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The Effect of State Income Tax Apportionment and Tax Incentives on New Capital Expenditures

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ABSTRACT: This study examines how variations in states' corporate income tax regimes affect new capital investment by business. Using U.S. state-aggregated data from 1983 to 1996, we find in pooled and fixed-effects regressions that new capital expenditures by corporations in the manufacturing sector are decreasing in the income tax burden on property (measured as the product of the statutory tax rate and the property factor weight), and increasing at a decreasing rate in investment-related tax incentives. The effect of the income tax burden on property is more pronounced for states mandating unitary taxation or the throwback rule. Triangulating our empirical findings with prior analytical and simulation studies suggests the following hierarchy for the relative importance of major attributes of state corporate income tax regimes: the unitary or throwback requirement is most influential on incremental capital investment, followed by apportionment weights and tax rates, and, finally, investment-related incentives.

Keywords: *state taxation; apportionment formula; tax incentives, unitary business principle, throwback rule.*

JEL Classification: *H20; H71.*

INTRODUCTION

The purpose of this study is to provide empirical evidence on the effects of variations in states' corporate income tax regimes on new capital investment by business. Specifically, the study examines whether states with lower income tax burdens on property, measured as a combination of statutory corporate income tax rates and apportionment formula factor weights, experience a higher level of new capital spending by corporations. In addition, the study examines whether such spending is higher in states with more investment-related tax incentives. Finally, the study examines whether the effects of the state income tax burden and tax incentives on corporations' new capital spending differ between

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states whose income tax base is determined using unitary taxation or the throwback rule, and states without those rules.

Although there is a growing body of literature aimed at understanding the effects of subnational fiscal policy on state-level economic activity, the emphasis in many prior studies has been placed on employment levels and job creation (e.g., Lightner 1999; Goolsbee and Maydew 2000). In contrast, we focus on investment decisions in the form of incremental capital spending by corporations. Our focus is motivated in part by the belief that, even though job creation continues to occupy center stage in the political rhetoric surrounding state tax policy, business location and investment decisions typically are the engines that drive employment and income growth. Moreover, the limited prior empirical research that examines the role of state corporate income taxes on investment location decisions has yielded ambiguous and contradictory results.[1]

We contribute to this literature by going beyond the traditional state corporate income tax regime variables—tax rates and apportionment formula weights—that have been considered in the prior studies. Specifically, we also include in our empirical model investment-related tax incentives and tax base differences in the form of unitary (combined) reporting and the throwback rule. We believe all of these attributes of state tax regimes together reflect more completely the true burden of the state corporate income tax on business investment. Further, these attributes are likely correlated with each other, given states' propensity to use them interchangeably as instruments of fiscal policy; thus, not including them jointly could account for the ambiguous results of prior studies.

Using state-aggregated data from 1983 to 1996, we find that the state corporate income tax burden on property has a significant negative effect on new capital expenditures by manufacturers, whereas the number of investment-related tax incentives available has a significant positive, though declining, influence on capital spending. Although these results are robust to making comparisons across states or within states over time, the economic magnitude of these effects is modest, at best. For the average state, a one-percentage-point decrease in the income tax burden on property is associated with an estimated $2 to $6 million increase in capital spending; an additional incentive is associated with an estimated $0.5 to $2.5 million increase in new capital spending.

In our analysis that takes account of tax base differences between the states, we find that, after controlling for state fixed effects, the results for income tax burden on property and tax incentives hold only in the subsample of states that impose unitary taxation or the throwback rule. Because these tax regimes typically are considered more burdensome, our results suggest that the overall advantages of non-unitary and non-throwback tax systems may well offset the small-magnitude effects of any differential tax rates or general investment-related tax incentives.

