belief that spillover effects should be considered in national policy.

Another difficult issue is whether these results provide support for a national policy of encouraging greater state and local public-services spending. The results provide some preliminary evidence of spillover effects of one state's public-services spending on other states. This preliminary finding, from one empirical study, should be regarded as more suggestive than definitive. Other explanations may be offered for this positive correlation between one state's output and its neighbor's public spending. Furthermore, a fuller analysis of this issue would also have to consider spillover effects on the household sector. Finally, all of the public-services spending categories in this report are relatively broad, and national policy is usually targeted at more specific policy areas.

The findings support the belief that spillover effects should be considered in national policy. Any changes in national policy toward public services should consider the national interest and focus on changes that clearly advance it. In a time of limited national financial resources, the national interest in better state and local public services is also likely to be advanced by policies that encourage greater innovation in public-services delivery, better evaluation of public-services quality, and dissemination of the results of those evaluations. These specific national policies have clear spillover benefits.

The results also strongly support the belief that federal intervention is crucial to ensure some minimum level of well-being for the poor. There are strong incentives for a state to improve its economic development by ignor-

ing the poor. Reduced spending on the poor can be used to finance either lower taxes or higher public-services spending, depending on the political philosophy of a particular state leadership, and either approach will encourage that state's own economic development. Social justice is as much a national goal as economic development, and federal leadership is necessary to ensure that it receives adequate attention.

APPENDIX

This appendix presents this project's empirical work in more detail. **Table A-1** outlines equations estimated for this project that examine the effects of the state and local public sector on private-sector productivity. **Table A-2** outlines equations that examine the effects of the public sector on private-sector output.

Variants of the Munnell Productivity Growth Model

Probably the best known model linking state economic development and the public sector is that of Alicia Munnell and Leah Cooke (1990). Munnell and Cooke's model regresses state output on state labor, state private capital, and state public capital. The coefficient on state public capital is argued to measure the effect of public capital on total factor productivity, since it measures the effect of public capital on output with private capital and labor held constant.

Whether this approach can separate effects on output from effects on total factor productivity is not clear. True capital and labor inputs are probably measured quite inaccurately. Even if these inputs were measured accurately, both are clearly endogenous, which will bias regression estimates. The estimated effect of public capital on output in Munnell-type regressions is unlikely to hold constant the true effects of labor and capital inputs on output. The public capital coefficient represents a combination of two influences of public capital on output: via its effects on capital and labor that are not properly controlled for in the regression, and via its effects on the unmeasured inputs that are part of total factor productivity.[23]

The first column of numbers in **Table A-3** presents results from a "Munnell-style" regression (equation 1 in Table A-1).[24] The regression differs from Munnell in several respects: the dependent variable is the logarithm of total private output in the state, not the logarithm of total output; the source of the state employment data is somewhat different; the model is estimated over the 1969-88 period rather than the 1970-86 period; it includes a complete set of year dummies; and it does not include a control for the state unemployment rate. The results are qualitatively but not quantitatively similar to Munnell's. Public capital is estimated to have a statistically significant effect on output in both cases. However, the estimated coefficient of 0.06 on public capital in this study is less than half of the estimated coefficient of 0.15 obtained by Munnell (Munnell and Cooke 1990, 16, equation 3 of Table 5).

Munnell's specification has been criticized for omitting unobserved "state effects" by Holtz-Eakin (1992) and Eisner (1991). It is plausible that states persistently differ in their level of productivity for reasons that will be difficult to fully explain with observable variables. For example, states may differ in their quality of worker "human capital"; this difference will affect productivity and is difficult to measure.

Omitted state effects in a Munnell-style specification could lead to two problems. First, omitted state effects will result in a correlation between the disturbance terms across years for a given state. OLS estimates of standard errors ignore this correlation. Because this correlation means the observations contain less independent evidence than is assumed by OLS, the OLS standard errors will understate the true standard errors.

Second, omitted state effects on productivity may be correlated with the right-hand variables such as public capital. States with higher productivity levels for unobserved reasons may as a result have higher levels of public capital. Part of the estimated positive effect of public capital in Munnell's

TABLE A-1
Outline of Data and Types of Equations Used to Estimate Effects of State and Local Public Sector on Private-Sector Productivity

Eight main types of equations were estimated. All equations used pooled annual data on 48 states (excluding Alaska and Hawaii) from 1969 through 1988; the unit of observation was the state for a given year, so 960 observations were used in all regressions. Output data and private employment data come from the Regional Economic Information Service, U.S. Bureau of Economic Analysis. Public and private capital data were obtained from Alicia Munnell. Data on state and local fiscal variables were compiled from various issues of *Governmental Finances in [year]*, published by the U.S. Census Bureau; personal income data come from the Regional Economic Information Service.

Equation 1: Munnell-Style Specification

$$\ln(privateGSP) = B0 + (yeareffect) + B1^{*}\ln(privatelabor) + B2^{*}\ln(privatecapital) + B3^{*}\ln(publiccapital)$$

The year effect is a uniform national effect on private GSP, which is captured by a vector of 20 year dummies, one for each year.

Equation 2: Holtz-Eakin/Eisner Style Specification

Same as equation 1, but with addition of state fixed effect on private GSP to right-hand side of equation. This is a vector of 48 dummy variables, one for each state.

Equation 3: Public Capital Breakdown Specification

Same as equation 2, but with substitution for ln(public capital) of three types of public capital:

$$B3h^{*}\ln(highway\ capital) + B3w^{*}(water\ and\ sewer\ capital) + B3o^{*}\ln(all\ other\ public\ capital)$$

Equation 4: State Trend Specification

Same as equation 3, but with addition of state-specific trend to right-hand side of equation. This state-specific trend is equal to a state fixed effect for each state times a time trend variable (1969=0, 1970=1, etc.). In addition, this equation and subsequent specifications drop the all other public capital variable, which is statistically insignificant.

Equation 5: Fiscal Variable Specification

Same as equation 4, but with addition of 10 variables measuring various components of state and local public spending and revenue as a proportion of state personal income:

$$B40^{*}(elementary\ and\ secondary\ education\ spending/personal\ income) + B41^{*}(higher\ education\ spending/personal\ income) + B42^{*}(health\ and\ hospitals\ spending/personal\ income) + B43^{*}(highway\ and\ roads\ spending/personal\ income) + B44^{*}(all\ other\ non\text{-}welfare\ spending/personal\ income) + B45^{*}(property\ taxes/personal\ income) + B46^{*}(all\ other\ taxes/personal\ income) + B47^{*}(fee\ revenue/personal\ income) + B48^{*}(intergovernmental\ revenue\ federal\ governemnt/personal\ income) + B49^{*}(deficit/personal\ income)$$

TABLE A-1 *(cont.)*
Outline of Data and Types of Equations Used to Estimate Effects of State and Local Public Sector on Private-Sector Productivity

The omitted 11th revenue/spending category from this list is state and local welfare spending. Inclusion of all 11 revenue/spending categories would result in perfect collinearity.

All revenue and spending data are for a given fiscal year, while personal income is for that same calendar year. The categories used are the same as the categories with similar names in the publication *Governmental Finances in [year].*

For each of these fiscal variables, both current and 11 lagged years of the fiscal variable were included in the estimating equation. Thus, the earliest fiscal information included, for the observations on manufacturing output in 1969, was for state and local revenue and spending as a proportion of personal income in 1958.

Equation 6: Manufacturing Specification

Same as equation 5, but with substitution of manufacturing output and private input variables for total private sector output and input variables:

$$\ln(\textit{manufacturing gross state product}) + B0 + B1^{*}(\textit{manufacturing employment}) + \ln(\textit{manufacturing private capital}) + \textit{other variables, including two types of public capital, ten fiscal variables, state fixed effects, and state specific time trends}$$

Equation 7: Industry Mix Specification

Same as equation 6, but with addition of two variables to predict state manufacturing output and manufacturing employment:

$$B50^{*}\ln(\textit{predicted state manufacturing output}) + B5e^{*}\ln(\textit{predicted state manufacturing employment})$$

The formulas for these variables are given in the appendix. These predicted variables give state manufacturing output and employment if each manufacturing industry had the same percentage growth rate in output and employment as its national counterpart for each year from 1969 on.

Equation 8: Neighbors Specification

Same as equation 7, but with addition of 12 "neighbor variables" to the specification. Two of the neighbor variables correspond to the two public capital variables; 10 of the neighbor variables correspond to the 10 fiscal variables. Each of these neighbor variables for state A is the simple unweighted average of the corresponding variable for all states that border state A. For example, the variable for neighbor spending on elementary and secondary education is the average over all bordering states of

elementary and secondary education spending/personal income

for each of these bordering states.