Our empirical analysis based on state-level panel data spanning a 14-year period captures both cross-sectional and time-series variations in state tax regimes and, as such, the results shed light on the relative importance of tax rates, apportionment formulae, tax incentives, and the definition of the tax base on business investment decisions. In so doing, our results potentially can inform state tax policymakers. Triangulating our empirical findings with prior analytical and simulation studies suggests the unitary and throwback requirements are most influential on the location of capital investment, followed by apportionment weights and tax rates. Investment-related incentives have the least impact. It is

[1] See Bartik (1994) and Wasylenko (1997) for reviews of this literature.

important to note, however, that while we find statistical significance for the tax policy variables, the economic significance appears almost negligible. Nevertheless, we believe that this is an informative finding because it shows that the economic impact of changing state corporate income tax variables is not as large as policymakers might expect. Faced with continuing cutbacks in federal subsidies and threats of a shrinking tax base arising from interstate competition, electronic commerce, and creative accounting and legal practices, many states have adjusted their corporate income tax rate schedules, changed their apportionment formulae, or offered different tax incentives to stimulate investment and firm location within the state. This study's results provide a basis for evaluating the inevitable trade-offs confronted in making these choices and suggest the need for caution.

At a broader level, our study contributes to the ongoing policy debate on tax competition, stimulated in part by highly publicized instances of state and local governments vying, through various tax subsidies, to influence plant location decisions. Although the theoretical literature on tax competition between independent governments suggests strongly that such competition is wasteful, recent contributions identify some efficiency-enhancing roles (Wilson 1999). Empirical evidence on whether and the extent to which real economic decisions are related to variations in state tax regimes can inform this debate (Hofmann 2002).

The rest of the paper proceeds as follows. The second section briefly reviews the previous research and develops the hypotheses tested in the study; the third section describes our empirical procedures and data; the fourth section presents the results; and the last section offers concluding remarks.

HYPOTHESIS DEVELOPMENT

Review of Previous Research

Beginning with the pioneering work of Hall and Jorgenson (1967), researchers have devoted much attention at the national level to the impact of tax policy on investment. At the state level, however, empirical studies of the role of tax policy in economic development have focused attention primarily on aggregate employment or changes in employment. The studies that have analyzed the impact of state-level tax differentials on capital investment and the location of industry have yielded far from conclusive results (Wasylenko 1997). For example, Carlton (1979, 1983) studied the location decisions of new firms, and found that a weighted average of state corporate and individual income tax rates was not a significant factor in those decisions. Papke (1987, 1991) regressed new capital expenditures on three different measures of tax burden and found that the effective tax level measure (manufacturing tax revenue/manufacturing gross profits) and the proportional business burden measure (state and local taxes from business/total state and local tax revenues) were both insignificant, but the simulated after-tax return measure (based on industry-specific simulations of the change in the tax liability given an additional investment in the state) was strongly significant. However, in re-estimating Papke's (1987) results with 1991 data, Tannenwald (1996) found a much smaller tax effect that was statistically insignificant.[2]

[2] While the after-tax rate of return as a measure of tax burden has some appeal because it can be argued that it captures many hidden features of the tax code, there are also some conceptual problems. First, applying lessons from theoretical tax competition models, Knight (2001) demonstrates that jurisdictions (such as states used in this study) rather than firms or individual plants are the appropriate unit of observation, and that after-tax rates of return do not vary across jurisdictions and, in any case, these rates are endogenous since they depend upon the distribution of investment across jurisdictions. Second, calculating a after-tax rate of return at the firm level requires capturing tax incentives targeted at individual firms, which are increasingly playing a more dominant role in state tax policy but which are inherently impossible to capture in any empirical study.

Bartik (1994) reviewed this literature and concluded that the estimated tax elasticity for investment in the manufacturing sector appears to range from −0.10 to −0.36. But Wasylenko (1997) contends that these elasticities are not measured with much precision, and it would matter a great deal from a policy perspective on which end of the range the actual elasticities lie. Unfortunately, most of these studies overlook the way that the income of a multistate firm is apportioned among the states in which it does business (explained more fully in the next section), and the specific effect the property (payroll) factor weight has on the cost of locating property (labor) in a state.