TABLE A-2
Outline of Data and Types of Equations Used To Estimate Effects of State and Local Public Sector on Manufacturing Output Growth

Four main types of equations were estimated. All equations used pooled data on year-to-year changes for 48 states (excluding Alaska and Hawaii) from 1969-70 to 1988-89. The unit of observation is the year-to-year change for a given state, so 960 observations are used in the analysis. Output data, personal income data, and earnings data come from the Regional Economic Information Service of the U.S. Bureau of Economic Analysis. Data on state and local fiscal variables were compiled from various issues of *Governmental Finances in [year]*, published by the U.S. Census Bureau. Wage data come from various editions of the *Handbook of Labor Statistics*, published by the U.S. Bureau of Labor Statistics. Data on state party control of the governorship and each house of the state legislature come from various editions of the *U.S. Statistical Abstract*. Data on state and local tax and expenditure limitations come from Preston and Ichniowski (1991), Howard (1989), Gold (1983), Merriman (1987), Hutchinson and James (1988), Gold and Fabricius (1989), and U.S. Advisory Commission on Intergovernmental Relations (1977).

Equation 1: Basic Manufacturing Growth Specification

$$\ln(\textit{manufacturing output in year } t) - \ln(\textit{manufacturing output in year } t\text{-}1) = B0 + (\textit{year effect}) + (\textit{state fixed effect}) + B1^{*}(\textit{predicted manufacturing growth based on state industry mix}) + B2^{*}(\textit{lagged value of change from year to year in some state and local revenue or spending category as a proportion of state personal income})$$

Year effect is uniform national effect on output growth, represented by vector of 20 dummy variables, one for each year. State fixed effect is uniform over time effect on state output growth, represented by vector of 48 dummy variables, one for each state.

Predicted manufacturing growth is percentage growth in state manufacturing output if each manufacturing industry in state had grown at same percentage rate as nation from year *t-1* to year *t* (see appendix text for precise definition). In addition to values of this variable for year *t-1* to year *t*, equation includes 11 lags in this variable. Thus, because observations on the dependent variable begin with 1969-70, data on predicted manufacturing output growth must be obtained from 1958-59 on. For years prior to 1963, state manufacturing GSP data to construct this predicted growth variable are not available. Instead, an analogous predicted growth variable was constructed for manufacturing earnings growth for each year from 1958-59 to 1988-89. The predicted manufacturing GSP growth variable for years 1958-59 through 1962-63 is estimated by regressing predicted manufacturing GSP growth on predicted manufacturing earnings growth for the years for which these two variables overlap, 1963-64 through 1988-89.

The state fiscal variables are the same 10 fiscal variables used in the productivity equations (Table A-1): elementary and secondary education spending; higher education spending; health spending; highways spending; other nonwelfare spending; property tax revenue; other tax revenue; fee revenue; intergovernmental revenue; deficit.The omitted 11th spending/revenue category, to avoid perfect collinearity, is welfare spending. A typical change variable is defined as the change from year *s-1* to year *s* in the proportion of state personal income that is either spent or raised as revenue in that category. For example, the highway spending variable for the change from year *s-1* to year *s* would be defined as

$$[(\textit{highway spending in year } s/\textit{personal income in year } s) - (\textit{highway spending in year } s\text{-}1/\textit{personal income in year } s\text{-}1)]$$

The equation to explain state manufacturing growth from year *t-1* to year *t* includes values of these state fiscal variables lagged one year, plus 10 additional lags; current values of changes in these fiscal variables are excluded. Thus, the changes in these state fiscal variables would start with the change from year *t-2* to year *t-1*, and go back to the change from year *t-12* to year *t-11*.

TABLE A-2 *(cont.)*
Outline of Data and Types of Equations Used To Estimate Effects of State and Local Public Sector on Manufacturing Output Growth

Equation 2: Augmented Manufacturing Growth Specification

Same as equation 1, but with addition of 11 variables for the lagged percentage growth in state manufacturing GSP, 11 variables for the lagged percentage change in state manufacturing wages, and 11 variables for the lagged percentage change in state personal income. Each percentage change from year *s-1* to year *s* is defined as the change in the natural logarithm of that particular variable. The specification starts all variables with a one-year lag behind the dependent variable, and includes up to 11 lags in each variable. For example, to explain the change in the logarithm of state manufacturing GSP from year *t-1* to year *t*, the specification includes the change in the logarithm of state manufacturing wages starting with the change from year *t-2* to year *t-1*, and goes back to the change from year *t-12* to year *t-11*.

Equation 3: Instrumental Variable Specification

Same as equation 1, but three broader fiscal variables are substituted for the 10 fiscal variables. In addition, instead of including from one to 11 lags in each fiscal variable, this specification includes from one to four lags. The three broader fiscal variables are the change from year *s-1* to year *s* in the following:

(total own source state and local revenue, including taxes, fees, and deficits/personal income)
(intergovernmental revenue from federal government/personal income)
(all non-welfare state and local public spending/personal income)

The omitted fiscal category to avoid perfect collinearity is still the change in the proportion of state personal income spent on welfare programs.

This equation is estimated both by OLS and by instrumental variables. In the instrumental variable procedure, these three fiscal variables are treated as endogenous. The change in each of these fiscal variables is regressed on the following variables: year dummies; state dummies; current and two lags in the predicted manufacturing GSP growth variable; current and two lagged values of the change from one year to the next in a dummy variable for Democratic control of the governorship; current and two lagged values of the change from one year to the next in a dummy variable for Democratic control of the upper house of the state legislature; current and two lagged values of the change from one year to the next in a dummy variable for Democratic control of the lower house of the state legislature; an interaction between all these changes in Democratic control variables and a time trend; an interaction between the gubernatorial control variable and a dummy variable for the South; current and two lagged values of changes in a dummy variable for whether the state has some type of tax expenditure limitation that limits the *level* of state taxes or spending; current and two lagged values of a dummy variable for whether the state has some type of tax expenditure limitation that limits the year-to-year *growth* in state taxes or spending; current and two lagged values of whether the state has some type of limitation on property tax levy growth or assessment increases; current and two lagged values in the predicted change in the state's intergovernmental revenue as a proportion of personal income if the state's intergovernmental revenue and personal income from year *s-1* to year *s* both grew at the same percentage rate as was true in the nation.

Fitted values of the fiscal variables were retrieved from this regression, and first through fourth lagged values of these fitted values were substituted for the actual values in the equation explaining manufacturing growth. This manufacturing growth equation was re-estimated by OLS using these fitted values, and the standard errors from this output were then corrected using the resulting coefficients and the actual values of the fiscal variables to recalculate a new sum of squared residuals. This somewhat involved procedure was needed to ensure that a given change from year *s-1* to year *s* in one of the fiscal variables was only predicted with instruments from year *s* and before. A conventional 2SLS computer package would have used all instruments to predict all endogenous variables, which would have resulted in using the change from year *t-2* to year *t-1* in one of these instruments to predict the change from year *t-5* to year *t-4* in one of the fiscal variables.

TABLE A-2 *(cont.)*
Outline of Data and Types of Equations Used To Estimate Effects of State and Local Public Sector on Manufacturing Output Growth

Equation 4: Neighbors' Specification

Same as equation 1, but with corresponding fiscal variables for the states' neighbors. Each "neighbor" fiscal variable for a state A is defined as the change from year *s-1* to year *s* in the simple average over all states bordering state A of some revenue and spending category as a proportion of state personal income. The same 10 fiscal categories are used for these neighbor variables as are used for the state itself. In addition, for each fiscal category there are 11 lagged values of these changes in each of the neighbor fiscal variables—from once lagged to the 11th lag. That is, to explain a state's manufacturing growth from year *t-1* to year *t*, we include the change from year *t-2* to year *t-1* in the average over its bordering states in each fiscal variable, the change from year *t-3* to year *t-2*, up to the change from year *t-12* to year *t-11*.

specification could reflect this reverse causation from productivity levels to public capital.

Holtz-Eakin and Eisner correct for this problem by adding state-specific fixed effects to the Munnell specification.[25] The second column of numbers in Table A-3 reports the results of adding state fixed effects to this report's model and data (equation 2 in Table A-1). These results are similar to those of Holtz-Eakin (1992) and Eisner (1991): adding state fixed effects to a "productivity" regression eliminates the estimated positive effects of total public capital on productivity.

These specifications could be criticized for assuming that all types of public capital have the same effects on productivity. Much of public capital consists of government administration buildings and other capital that probably have negligible positive effects on private-sector productivity. Government buildings may even depress productivity if the taxes used to finance these buildings adversely affect productivity.

The third column of numbers in Table A-3 modifies the specification by breaking down public capital into three types: highway capital, water and sewer capital, and all other public capital (equation 3 of Table A-1). State fixed effects are still included in the specification. Highway capital and water and sewer capital are estimated to have positive effects on productivity levels that are of at least marginal statistical significance. All other public capital is estimated to have significantly negative effects on productivity. These results are qualitatively similar to those obtained by Eisner using the same breakdown of types of public capital (Eisner 1991, Table 6B).

Another plausible modification to the Munnell specification is to allow for unobserved differences across states in productivity growth *trends*. These differences may occur for two reasons. First, a state with better human capital—workers more able to learn, more entrepreneurs with good business skills, more high-quality researchers—will have higher productivity levels and growth. Not all these differences in human capital will be measured. Second, measured productivity growth in Munnell-style regressions depends on the growth of any inputs that are not adequately controlled for in the regression. Growth of inputs will of course be higher in any state that has recently become more attractive toward that type of business input. But we would expect this adjustment to a new equilibrium allocation across states of business inputs to be quite protracted. Inputs are only imperfectly mobile. In addition, because the productivity of one input depends on the availability of others, growth of a wide variety of inputs in a state can only proceed gradually, in a process of cumulative causation. It would not be surprising for a state to still be adjusting to a new equilibrium

TABLE A-3
State Public Capital and Overall Private-Sector Productivity

	Munnell-Style Regression Coefficients (Eq. 1 of Table A-1)	Holtz-Eakin/ Eisner-Style Regression Coefficients (Eq. 2 of Table A-1)	Coefficients From H-E/E Regression with Different Types of Public Capital (Eq. 3 of Table A-1)	Coefficients From Regression With Both Unobserved Levels and Trends in Productivity (Eq. 4 of Table A-1)	Unobserved Levels and Regression Trends, Dropping All Other Public Capital (Eq. 4 of Table A-1)
ln (Labor)	.647	.732	.772	.975	.975
	(.017)	(.028)	(.028)	(.028)	(.027)
ln (Private Capital)	.368	.422	.402	.324	.324
	(.014)	(.031)	(.030)	(.023)	(.022)
ln (Public Capital)	.061	-.119			
	(.022)	(.026)			
ln (Highway Capital)			.047	-.032	-.032
			(.033)	(.033)	(.033)
ln (Water and Sewer Capital)			.033	.047	.047
			(.018)	(.015)	(.014)
ln (All Other Public Capital)			-.147	-.001	
			(.019)	(.020)	

Notes: Dependent variable is ln(private GSP). Standard errors are in parentheses. All regressions use annual state data on 48 states (excluding Alaska and Hawaii) from 1969 to 1988. All equations contain a complete set of year dummies. Public capital and private capital data come from Alicia Munnell. GSP data is from Bureau of Economic Analysis. The revised GSP data for 1977-89 is linked to the old 1963-86 data by adjusting up all the old data by a uniform percentage amount for each state, so that the ln(private GSP) is the same for 1977 in both data series. The employment data is total full-time and part-time employment from the Regional Economic Information Service (REIS) of the U.S. Bureau of Economic Analysis. The Holtz-Eakin/ Eisner-style regressions include a complete set of dummy variables for states. The F-test statistic for these state dummies is 129.72 (df = 47; 890) for the total public capital case, 127.65 (df = 47; 888) for the case with three types of public capital.

in input levels throughout the entire 20-year period that is included in these regressions. It is unlikely that all state characteristics that affect a state's equilibrium level of business inputs can be fully controlled for.

The last two columns in Table A-3 present the private-sector productivity regression estimates when unobserved state-specific trends in productivity are allowed for in the regression (equation 4 of Table A-1). F-test statistics overwhelmingly indicate that unobserved state trends are statistically significant. The estimated effects of water and sewer public capital on productivity go up in magnitude and statistical significance in this specification. The estimated effects of highway capital and other public capital on productivity become statistically insignificant.

Private Productivity and Other Measures of State and Local Fiscal Policy

"Public capital" is a limiting way of describing state and local fiscal policies. For labor-intensive public services such as education, the correlation between service quality and physical capital is weak. Even for more capital-intensive services such as highways, the available capital measures may not be a good gauge of how these services influence productivity. These capital measures assume that public capital in all states follows a uniform national pattern of depreciation, and that public capital is fully used upon installation.

State and local taxes may also influence measured productivity. Tax rates may affect the incentives to improve productivity through various types of innovations and investment. State and local taxes and public services are difficult to measure. In this study, state and local tax rates are measured as the percentage of state personal income that goes to taxes. This measure roughly approximates average effective tax rates.

Public services were measured as public spending in a particular area as a percentage of state personal income. The key advantage of this measure is that it is consistent with the tax measure used. This consistency allows easy calculation of the effects of a balanced-budget change in state and local fiscal policy.

In addition, dividing spending by state personal income provides a rough control for state economic characteristics that may affect public service quality. State personal income will be positively correlated with state wages and the volume of state production. As state private-sector wages increase, it becomes more difficult with a given amount of public spending to have the same personnel quality. States with larger economies will make greater use of public services (e.g., more trucks and cars will use state roads if the state economy is larger), increasing congestion for a given amount of public spending. Scaling public spending by income provides a better control for these wage and congestion effects than other plausible alternatives, such as scaling public spending by population.

Finally, scaling public spending by income has the appeal of tradition. This specification has been used by a number of researchers examining regional economic development, following the precedent set by Helms' (1985) influential article.

Public spending's effects on economic development would be expected to occur only gradually over time. This assumption is implicit when using public capital as the measure of public services. Measured public capital at any point in time is simply some complex distributed lag function of past public capital investment. Public spending on other public services such as education may also have effects on economic development only after a considerable lag. In the case of education, the quality of educational services may not improve until some years after an increase in funding. After educational quality has improved, it will take some time for the quality of the local labor force to improve. Similar arguments can be made for other types of public spending. Hence, all estimates of the effects of different types of public spending allow for public spending in a given year to have effects on economic development up to 11 years later. Taxes are also allowed to have up to an 11-year lagged effect.

The choice of 11 lagged years of public spending variables is arbitrary. The state and local fiscal data used here are available starting in 1958. Including 11 lags allows 20 observations for each state, from 1969 through 1988, encompassing three business cycles (1969-73, 1973-79, 1979-88). Each lag added eliminates 48 years of observations, one for each state. Adding more than 11 lags would eliminate some observations from the 1969-73 business cycle, which seems undesir-

able. One might consider shortening the lag length to allow for some observations from the 1960s boom period. As will be seen below, however, the effects of state and local fiscal variables seem to be showing some change, even up to 11 years after a given fiscal variable is shifted.

Including both highway public capital and current and lagged highway spending requires some care in interpreting the results. These variables are to some extent separate attempts to measure the same thing. The highway public capital variable assumes that a particular pattern of depreciation will adequately measure the effects of past highway spending on productivity, whereas the highway public spending variables allow current and lagged highway spending to have any pattern of effects over time. There might be some concern that including both variables will result in so much multicollinearity that one cannot determine how highway spending affects a state's economy. As we will see, however, in different specifications either the highway public capital or the highway spending variable tends to dominate the results—one of these variables will be positive and significant, whereas the other will be statistically insignificant. Hence, in practice the data appear to be able to tell us that one of these variables matters to productivity whereas the other does not.

Table A-4 presents results when 11 lags in a set of 10 public revenue and spending variables are included in the private productivity regression. The omitted public spending variable is welfare spending as a proportion of personal income; including all revenue and spending variables would result in perfect collinearity. Individual coefficients should be interpreted as the effects of some tax change or public spending change when the budget balance is kept the same by a corresponding change in welfare spending. The results presented for each fiscal variable are the long-run effects of a tax or spending change after 11 years. The first column presents results from OLS estimation. The second column presents results corrected for serial correlation, which is highly significant even though the regression controls for fixed unobserved state-specific trends in productivity.

Tax, public spending, and public capital variables generally have the expected sign in these two regressions. (One possible exception is the significant negative effects of intergovernmental revenue on productivity, which will be considered below.) The negative effects of the tax variables are generally of the same order of magnitude as the positive effects of the public spending variables. The estimated tax effects tend, however, to have a greater degree of statistical significance.

The third column shows how the results change when state-specific trend controls are dropped from the specification. The results still indicate that taxes and public-services spending have roughly offsetting effects on private-sector productivity. But tax increases or cuts in public-services spending, when used to finance increased welfare spending, are now estimated to increase private-sector productivity. It is difficult to see how increased welfare spending could spur productivity improvements. A more plausible interpretation is that states with generally faster productivity growth as a result have generally faster increases in spending on welfare. Thus, omitting state-specific trends seems to lead to "biased" results if one's goal is to estimate the true causal influences of state fiscal policies on productivity.