McClure (1980) demonstrates how formulary apportionment transforms the state corporate income tax into separate taxes on sales, payroll, and property. Building on McClure (1980), analytical studies by Gordon and Wilson (1986) and Anand and Sansing (2000) show that variation in income tax rates and apportionment factor weights should affect firms' economic decisions relating to the location of jobs and property, but empirical studies have yielded conflicting evidence.[3] For example, Weiner (1996) finds that formula apportionment has no independent effect on capital-labor ratios across states in 1990, and only a modest and marginally significant effect on capital spending when examining changes in apportionment formulae from 1982 to 1990. In studying employment changes from 1994 to 1995, Lightner (1999) finds that low income tax rates, rather than factor weights, spur employment growth. Goolsbee and Maydew (2000), however, find that the apportionment formula, rather than the tax rate, is more influential—using panel data from 1978 through 1994, they estimate that double-weighting the sales factor increases manufacturing employment in the state by 1.1 percent. Employing a different methodological approach consisting of an eight-region applied general equilibrium model to simulate the effects of heavier sales factor weights on economic development and corporate tax revenues, Edmiston (2002) finds that a significant positive impact on economic development exists only in the very long run and that the short-run effects are negligible.

Inconsistencies thus remain about whether changes in formula apportionment can stimulate productive activity and, if so, what is the magnitude of these effects. As noted by Klassen (1999) and Edmiston and Arze (2002), further research is necessary to clarify these inconsistencies. We believe that incorporating the role of tax incentives and differences in the definition of the tax base (via unitary reporting or the throwback rule) together with formula apportionment can prove beneficial, and we develop those arguments below.

The Apportionment Formula

A key structural feature of the state corporate income tax in the U.S. is the apportionment formula used to subdivide multistate firms' income among jurisdictions with which they have sufficient contact (nexus). In general, a corporation's business income is apportioned among the states based on what portion of its sales, payroll, and property occur in each state. The theory is that these factors will fairly reflect the tax attributable to each state. Specifically, a multistate firm's income tax expense, x, in any particular state i is computed by the following formula:

[3] We should point out that studies examining tax-planning responses rather than real economic effects to state tax regime differences have had more success. For example, in state-aggregated data, Klassen and Shackelford (1998) find that companies likely structured their shipments strategically so as to reduce sales in states that apply a high assessment to gross receipts through the apportionment system. Similarly, in firm-level data, Gupta and Mills (2002) find that firms' state effective tax rates first increase and then decrease with the number of states in which they do business, consistent with their strategic use of tax regime differences, including apportionment formula, to lower their tax burdens.

$$x_i = \left[\left(w_i^s * \frac{s_i}{S}\right) + \left(w_i^L * \frac{l_i}{L}\right) + \left(w_i^P * \frac{p_i}{P}\right)\right] * \pi * r_i,$$

where π is the firm's U.S. (or worldwide) taxable income; r_i is the statutory tax rate in state i; s_i, l_i, and p_i are the firm's sales, payroll, and property in state i, while S, L, and P are the firm's total sales, payroll, and property; and w_i^S, w_i^L, and w_i^P are the factor weights in state i for sales, payroll, and property, respectively, that must sum to 1. Thus, the term in the brackets captures the percentage of a firm's income taxable in state i.

McLure (1980) has demonstrated that, to the extent tax rates vary across jurisdictions, this three-factor apportionment formula effectively transforms the state corporate income tax into separate (excise) taxes on sales, payroll, and property. For example, when a multistate firm invests in new property, the property ratio will increase in that state, which, in turn, will increase the amount of income tax paid to that state by $\Delta\ (p_i/P) * w_i^P * r_i$, or by the change in the property ratio times the income tax burden on property. The property ratio will decrease in all other states (because the denominator is now larger), and lower the income taxes paid to those states. Other things being equal, these effects provide the firm with an incentive to locate property in states with lower property factor weights and tax rates.

These effects also provide states with an incentive to lower the property factor weight in order to attract new business investment.[4] Figure 1 illustrates the changes that have taken place in states' apportionment formulae during the time span covered by our data. Consistent with the states' incentives, the trend away from equally weighting the factors toward double-weighted sales or even 100 percent sales has been increasing since the late 1980s.

The combined, multiplicative effect of the property factor weight and the tax rate leads to the first hypothesis, stated in alternate form:

H1: *Ceteris paribus*, new capital expenditures in a state are decreasing in its income tax burden on property.

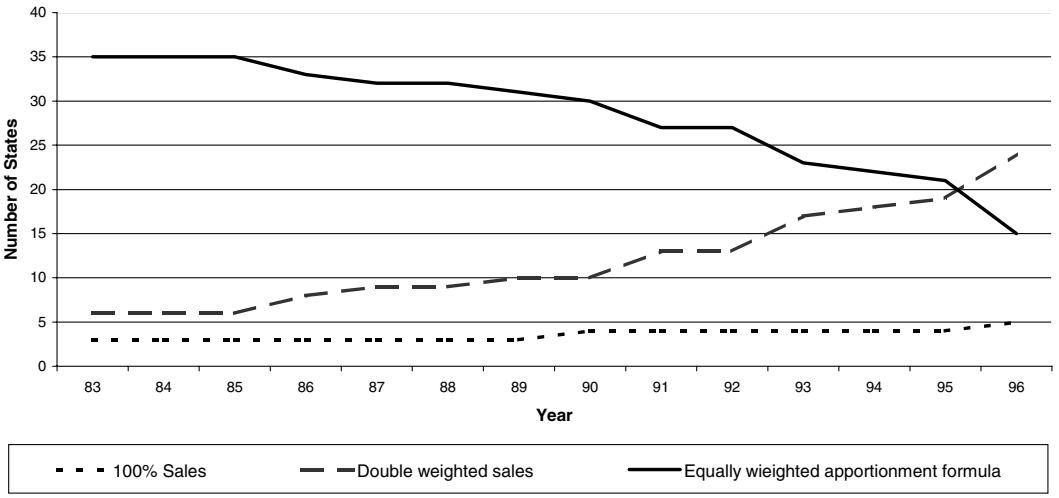
Investment-Related Tax Incentives

Apart from varying apportionment factor weights, states have also competed aggressively with each other by offering a variety of tax incentives. These actions have been motivated in part by the fact that changes to statutory rate schedules or the apportionment formulas have far-reaching impacts on a state's tax revenues. A tax credit, on the other hand, can be a more flexible tool for providing economic incentives to businesses—it can be less permanent than a change in rates or apportionment formula, targeted to a specific sector of the economy, and structured to reward only incremental investments.

While the Tax Reform Act of 1986 did away with the federal investment tax credit, a number of states have not only preserved their investment tax credit, but have also enacted new or expanded investment-related tax credits, such as enterprise zone credits, new

[4] Historically, states have placed equal weights on all three factors such that each factor receives a one-third weight. However, states have increasingly begun placing a greater (smaller) weight on the sales (payroll, property) factor since the U.S. Supreme Court's decision in *Moorman Manufacturing Co. v. Bair*, 437 U.S. 267, 98 S.Ct. 2340 (1978), which upheld the constitutional validity of Iowa's single-factor apportionment formula based solely on sales. The higher (lower) weight on the sales (property, payroll) factor also serves to export the tax burden to out-of-state firms that typically tend to have less property and payroll within the state. Some studies have tried to identify the factors associated with states' decision to switch their apportionment formulae. Omer and Shelley (2002) find that these changes are positively related to the number and timing of changes in competing states' apportionment formulae.

FIGURE 1
Trends in Apportionment



facilities credits, corporate headquarters (relocation) credits, etc. The effect of such incentives is to lower the tax cost of doing business in the state. Thus, these incentives act as substitutes for or complements to tax rate changes or apportionment formula changes designed to accomplish similar objectives. Consequently, any study of the relationship between state income taxes and economic activity is incomplete, and perhaps misleading (due to a correlated omitted variable problem), without also considering the effects of tax incentives.[5] Hypothesis 2 follows from the fact that the stated purpose of these tax incentives is to stimulate business investment:

H2: *Ceteris paribus*, new capital expenditures in a state are increasing in the investment-related tax incentives available in that state.