Manufacturing Productivity and State and Local Fiscal Policies

As argued in the text, manufacturing output is probably much better measured, particularly at the state level, than overall private-sector output. This better measurement suggests that more reliable results will be obtained by focusing on manufacturing productivity rather than overall private-sector productivity.

The first column of **Table A-5** presents results when the second specification of Table A-4 is

TABLE A-4
Long-Run Effects of State Fiscal Variables on Private-Sector Productivity

	Basic Specification	Corrected for Serial Correlation	Dropping Unobserved Trends
ln(Labor)	.902	995	.903
	(.036)	(.037)	(.033)
ln(Private Capital)	.241	.108	.152
	(.024)	(.020)	(.022)
ln(Highway Capital)	.175	.172	154
	(.052)	(.062)	.(.044)
ln(Water and Sewer Capital)	.026	.016	.058
	(.016)	(.019)	(.020)
Elementary and Secondary Education	3.81	2.54	-7.22
	(1.77)	(1.99)	(1.85)
Higher Education	5.33	4.37	-4.13
	(2.05)	(2.34)	(2.05)
Health	6.74	4.04	-2.63
	(2.63)	(3.07)	(2.07)
Highways	-1.64	-1.91	-9.77
	(1.60)	(1.87)	(1.69)
Other Public Spending	3.71	3.31	-11.74
	(1.71)	(1.96)	(1.63)
Property Taxes	-3.61	-4.17	6.25
	(1.43)	(1.61)	(1.41)
Other Taxes	-6.15	-5.43	4.06
	(1.45)	(1.68)	(1.29)
Fees	-5.22	-4.61	2.56
	(1.62)	(1.88)	(1.67)
Intergovernmental Revenue	-3.63	-3.13	10.88
	(1.31)	(1.51)	(1.17)
Deficit	-7.16	-6.08	5.72
	(1.34)	(1.52)	(1.31)

Notes: The dependent variable is the logarithm of state private sector output, annual observation on 48 states from 1969 to 1988. State and local fiscal spending is measured as a proportion of state personal income. Thus, the coefficients are to be interpreted as the long-run percentage effect on state private output (or private productivity, as labor and private capital are held constant) of an increase in some tax or spending category by 1% of the state's personal income. All specifications include a complete set of state dummies and year dummies. The estimated first-order serial correlation coefficient used in estimating the second specification's results is .598, with a standard error of .030. The first and second specification include fixed state-specific trends in productivity growth (Eq. 5 of Table A-1), while the third specification does not. An F-test of omitting the 11 lags in fiscal variables from the first specification (i.e., testing this specification against the last specification in Table A-3) is 3.54 (df = 120; 722), which is significant. The serial correlation correction coefficient (standard error) for the third specification is .804 (.021).

TABLE A-5
Long-Run Effects of State Fiscal Variables on Manufacturing Productivity

	Basic Specification	With Industry Mix Controls
ln(Manufacturing Labor)	.936 (.045)	.888 (.042)
ln(Manufacturing Capital)	.069 (.021)	.040 (.017)
ln(Highway Capital)	.330 (.126)	.027 (.105)
ln(Water and Sewer Capital)	.031 (.038)	.028 (.031)
Elementary and Secondary Education	8.07 (4.05)	6.88 (3.36)
Higher Education	12.32 (4.79)	10.57 (3.96)
Health	4.28 (6.26)	6.30 (5.19)
Highways	4.67 (3.80)	6.83 (3.15)
Other Public Spending	9.16 (3.98)	7.18 (3.29)
Property Taxes	-6.98 (3.18)	-5.56 (2.63)
Other Taxes	-9.64 (3.41)	-9.94 (2.84)
Fees	-7.85 (3.81)	-8.82 (3.15)
Intergovernmental Revenue	-7.29 (3.07)	-7.09 (2.54)
Deficit	-8.12 (3.09)	-7.67 (2.57)
Predicted Output Mix Control		1.030 (.053)
Predicted Labor Mix ontrol		-.712 (.131)

Notes: Standard errors are in parentheses. Basic specification is described in Eq. 6 in Table A-1; industry mix specification is described as Eq. 7 in Table A-1. The dependent variable in both specifications is the logarithm of state manufacturing output. State fiscal variables are measured as proportions of state personal income. The observations are yearly observations on 48 states (excluding Alaska and Hawaii) from 1969 to 1988. Both specifications control for state effects, state trend effects, year effects, and serial correlation. Estimates reported here for fiscal variables are the long-run cumulative effects after 11 years. The industry mix control estimates are those described in the main text. The basic specification is the same as the second specification used for overall private sector productivity in Table A-4 (i.e., the specification with unobserved trends and corrected for serial correlation), except that the specification here uses manufacturing output, labor, and capital rather than private sector output, labor, and capital.

re-estimated using manufacturing data. The estimated effects of taxes and public-services spending on productivity tend to be larger using manufacturing data. The estimated public service variable effects tend to have greater statistical significance.

All of the above-discussed results could be biased due to a failure to control for industry mix. Productivity levels vary across industries. The mix of industries in a given state may change greatly over time. Some of the estimated effects of fiscal variables on productivity could be due to changes in the mix of industries in a state, not changes in the productivity levels of individual industries.

To control for industry mix, two variables were constructed. The output mix variable is the predicted manufacturing output in the state in year *t* if, for each year from 1969 on, the output of each industry in the state had grown from year *t0* to year *t1* at the national average. The employment mix variable is a similarly constructed measure for employment. Algebraically, predicted manufacturing output or employment growth is expressed as:

(1) *Predicted change in state output or employment year t-1 to year t = sum over all industries of (industry output or employment in state s at year t-1)* (change in industry output or employment in nation from year t-1 to year t, as proportion of industry output or employment in nation at year t-1)*

The mix variable then assumes that predicted manufacturing output or predicted manufacturing employment as of year *t* is equal to:

(2) *Predicted output or employment in year t = state output or employment in 1969, plus sum of predictions from Equation 1 of output or employment change from 1969-70, 1970-71, etc., up to the predicted change from year t-1 to year t*

The regressors used as industry mix controls are the logarithms of these predictions from equation 2. The output mix variable will go up relative to the employment mix variable if the state is attracting industries with high labor productivity. These two variables control only for the effects of industry mix on labor productivity, not total factor productivity.

The hope is that these industry mix variables will control for changes in state manufacturing productivity that occur because the state's industry mix shifts toward or away from high productivity industries. The remaining variables, such as the fiscal variables and public capital variables, will then be focused on explaining why a state's productivity might go up or down holding industry mix constant.

The second column of Table A-5 shows how the results change when these two industry mix variables are added. The two industry mix variables are highly statistically significant and have the expected signs. Including these variables reduces slightly the standard errors in estimating the effects of the fiscal variables. The estimated effects of the public capital variables are greatly reduced in magnitude, and become statistically insignificant as well as substantively small. Results from this second regression are the ones reported for manufacturing productivity in Figures 2, 3, and 5 of the text.

Table A-6 shows the complete set of short-run and long-run effects of the fiscal variables in this "industry mix" specification for manufacturing productivity. These results are also used in Figures 3 and 5 of the text.

Table A-7 uses the variance/covariance matrix of the estimated fiscal variable effects in this

TABLE A-6
Short-Run Versus Long-Run Effects of State Fiscal Variables on Manufacturing Productivity