Unitary Reporting

Apart from tax rate and formula apportionment differences, states also differ fundamentally in how they define the corporate income tax base, i.e., the computation of a firm's taxable income subject to tax. The primary source of this difference lies in whether the state applies the unitary business principle. States mandating unitary (combined) reporting do so by requiring a company to file a combined return with all of its affiliates that form a unitary group. This allows the state to apply its apportionment formula to the combined income of a related group of corporations, even though some of the entities in the group might not otherwise be taxable in the state. On the other hand, non-unitary (separate reporting) states require each entity conducting business within the state to file a separate return. Non-unitary reporting creates significant tax-planning opportunities for multistate enterprises by allowing them to shift income and profits to low-taxed jurisdictions using

[5] Hines (1996) emphasizes this point in his study of the impact of state taxes on the surge in foreign direct investment in the U.S. during the 1980s.

techniques such as transfer-pricing strategies, passive investment companies, or inter-company loans (Smith 2000).

Recently, Williams et al. (2001) demonstrated analytically and with simulations that tax rate changes cause little or no change in the allocation of property and labor for non-unitary states, but can result in significant changes for unitary states. These conclusions are consistent with firms in unitary states being less able to use tax-planning techniques to minimize state income taxes, as well as being taxed on a broader base of income (Moore et al. 1987). Thus, tax burdens tend to be more onerous in unitary states than in non-unitary states, motivating the following hypothesis:

H3: *Ceteris paribus*, new capital expenditures in unitary states are decreasing in the income tax burden on property and by a greater amount than in non-unitary states.

The Throwback Rule

Another feature that affects the apportioned state corporate income tax base is the throwback rule. The rationale for this rule is to prevent any part of the corporation's sales from being assigned to states where the taxpayer is not taxable and thus escape taxation altogether. For purposes of the sales factor in the apportionment formula, sales of tangible personal property are sourced to the state to which the goods are shipped or delivered to the customer (the destination state). If the taxpayer is not taxable in the destination state because it lacks sufficient nexus, then those sales are not included in the numerator of the destination state's sales factor. States employing the throwback rule, however, reassign (throw back) those sales to the state of origin and include them in the numerator of that state's sales factor. Firms can reduce their overall state effective tax rates by shipping from non-throwback states into states where they have no nexus such that those sales are not included in the numerator of any state's sales factor, thus becoming ''orphan'' or ''nowhere'' sales.

For this reason, the lack of a throwback rule is viewed as a major tax incentive for in-state corporations, and recently some states (e.g., Arizona) have abolished the throwback rule in order to create incentives for corporations to remain or relocate to the state (Smith 2000). Although the throwback rule applies specifically to the sales factor and not the property factor per se, which is the main focus of this study, it is possible that companies choose to locate in non-throwback states so as to be able to engage in the tax planning afforded by the throwback rule. This reasoning motivates the following hypothesis for our study:

H4: New capital expenditures in states employing the throwback rule are decreasing in the income tax burden on property and by a greater amount than in states not employing this rule.

EMPIRICAL PROCEDURES

Model and Data

To test H1 and H2, we use data aggregated to the state level and estimate pooled and fixed-effects regression models of the following general form:[6]

$$Ln(CAPX_{it}) = \beta_0 + \beta_1 \cdot BURDEN_{it} + \beta_2 \cdot INCENT_{it} + \beta_3 \cdot VALADD_{it-1} + \varepsilon_{it},$$

[6] We also estimated this model cross-sectionally for each year and discuss these results (not tabulated) later.

where *CAPX* is a measure of the dollar amount of new capital expenditures incurred by the manufacturing sector in state *i* in year *t*, as reported by the U.S. Bureau of Census in their *Annual Survey of Manufactures* (1982–1996);[7] *BURDEN* is the product of the property factor weight (*PWT*) and the average marginal corporate income tax rate (*RATE*) of state *i* in year *t*; *INCENT* is the number of business tax incentives in state *i* in year *t,* as reported in *Site Selection* (1982–1995); and *VALADD* is the value added by the manufacturing sector in state *i* in the year $t - 1$, as reported in the Bureau of Census, *Annual Survey of Manufactures* (1982–1996). Apart from *VALADD*, we include the following additional control variables in alternative specifications of the regression model: energy costs (*ENRG*), defined as the total industrial sector energy price (per million Btu) as reported by the U.S. Department of Energy; public expenditures (*PUB*), defined as the total state and local direct general expenditures (less expenditures for welfare) as reported by the U.S. Census Bureau's (1982–1996) *State and Local Government Finances*; and census region dummies. Table 1 details a complete definition of each variable and the data sources for constructing them, and we discuss below certain measurement issues affecting some of these variables.

The choice of the dependent variable as the dollar amount of new capital expenditures in the manufacturing sector warrants some discussion, especially given the growing importance of the nonmanufacturing sectors in our economy. Our choice is motivated by several factors. First, the property factor in states' apportionment formulae includes only real and tangible personal property. Thus, intangibles do not impact the income tax burden on property that is captured via the apportionment formula. Second, capital expenditures in nonmanufacturing sectors are not readily observable and hence reliable measures are not available for each state over time. Both of these reasons probably explain why prior studies (e.g., Carlton 1983; Weiner 1996; Goolsbee and Maydew 2000) are dominated by data from the manufacturing sector. Our use of these data allows for better comparability with these studies. Finally, concerns about generalizability of our results outside the manufacturing sector are mitigated by the fact that growth in manufacturing correlates highly with the growth in state gross domestic product ($\rho = 0.65$), an overall measure of states' economic growth that also includes the contributions made by intangibles. For individual states, this correlation ranges between 0.30 and 0.85 during our sample period.[8]

In constructing *BURDEN*, we use the top statutory rate as a proxy for the average marginal tax rate in each state. While many of the states have progressive tax rate schedules, the top statutory rate is reached at fairly low levels of income. During the time period covered by our study, six states allow federal income taxes to be deducted in the computation of state taxable income; for these states, the statutory rate is adjusted for this difference in the tax base.[9]

Constructing the *INCENT* variable is complicated. Ideally, we would like to quantify the relative amount of tax relief available through these incentives across all states over

[7] Apart from availability of reliable data, our choice of using state-aggregated data on capital expenditures as the left-hand side variable, rather than individual firm or plant-level data, is reinforced by Knight's (2001) recent work based on tax competition models. He shows that plant-level data, as used in some prior studies (e.g., Carlton 1983; Papke 1991), violate a key assumption of discrete choice analysis—that of independence of plant location choices. Further, we use the natural log of *CAPX* to correct for the nonlinearity of the relationship between *CAPX* and the explanatory variables. A Box-Cox subroutine in SAS suggested a log transformation of *Y*. In addition to creating a tighter model, the log specification allows for the interpretation of the regression coefficients as the percentage change in *CAPX* that results from a one-unit change in the explanatory variables.

[8] Only four states (Delaware, Hawaii, Kansas, and Montana) have correlations of less than 0.30 between growth in manufacturing and growth in state gross domestic product. This is not surprising given the large role of tourism and agriculture in these states.

[9] The six states are Alabama, Arizona, Louisiana, Missouri, North Dakota, and Iowa. Arizona discontinued the deductibility of federal income tax in 1990.