	Cumulative Effects After:											LR Effect=
	0 years	1 year	2 years	3 years	4 years	5 years	6 years	7 years	8 years	9 years	10 years	11 years
Elementary and Secondary Education	-0.44	0.96	2.24	2.60	3.04	2.99	4.03	4.55	5.04	4.90	5.99	6.88
	(0.86)	(1.17)	(1.40)	(1.60)	(1.79)	(2.00)	(2.22)	(2.44)	(2.64)	(2.87)	(3.12)	(3.36)
Higher Education	0.54	2.33	2.83	3.23	2.68	1.43	2.00	3.40	5.43	6.70	8.52	10.57
	(1.02)	(1.50)	(1.90)	(2.24)	(2.50)	(2.74)	(2.93)	(3.09)	(3.27)	(3.48)	(3.72)	(3.96)
Health	-0.00	1.15	2.19	2.80	3.54	4.76	6.66	9.61	9.02	7.92	6.66	6.30
	(1.10)	(1.49)	(1.82)	(2.13)	(2.43)	(2.78)	(3.18)	(3.54)	(3.92)	(4.37)	(4.81)	(5.19)
Highways	0.97	2.13	3.09	3.43	3.48	2.14	2.97	3.58	4.28	5.22	5.76	6.83
	(0.84)	(1.12)	(1.39)	(1.64)	(1.85)	(2.06)	(2.25)	(2.40)	(2.57)	(2.74)	(2.93)	(3.15)
Other Public Spending	-0.65	0.58	0.92	1.33	1.61	0.90	1.55	2.47	3.73	4.23	5.84	7.18
	(0.79)	(1.08)	(1.31)	(1.53)	(1.73)	(1.94)	(2.14)	(2.36)	(2.58)	(2.80)	(3.04)	(3.29)
Property Taxes	-0.56	-1.98	-3.28	-4.02	-4.18	-3.21	-4.17	-4.23	-4.55	-4.41	-4.96	-5.56
	(0.83)	(1.08)	(1.25)	(1.42)	(1.54)	(1.64)	(1.76)	(1.89)	(2.02)	(2.19)	(2.41)	(2.63)
Other Taxes	-1.01	-1.63	-3.06	-3.62	-4.31	-3.89	-4.71	-5.94	-6.74	-7.22	-8.68	-9.94
	(0.74)	(0.98)	(1.17)	(1.30)	(1.45)	(1.63)	(1.81)	(1.97)	(2.16)	(2.36)	(2.60)	(2.84)
Fees	-0.63	-1.90	-2.26	-2.84	-3.28	-3.25	-3.72	-5.14	-7.08	-6.33	-7.60	-8.82
	(0.76)	(1.02)	(1.23)	(1.44)	(1.62)	(1.82)	(2.01)	(2.23)	(2.43)	(2.64)	(2.90)	(3.15)
Intergovernmental Revenue	0.10	-0.77	-0.80	-1.32	-1.90	-1.07	-2.42	-3.68	-5.02	-5.94	-6.70	-7.09
	(0.72)	(0.97)	(1.16)	(1.33)	(1.49)	(1.65)	(1.81)	(1.97)	(2.10)	(2.23)	(2.38)	(2.54)
Deficit	0.08	-1.34	-2.19	-2.50	-2.70	-1.99	-2.68	-3.39	-4.56	-5.06	-6.47	-7.67
	(0.70)	(0.92)	(1.07)	(1.20)	(1.33)	(1.48)	(1.64)	(1.81)	(1.97)	(2.15)	(2.35)	(2.57)

Notes: These are the complete short-run and long-run effects of fiscal variables on manufacturing productivity for the second specification in Table A-5. Some of these effects are shown in Figures 3 and 5 of the main text.

TABLE A-7
Estimated Long-Run Net Effects of Tax-Financed Increases in Public-Services Spending on Manufacturing Productivity

Increase in Spending of 1% of State Personal Income on:	Financed by Increase in: Property Taxes	Non-Property Taxes
Elementary and Secondary Education	1.32	3.06
	(1.95)	-(2.06)
Higher Education	5.01	.63
	(3.56)	(3.23)
Health	1.27	-3.11
	(3.18)	(3.12)
Highways	.74	-3.64
	(4.63)	(4.50)
Other Public Services	1.62	-2.76
	(2.36)	(2.19)

Notes: Standard errors are in parentheses. Estimates come from same regression reported in Table A-6 and second column of Table A-4. Estimates show net percentage effect on state manufacturing productivity of an increse in state and local public spending of some type of 1% of state personal income, financed by an increase in some type of state or local tax of 1% of state personal income.

regression to calculate the net long-run effects of various balanced-budget increases in both taxes and public spending. None of the estimated net effects is statistically significant at conventional levels of significance.

Table A-8 adds to this specification the average level of fiscal variables and public capital variables in all neighboring states. These neighbor variables are calculated as the simple average of the fiscal variables and the public capital variables for all states that border the state. For example, if state X was bordered only by states A, B, and C, and a given state and local spending variable was 0.03 of personal income in state A, 0.04 of personal income in state B, and 0.05 of personal income in state C, the neighbor value for that variable for state X would be 0.04, regardless of the relative size of states A, B, and C, the length of the common borders between state X and these three states, or how close the population centers of state X were to the population centers of states A, B, and C. The only argument for ignoring all these factors is simplicity.

The estimated coefficients of these "neighbor" fiscal variables are generally greater than for the corresponding own-state variables. In interpreting the relative size of the neighbor fiscal variables and the own-state fiscal variables, it should be recognized that most states will have four or more neighbors. Even though the neighbor coefficients tend to be larger, a given dollar-sized change in a fiscal variable, for an average state surrounded by average-sized neighbors, will tend to have larger own-state effects than neighbor effects.

TABLE A-8
Estimated Long-Run Effects of Own and Neighbor Fiscal and Public Capital Variables on Manufacturing Productivity

	Effects of a State's Own Public Sector on Its Manufacturing Productivity	Spillover Effects of a State's Neighbors' Public Sector on a State's Manufacturing Productivity
ln(Highway Capital)	-.127	.036
	(.118)	(.263)
ln(Water and Sewer Capital)	-.015	-.004
	(.034)	(.081)
Elementary and Secondary Education	7.75	18.30
	(3.72)	(8.57)
Higher Education	10.39	4.35
	(4.38)	(9.49)
Health	7.22	19.15
	(5.82)	(14.13)
Highways	8.69	22.36
	(3.59)	(8.44)
Other Public Services Spending	11.56	25.73
	(3.65)	(9.10)
Property Taxes	-9.40	-13.79
	(3.18)	(7.05)
Other Taxes	-11.61	-11.74
	(3.08)	(7.35)
Fees	-11.59	-27.61
	(3.39)	(8.97)
Intergovernmental Revenue	-7.52	-21.39
	(3.01)	(6.59)
Deficit	-10.93	-21.40
	(2.89)	(6.81)

Notes: Standard errors are in parentheses. All estimates in this table come from one regression, described as Eq. 8 in Table A-1. The dependent variable is the logarithm of manufacturing output. Control variables include: state dummies, state trend effects, year dummies, state manufacturing labor and capital, and predicted state manufacturing output and labor based on its industry mix. Estimates control for serial correlation. The fiscal variables are measured as a proportion of the personal income of the state or states that are doing the spending, taxing, etc. The neighbors' variables are single averages of these proportions over all bordering states.

The neighbor public capital variables have effects that are both statistically insignificant and substantively small. Apparently the restrictions imposed in constructing these capital variables do not well represent the long-run spillover effects of a state's spending on infrastructure on its neighbors.

Compared to other variables, the spillover effects of the two tax variables are relatively modest compared to their own state effects. As discussed in the text, fiscal variables may affect neighboring states due to three types of spillover effects: demand spillovers, incentive spillovers, and public good spillovers. Public good spillover effects will tend to increase the positive spillover effects of public spending variables, but they should not be important for tax variables.

Higher education neighbor effects are also relatively modest. This finding could still be consistent with strong *national* effects of a state's higher education spending if college-educated labor is so mobile that most spillover effects occur outside of the bordering states.

Table A-9 uses the coefficients and variance/covariance matrix from this regression to estimate the net effects of simultaneous tax-financed increases in public spending in a state and all its neighbors. The net effects of this package are generally not statistically significantly different from zero. The exception is for tax increases used to finance the catch-all category of "other public services"; this overall package is estimated to have statistically significant net positive effects.

Manufacturing Output Growth and State and Local Fiscal Policies

As argued in the text, there are advantages to focusing the empirical investigation on manufacturing output growth rather than productivity growth. Output growth is easier to measure than productivity growth. A positive effect of public services on state manufacturing output growth is a good sign of a positive effect of public services on productivity; manufacturers serve a national market, so a state's cost structure is the key variable affecting its attractiveness to manufacturers. In addition, the effects of public services in attracting labor to a state will be captured by an output measure, but not by a productivity measure. Finally, from the perspective of state and local policy makers, the effects of fiscal policies on business output are at least as important as their effects on business productivity.

Table A-2 summarizes the various manufacturing output growth equations that were estimated in the project and that are discussed in the text or this appendix.

Table **A-10** presents the full set of results for the basic manufacturing output growth equation that was estimated. It is this basic manufacturing output growth equation that is extensively summarized in the text and in Figures 2, 4, and 6.