TABLE 1
Variable Definitions and Data Sources

CAPX =	dollar amount (in millions) of new capital expenditures for firms in the manufacturing sector, by state. The measure includes permanent additions and major alterations to manufacturing establishments, and machinery and equipment subject to depreciation purchased during the year, or leased under capital leases. (See U. S. Census Bureau's *Annual Survey of Manufactures* (1982–1996).);
LCAPX =	the natural logarithm of *CAPX*;
PWT =	the property factor weight used in the state's apportion formula, expressed as a percent times 100 (1/4 = 25). (See ACIR's (1982–1995) *Significant Features of Fiscal Federalism* and CCH's (1982–1996) *All States Tax Reporter*.);
RATE =	the top statutory corporate income tax rate, by state, expressed as a percent times 100 (5% = 5). (See ACIR's (1982–1995) *Significant Features of Fiscal Federalism* and CCH's (1982–1996) *All States Tax Reporter*.);
BURDEN =	the product of *PWT* and *RATE* divided by 100. For a state with a *PWT* of 25 and a *RATE* of 5, *BURDEN* = 1.25;
INCENT =	the number of tax incentives for industry. These incentives include: corporate income tax exemptions; exemptions or moratoriums on land and capital improvements; exemptions or moratoriums on equipment and machinery; tax exemptions for manufacturers inventories; sales/use tax exemptions on new equipment; tax exemptions on raw materials used in manufacturing; tax incentives for job creation; tax incentives for industrial investment; tax exemptions to encourage R&D; accelerated depreciation for industrial equipment; etc. (See *Site Selection* (1982–1995).);
INCENTSQ =	the squared value of *INCENT*;
VALADD =	dollar amount (in $billions) of value added by the manufacturing sector. This measure of manufacturing activity is derived by subtracting the cost of materials, supplies, containers, fuel, purchased electricity, and contract work from the value of shipments (products manufactured plus receipts for services rendered). This figure is adjusted for the net change in finished goods and work-in-process between the beginning- and end-of-year inventories. Value added is considered to be the best available value measure for comparing the relative economic importance of manufacturing among geographic areas. (See U.S. Census Bureau (1982–1996) *Annual Survey of Manufactures*.);
ENRG =	the average total energy price (in nominal dollars per million Btu) for the Industrial Sector. It includes prices for Coal, Natural Gas, Petroleum products, and Electricity. (See U.S. Department of Energy, Energy Information Administration (1995) *State Energy Price and Expenditure Report 1995.*); and
PUB =	state and local direct general expenditures (in $millions), less amounts expended for public welfare. (See ACIR's (1982–1995) *Significant Features of Fiscal Federalism* and U.S. Census Bureau (1982–1996) *State and Local Government Finances*.)

time. However, these incentives vary widely in the tax base to which they apply, the rates at which they are allowed, and their qualification criteria, making such quantification virtually impossible. Further, state-aggregated data on the cost (in lost revenues) of such tax incentives is not consistently available and, even if available, this measure is likely endogenous.[10] Hence, we define *INCENT* as the number of tax incentives for industry offered by

[10] Even if the data were available, what we would get is the dollar amount of investment-related incentives actually *used* by firms, which would be endogenous to new capital expenditures. Furthermore, that data would still not accurately reflect the relative amounts of tax relief *available* across states.

each state in a given year and obtain the count from *Site Selection* (1982–1995).[11] We use this count as a rough proxy of tax relief—at the very least, a high count implies a legislative climate sympathetic to business.[12] We note that our *INCENT* variable captures only general tax incentives available to all taxpayers. Targeted tax incentives negotiated with specific firms are not included in this study as this data is not available; in any event, omitting them should only bias against finding significant results.

The practice of using tax incentives to compete for new and existing businesses is widespread (Burstein and Rolnick 1996). Beyond some minimal number of incentives, additional incentives offered by the states are likely to be narrower in scope and offer less incremental tax relief. Further, two or three narrowly defined incentives may not be better than one broad-based incentive. It seems plausible, therefore, to expect a diminishing marginal response to incentives offered by a state. To capture this potential nonlinearity, we also include *INCENTSQ*, the square of *INCENT*, in the model.

VALADD is used to control for size differences across states. In contrast with other studies that have used population to control for states' size (Papke 1987, 1991; Weiner 1996), we believe *VALADD* is more appropriate since it measures the size of the manufacturing sector in each state's economy (Klassen and Shackelford 1998).[13] To mitigate the potential endogeneity of *VALADD* with new capital expenditures, we use a one-year lagged value for *VALADD*.