This manufacturing output growth equation is quite similar in spirit to a first-differenced version of the final estimated manufacturing productivity level model. Manufacturing output growth is regressed on a set of lagged changes in state and local revenue and spending variables, while in the productivity levels equation levels of fiscal variables were used. Public capital variables are dropped from the output growth specification because of the more limited time span over which observations on the public capital variables are available, and because the public capital variables were statistically insignificant in the manufacturing productivity levels equation. The control for unobserved state trends in productivity growth becomes a set of state fixed effects in the output growth equation. The industry mix control is the shift-share predictor of the proportional growth in manufacturing output from year $t-1$ to t if the state's manufacturing industries had all grown at the national average percentage growth for each particular industry, or

TABLE A-9
Estimated Long-Run Effects on Manufacturing Productivity of Tax-Financed Increases in Public Service Spending in a State and All Its Neighbors

	Property Tax-Financed Increase In:				
	Elementary and Secondary Education	Higher Education	Health	Highways	Other Public Services
Net Effect of Own State's Fiscal Package	-1.65	.99	-2.18	-.71	2.16
	(2.18)	(3.90)	(5.25)	(3.65)	(2.71)
Net Effect of Neighboring States' Fiscal Packages	4.51	-9.44	5.36	8.57	11.94
	(4.47)	(8.25)	(12.34)	(8.60)	(6.77)
Net Overall Effect	2.86	-8.45	3.18	7.86	14.10
	(5.10)	(9.51)	(13.68)	(10.37)	(7.70)
	Non-Property Tax-Financed Increase In:				
	Elementary and Secondary Education	Higher Education	Health	Highways	Other Public Services
Net Effects of Own State's Fiscal Package	-3.86	-1.22	-4.39	-2.92	-.05
	(2.25)	(3.50)	(5.04)	(3.47)	(2.43)
Net Effects of Neighboring States' Fiscal Policies	6.56	-7.39	7.41	10.62	13.99
	(5.06)	(8.65)	(11.80)	(8.39)	(6.79)
Net Overall Effect	2.70	-8.61	3.02	7.70	13.94
	(5.54)	(9.52)	(13.18)	(9.87)	(7.61)

Notes: Each estimate shows long-run net percentage effect on state manufacturing productivity of some tax spending package that simultaneously increases some type of tax and some type of public spending by 1% of state income. All estimates are from regressions reported in Table A-8. Standard errors of estimated net effects are in parentheses.

(3) *Predicted proportional growth in state output from year t-1 to year t = sum over all industries of (proportion of state's output in industry as of year t-1)* (change in industry's output in nation year t-1 to year t, as proportion of industry's output in nation in year t-1)*

Lags in this predicted manufacturing output growth are also included.

TABLE A-10
Short-Run and Long-Run Effects of State Fiscal Variables on Manufacturing Output

	Cumulative Effects After:										LR Effect =
	1 year	2 years	3 years	4 years	5 years	6 years	7 years	8 years	9 years	10 years	11 years
Elementary and Secondary Education	1.43	3.19	4.32	5.66	6.35	8.47	10.51	12.26	12.24	13.12	12.69
	(0.95)	(1.43)	(1.83)	(2.22)	(2.59)	(2.99)	(3.38)	(3.74)	(4.09)	(4.42)	(4.76)
Higher Education	1.35	2.35	3.85	5.33	5.45	7.71	10.94	15.19	16.11	17.09	19.22
	(1.16)	(1.84)	(2.39)	(2.90)	(3.32)	(3.73)	(4.13)	(4.49)	(4.83)	(5.19)	(5.55)
Health	1.31	3.18	4.08	7.68	10.80	15.16	20.12	22.78	23.06	23.50	23.96
	(1.30)	(1.99)	(2.61)	(3.20)	(3.76)	(4.39)	(5.08)	(5.72)	(6.38)	(6.98)	(7.52)
Highways	1.05	2.53	2.48	2.39	0.98	2.70	5.06	6.49	7.07	7.17	7.47
	(0.92)	(1.33)	(1.69)	(2.05)	(2.37)	(2.70)	(3.00)	(3.28)	(3.57)	(3.83)	(4.09)
Other Public Spending	1.55	3.25	4.33	6.25	6.52	8.75	11.34	13.65	13.95	15.40	16.12
	(0.86)	(1.28)	(1.67)	(2.09)	(2.49)	(2.90)	(3.30)	(3.70)	(4.07)	(4.42)	(4.74)
Property Taxes	-2.04	-3.94	-5.22	-6.45	-5.79	-8.42	-9.34	-10.98	-10.96	-11.13	-10.95
	(0.91)	(1.32)	(1.66)	(1.98)	(2.26)	(2.55)	(2.82)	(3.10)	(3.35)	(3.61)	(3.91)
Other Taxes	-1.43	-3.88	-5.14	-6.87	-7.57	-10.35	-12.92	-14.78	-15.34	-16.53	-17.46
	(0.83)	(1.20)	(1.53)	(1.85)	(2.16)	(2.49)	(2.84)	(3.16)	(3.48)	(3.79)	(4.12)
Fees	-1.64	-3.41	-4.81	-6.74	-7.40	-8.81	-11.58	-14.11	-12.10	-12.73	-13.19
	(0.85)	(1.28)	(1.64)	(2.04)	(2.41)	(2.79)	(3.18)	(3.56)	(3.91)	(4.25)	(4.60)
Intergovernmental Revenue	-1.41	-2.24	-2.31	-2.51	-0.67	-2.27	-4.79	-6.45	-7.14	-7.63	-7.59
	(0.82)	(1.23)	(1.57)	(1.93)	(2.25)	(2.57)	(2.89)	(3.20)	(3.45)	(3.68)	(3.90)
Deficit	-1.87	-3.97	-4.93	-6.45	-6.91	-9.49	-11.85	-14.08	-14.56	-15.83	-16.39
	(0.78)	(1.12)	(1.41)	(1.70)	(1.97)	(2.26)	(2.57)	(2.87)	(3.16)	(3.44)	(3.73)

Notes: Standard errors are in parentheses. Specification is described as Eq. 1 in Table A-2. This specification's results are also summarized in Figures 2, 4, and 6 of text.

One difference between the manufacturing output growth specifications and the productivity levels specification is that the output growth specification starts all fiscal variables with a one-year lag, omitting current period changes in all fiscal variables. This is done to limit from the outset the potential problems due to endogeneity of fiscal variables. The fiscal variables are equal to the change in the ratio of revenue and spending to income, which will be affected by manufacturing output growth. It is assumed that the immediate effect of the fiscal variables on manufacturing output growth is small enough that omitting the current period change in fiscal variables from the specification will not miss much of the long-run effect of a change in these variables.

This basic manufacturing growth equation is meant to be a reduced-form equation showing how state and local fiscal variables affect manufacturing output. Hence, such potentially significant variables as state wage rates are excluded because wages are in part endogenously determined by a state's economic development performance. Furthermore, permanent wage differences across states are to some extent absorbed by the state fixed effects. Still, it is a relevant issue whether these state and local fiscal effects on growth persist when controls for wages are included. In addition, we want to know whether these effects persist after controls for past output growth and income growth are included. Both output growth and income growth would affect the values of the fiscal variables.

Table **A-11** reports the estimated long-run effects when the equation includes controls for past wage growth, past output growth, and past income growth. Even after controlling for these effects, state and local fiscal variables still have significant effects. An F-test on the joint significance of these fiscal variables gives a value of 1.432 (df=110; 738), which is significant at the 5% level. This is the "Granger causality" test that is mentioned in the text.[26]

The other way to avoid endogeneity problems is to use instrumental variables. The discussion

TABLE A-11
Long-Run Coefficients of Fiscal Variables in Manufacturing Output Equation, With Controls for Past Growth, Wages, and Personal Income

Elementary and Secondary Education	9.79	(4.75)
Higher Education	9.59	(5.60)
Health	16.06	(7.62)
Highways	6.81	(4.25)
Other Public Spending	12.76	(4.74)
Property Taxes	-12.65	(4.11)
Other Taxes	-12.20	(4.18)
Fees	-7.90	(4.75)
Intergovernmental Revenue	-12.57	(4.03)
Deficit	-14.02	(3.83)
Wages	-1.28	(0.34)
Personal Income	-0.65	(0.28)
Past Growth	-0.07	(0.13)

Notes: Standard errors are in parentheses. This specification is described in equation 2 in Table A-2. All coefficients are the "long-run" coefficients, i.e., the sum of the coefficients over all 11 lags of each variable. The F-test on dropping the fiscal variables from this specification is 1.432 (df = 110; 738), which is significant at the .00428 level.

of equation 3 in Table A-2 lists the instrumental variables that were used to try to predict "exogenous" changes in fiscal variables. The rationale for these political instrumental variables is described in the text.

Because instrumental variable estimation throws out a great deal of information, it is inherently more imprecise than OLS estimates. To limit the imprecisions, the IV specification was limited to four types of fiscal variables and only four lagged years of fiscal variables and only four lagged years of fiscal variable changes. Preliminary estimates with more complete models yielded extremely imprecise estimates.

Table A-12 presents the IV-estimated long-run fiscal effects, and, for comparison, estimation of a similar specification using OLS. Hausman tests of the overall specification and of the long-run effect did not reveal any significant differences between these IV estimates and the OLS estimates.

Finally, **Table A-13** presents the full set of long-run fiscal effects when the reduced form model is once again estimated by OLS, but with neighbor fiscal variables added. **Table A-14** uses these results to calculate the net effects on a state's manufacturing output of a state and all its neighbors simultaneously adopting various packages of tax-financed increases in public services.

TABLE A-12
Instrumental Variable Estimation and OLS Estimation of the Long-Run Effects of State Fiscal Variables on Manufacturing Output

	IV Estimates	OLS Estimates
Nonwelfare Spending	24.12	3.15
	(16.32)	(1.67)
Own Source Revenue	-24.82	-4.71
	(14.52)	(1.58)
Intergovernmental Revenue	-19.38	-2.64
	(13.81)	(1.72)

Notes: Standard errors are in parentheses. This specification is described as Eq. 3 in Table A-2. The long-run effects reported here are based on a 4-lag specification. A Hausman test of the IV vs. OLS specification yielded an F-test statistic of .68 (df = 12; 857), which is clearly insignificant.

Response to Possible Objections to Models Used in This Report

Several possible objections can be made against the models used in this report. All of these objections ultimately come down to problems caused by the possible endogeneity of the state fiscal variables.

Objection 1: Models with fixed state effects may be biased if there are relatively short panels of data on each state. The econometric literature (Nickell 1981; Holtz-Eakin, Newey, and Rosen 1988; Holtz-Eakin 1992) indicates that panel data models that assume fixed effects may be biased

TABLE A-13
Estimated Long-Run Effects of Own and Neighbor Fiscal Variables on a State's Manufacturing Output

	Effects of a State's Own Public Sector	Effects of a State's Neighbors' Public Sector
Elementary and Secondary Education	12.99	32.97
	(5.13)	(11.23)
Higher Education	15.52	29.18
	(5.99)	(11.95)
Health	19.28	50.37
	(7.96)	(17.56)
Highways	10.46	40.50
	(4.29)	(9.02)
Other Public Services Spending	18.35	41.80
	(5.06)	(11.86)
Property Taxes	-12.73	-20.01
	(4.39)	(9.11)
Other Taxes	-16.49	-33.26
	(4.38)	(9.26)
Fees	-15.25	-59.59
	(4.82)	(11.61)
Intergovernmental Revenue	-9.09	-25.74
	(4.18)	(8.95)
Deficit	-17.72	-39.61
	(4.02)	(8.76)

Notes: Standard errors are in parentheses. All estimates come from one specification, described as Eq. 4 in Table A-2.

if the panel is short and some of the right-hand-side variables are not strictly exogenous to the entire time series of observations. The problem can be seen by writing the fixed effect models as deviations from time period means:

(4) $$y_{iit} - y_{i.} = \sum_j B_j(X_{ijt} - X_{ij.}) + \in_{it} - \in_{i.}$$

where:

y_{it} = some dependent variable for state i in year t;

$y_{i.}$ = the time period mean for that dependent variable for state i;

TABLE A-14
Estimated Long-Run Net Effects on Manufacturing Output of Tax-Financed Increases in Public-Services Spending, in a State and All Its Neighbors

	Property Tax-Financed Increase In:				
	Elementary and Secondary Education	Higher Education	Health	Highways	Other Public Services
Net Effect of State's Own Fiscal Package	0.26 (3.18)	2.79 (5.07)	6.55 (7.18)	-2.26 (4.25)	5.62 (3.56)
Net Effect of Neighboring State's Fiscal Package	12.96 (6.11)	9.17 (9.84)	30.36 (14.91)	20.49 (9.17)	21.79 (8.50)
Net Overall Effect	13.22 (6.82)	11.96 (10.79)	36.91 (15.95)	18.23 (10.86)	27.41 (9.43)
	Non-Property Tax-Financed Increase In:				
	Elementary and Secondary Education	Higher Education	Health	Highways	Other Public Services
Net Effects of State's Own Fiscal Package	-3.50 (3.24)	-0.97 (4.59)	2.79 (7.03)	-6.03 (3.84)	1.86 (3.31)
Net Effects of Neighboring States' Fiscal Package	-0.29 (6.69)	-4.08 (10.63)	17.11 (15.07)	7.24 (8.38)	8.54 (8.48)
Net Overall Effect	-3.79 (7.51)	-5.05 (11.41)	19.90 (16.33)	1.21 (9.43)	10.40 (9.84)

Notes: Standard errors are in parentheses. All estimates come from regression reported in Table A-13 and described as Eq. 4 in Table A-2. Each estimate reports long-run percentage effect on manufacturing output of simultaneously increasing some revenue category and some spending category by 1% of state personal income.

X_{ijt} = some independent variable j for state i at time period t;

$X_{ij.}$ = the time period mean for state i for variable j;

e_{it} = the disturbance term from the underlying regression equation for state i at year t;

$e_{i.}$ = the mean over all years of the disturbance term for state i.

For estimates of this equation (4) to be consistent as the number of states increases, holding the number of years constant, X_{ijt} must be uncorrelated with $e_{i.}$. This will be the case if the number of years is large, as $e_{i.}$ will then be zero. This will also be the case if the variable X_{ijt} is not only uncorrelated with the current period disturbance, e_{it}, but also uncorrelated with all past and future values of the disturbance for that state.

In the present model, the number of time period observations is actually modestly large—20 years of data. This is much greater than the five or 10 time periods of observations that are assumed in the econometric discussions of this issue. For 20 years of data, it is not unreasonable to assume that in fact the time period mean of the disturbance term will likely be close to zero, which will make the problem small.

In addition, some of the proposed solutions to this problem seem likely to create even greater problems in the models used in this report. For example, Holtz-Eakin (1992) and others suggest considering first differences in all the y_{it} and X_{ijt} variables to eliminate the fixed effects, and then using lagged values of the X_{ijt} variables as instruments to deal with problems caused by serial correlation. But in the present model, using lagged values of the fiscal variables as instruments requires assuming that the effect of fiscal variables does not change after some arbitrarily chosen time period. The estimates in this report suggest that this is a questionable assumption.

Objection 2: The estimated negative effects of the intergovernmental grants variables on productivity and output growth indicate some problems with the model. A zero coefficient might be expected on the intergovernmental grants variable, under the assumptions that intergovernmental grants and welfare expenditures are both exogenous to state productivity and output growth and that they have no strong independent effects on state productivity and output. The estimated negative effects on the intergovernmental grants variable indicates that these assumptions may be partly incorrect.

One possibility is that welfare expenditure and intergovernmental grants are endogenous. States doing worse off will tend to have greater welfare expenditures, thus increasing intergovernmental grants under federal matching formulas.

A second possibility is that welfare expenditure actually does have directly negative effects on state productivity and output. These negative effects might occur due to negative effects of welfare spending on work effort and labor market efficiency.

A third possibility is that intergovernmental grants actually have negative effects on state productivity and output. These might occur if intergovernmental grants, with their regulations and matching requirements, have significant effects in reducing the efficiency of state and local government operations.

Of these three possibilities, the first possibility, that welfare expenditures and intergovernmental grants are endogenous, seems the most likely. Although this endogeneity will bias the coefficient on the intergovernmental grants variable, it is not obvious that the coefficients on the other fiscal variables will be biased.

In the results that try to correct for endogeneity (Table A-12), the effects of own-source revenue and nonwelfare spending seem if anything to be greater than the results when estimated by OLS. Although these instrumental variable results are imprecise, the data do not clearly indicate that the own-source revenue and nonwelfare spending coefficients in OLS are biased toward finding effects on manufacturing output that are too large.

Objection 3: The effects of neighbor fiscal variables are too large to be plausible and may indicate bias due to common trends in the economies and fiscal policies of neighboring states.

As mentioned above, the effects of the neighbor fiscal variables are not so large once one realizes the average state has at least four neighbors. The effects of neighbor-state fiscal variables, after being divided by four or five, are smaller than the effects of the own-state fiscal variables.

Neighbor-state fiscal variables' estimated coefficients could be biased if these fiscal variables are endogenous. Perhaps it is actually the economy of the nearby states that is causing changes both in the own-state economy and in the nearby states' fiscal variables.

Unravelling the exact direction of causation here is a difficult task. This issue is not directly examined in this report because the initial attempts to correct for endogeneity using instrumental variables did not suggest that this approach would yield very precise estimates if extended to the neighboring states' fiscal variables.

The estimates presented in this report are, to the author's knowledge, the first estimates to consider the relationship between a state's economic fortunes and its neighbors' fiscal policies. At the least these results show that the relationship is strong, regardless of the questions that remain about the direction of causation.

ENDNOTES

1. There are, of course, other "laboratories" that allow us to get beyond the limitations of aggregate national data on one nation. For example, some studies have looked at public capital's effects on productivity across industries (e.g., Nadiri and Manuneas 1991), and others have looked at public services and productivity across countries (e.g., Ram 1986).

2. The deflator used is the official state and local government deflator used in the U.S. national income accounts. This deflator basically assumes zero labor productivity improvements in the state and local government sector, with the exception of some improvement due to hiring of more-educated teachers. Some productivity improvement in the use of nonlabor inputs is allowed for. This deflator may overstate cost increases in the state and local government sector because in some cases state and local governments have probably figured out creative ways to improve the productivity of public services. But this deflator may also understate costs increases in that it ignores the social problems that may have increased costs in the state and local government sector.

3. As shown in the figure, in the 1960s, real *nonwelfare* state and local public spending as a percentage of real GDP actually exceeded *total* nominal state and local public spending as a percentage of nominal GDP. This may seem impossible, but is not. The real numbers essentially ask what would have happened if all purchases—by state and local governments, consumers, businesses, etc.—occurred at 1987 prices, whereas the nominal numbers use each year's own prices. Spending on the various categories of goods at 1987 prices need not add up to total spending at 1960s prices.

4. Some studies seek to measure productivity by using output as a dependent variable and by including private capital and labor on the right hand side. Such a regression has the problem that private capital and labor are both clearly endogenous, which will tend to bias the coefficients on these variables. A few studies, such as Hulten and Schwab (1991), measure productivity as the residual once the factor share-weighted sums of private capital and labor are subtracted from private output. Such a procedure assumes that all the assumptions about factors being paid their marginal products are still valid even when every variable is measured at a quite aggregate level. Finally, all productivity studies that focus on total factor productivity must rely on measures of private-sector capital. Such measures are typically constructed by information on past private-sector investment and quite arbitrary assumptions about patterns of private-capital depreciation and scrappage.

5. Of course, many firms outside manufacturing will also be regional exporters. Manufacturing is a convenient sector to examine, because the overwhelming proportion of manufacturing output is exported outside of regions; in nonmanufacturing industries a lower percentage of output is exported outside of its region of origin.

6. To put the issue in a measurement-error framework, public spending is equal to true public service quality plus a probably sizable measurement error. The coefficient on public spending will tend to be a biased estimate of the true effect of public service quality, and this bias will be toward zero. The estimated coefficient on public spending will be a combination of the true effect of public service quality and the zero effect of the measurement error noise that is part of the public spending variable.

7. In addition, public capital measures do not reflect public capital costs that vary regionally, such as construction wages. The public spending measures used here—public spending as a percent of income—control somewhat for regional variation in wages.

8. Unweighted means over all states of the state and local spending categories as a percent of state income, over the entire 1958-89 period, are: elementary and secondary education, 4.4%; higher education, 1.6%; highways and roads, 2.4%; health and hospitals, 1.2%; all other nonwelfare, 5.1%; welfare, 1.6%.

9. In general, property taxes do not directly finance most higher education in the United States, although there are exceptions—for example, some states finance community colleges in part through property taxes. But property taxes could indirectly finance an increase in higher education spending. For example, if a state increased spending on higher education, and financed this increase by cutting state aid to local governments, and local governments in turn responded by increasing their property tax rates, then higher property taxes would have ended up financing the increase in higher education spending.

10. These two papers extend the research in Bartik (1991a), *Who Benefits From State and Local Economic Development Policies?*

11. As mentioned in the notes to the table, the table assumes that a given percentage increase in manufacturing output in some local area will result, due to multiplier effects, in the same percentage increase in total employment. Also, the calculations for the benefits of employment growth are based on data on employment growth and income growth for metropolitan areas, while the estimates in this paper are for states. Thus, the table implicitly assumes that states and metropolitan areas are similar types of regional economies that will behave similarly in response to taxes and employment growth.

12. It should be noted that this result holds even though the tax effects estimated in this paper are higher than in most other studies of state and local taxes and growth. In the current paper, a 10% cut in state and local tax rates results in about a 15% increase in manufacturing output, an elasticity of -1.5. As summarized in Bartik (1991a), most other studies find a considerably smaller elasticity, averaging about -0.25. However, elasticities tend to be larger for studies that use public service controls, studies that allow for fixed region effects of growth, and studies that focus on manufacturing. The current paper does all three.

13. This calculation is based on a change in property taxes of 0.36% of personal income and a change in non-property taxes of 0.64% of personal income. Based on the coefficients in the appendix, this would change manufacturing productivity by 8.4% and manufacturing output by 15.1%. As a first approximation, it is reasonable to assume that total national employment and capital does not change much in response to a change in one state's taxes. It should also be noted that, because of the difficulties in properly controlling for inputs in examining productivity growth, it seems likely that some of the measured effects of taxes on productivity probably actually reflect effects on input growth, not on productivity. Thus, it could be argued that the estimate that one-half of the output effects of lower state and local taxes is due to changes in inputs is a minimum estimate.

14. As explained in the appendix, the 11-year adjustment period is quite arbitrarily chosen. It is the longest period that could be accommodated with a reasonable number of observations given the available data on state and local public services and taxes.

15. The calculated average is the simple average of tax and spending rates in all bordering states, and is not weighted by the size of the bordering state, the length of the border with that state, or the degree to which population and business in the bordering state area are "close" in some sense to population and business in the state itself. Simplicity seem desirable in measuring these neighboring states' fiscal policies, and all these plausible adjustments would be more or less arbitrary.

16. Those familiar with Granger causality tests will recognize that the text describes the so-called "direct" version of the Granger causality tests. This version of the test has been advocated by Geweke (1984) and Harvey (1989).

17. The F-test statistic is 1.432, with 110 numerator degrees of freedom and 738 denominator degrees of freedom. The probability of a test statistic greater than or equal to this magnitude is 0.00428. In addition to controls for past growth, this regression included controls for past changes in wages and personal income. Eleven lags were included in all fiscal variables, growth variables, and wage and income variables.

18. Eberts and Fogarty used the Sims variant of the Granger causality test. It is interesting to note that the results appeared to be sensitive to the lag length used, with longer lags leading to stronger evidence of a statistically significant effect of public investment on private investment. In some cases, it appeared that public investment in one period was statistically significantly correlated with private investment 11 or 12 years later.

19. The three types of tax and spending limitations included are described in the appendix.

20. A downturn in manufacturing output growth might make it more likely that political party control will shift, but it is not obvious whether it will bias the shift toward or away from Democrats or Republicans. Furthermore, both economic downturns and economic booms may lead to tax or spending limitations.

21. Because the analysis of the determinants of state manufacturing output also includes a complete set of time dummies and state dummies, including as instruments these interaction terms with a time trend and a 0-1 variable for the South will not lead to spurious results generated by the influence of time or region on state manufacturing output growth.

22. This variable only varies across states for a given year, because states differ in the level of intergovernmental grants as a percentage of their income. Hence, using this variable as an instrument depends on accepting some of the functional form restrictions of the analysis, namely that intergovernmental grants should be scaled by some measure of the economic size of the state in determining their likely effect. If the intergovernmental grants variable were defined as the natural logarithm of intergovernmental grants divided by state income, then the predicted change in this variable would not vary across states for a given year. But this functional form assumes that a change in intergovernmental grants from 1% to 2% of income has the same effect as a change from 2% to 4%, which seems implausible.

23. Because of these problems, some authors have argued for measuring total factor productivity without a regression, by subtracting factor-share weighted inputs from output. See, for example, Hulten and Schwab (1991). This approach was not tried in the current project.

24. The author is grateful to Alicia Munnell for providing her data on public capital and private capital by state.

25. Eisner actually uses the differences from state averages for all variables, which has the same effect as including a vector of state dummies. Holtz-Eakin also considers a random-effects specification, in which it is assumed that the state effect is uncorrelated with the right-hand-side variables. However, he finds that the data reject the random-effects specification in favor of the fixed-effects specification. In addition, Holtz-Eakin considers an alternative specification that tries to use instrumental variables to correct for biases that may occur in short-panel fixed-effect models with serial correlation. The issue of how to estimate fixed-effects models using this database is considered further later in the appendix.

26. Note that the true long-run effects of the fiscal variables cannot be directly determined from the coefficients reported in Table A-11. Wages, personal income, and past growth are clearly endogenous. The long-run-reduced-form effects of the fiscal variables on manufacturing output can only be determined from the reduced-form equation, or from combining the equation in Table A-11 with other equations describing how wages, personal income, growth, and the fiscal variables adjust over time.

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