We begin our sample period with 1983. Data for most of the above variables is available only through 1996, so we construct a panel for the 14-year period from 1983–1996. During this time period, there are 44 states that impose a corporate income tax, so there are a total of 616 (44 $\times$ 14) state-year observations.[14]

Descriptive Statistics

Panel A of Table 2 provides descriptive statistics for the variables included in the model. The mean (median) dollar amount of new capital expenditures over the 14-year period is almost $1.9 billion ($1.3 billion). The mean (median) top statutory corporate income tax rate during that period is 7.28 (7.13) percent and ranges from a low of 2.76 percent to a

[11] The specific incentives included in the *INCENT* variable are listed in Table 1. *Site Selection* is the official publication of the International Development Research Council (IDRC) with a reported circulation to over 45,000 executives responsible for making location-planning and facility-expansion decisions around the world. *Site Selection*'s editorial claims that more than 50 percent of their subscribers are the highest-level managers and decision makers. The Business Periodicals Index indexes this publication. See http://www.siteselection.com for more information.

[12] Hines (1996, 1092) also struggles with the quantification of incentives, and concludes that ''it is not possible to obtain a precise and exogenous measure.'' In an earlier paper, he also used a count of incentives offered to investors but found it to be insignificant in explaining foreign direct investment in the states (Hines 1993).

[13] For example, according to 1990 census data, Connecticut and Oklahoma had similar populations, numbering between 3.2 and 3.3 million people. However, the more industrialized state of Connecticut had a *VALADD* of $23.8 billion that year, while the more agrarian Oklahoma had a *VALADD* of $11.9 billion. One would not expect the same level of new capital expenditures for the manufacturing sector in Oklahoma as in Connecticut. Thus, population does not adequately control for the size of the manufacturing sector. In any event, we also used the gross state product (GSP) as an alternate control for state size. GSP is the state-level counterpart of gross domestic product (GDP) at the national level. Results with GSP are similar. Finally, in the regression results reported later, we also use White's (1980) procedure to correct for heteroscedasticity created by size differences across states.

[14] The six excluded states are Michigan, Nevada, South Dakota, Texas, Washington, and Wyoming. Michigan's Single Business Tax is a value-added tax; Texas' franchise tax was a net-worth tax until 1991; Washington's business and occupation tax is based on gross receipts; Nevada, South Dakota, and Wyoming impose no business income tax. Since some of these states tax businesses in different ways, it did not seem proper to include them in the sample and simply assign an income tax rate of zero. However, for sensitivity purposes, we also estimated the regression models including Michigan, Texas, and Washington, as well as including all 50 states, and report these results later.

high of 13.80 percent. Half the states during the sample period use a one-third property factor weight, implying an equally weighted formula apportionment. The average number of incentives offered during the period of the study is 10, and ranges from a low of 3 to a high of 14.

Figure 2 shows the trends in the tax variables and new capital expenditures for the 44 states in our sample over time. While the mean rate increased slightly—from 7.06 percent in 1983 to a high of 7.6 in 1992, and back down to 7.3 percent in 1996—the decline in the mean property factor weight has resulted in a slight decline in the mean burden, from 2.12 to 1.87 percent. The mean number of incentives increased from 7.7 to 11. However, the mean value for new capital expenditures in the manufacturing sector increased dramatically from $1.2 billion to $3.4 billion during the same time period.

To better depict the variability in the tax variables over time, Panel B of Table 2 categorizes the tax variables (*PWT*, *RATE*, *BURDEN*, and *INCENT*) into relevant ranges, and reports the number of states whose values fall in each category in each year. The mean of the natural log of capital expenditures is also shown for each category and by year. While the states with the highest property factor weights experienced the lowest new capital expenditures, the relation does not hold monotonically across all three categories. Likewise, the mean capital spending of states with the lowest top statutory tax rates is statistically indistinguishable from that of the states in the highest rate bracket. In contrast, *BURDEN*, which is the product of the property factor weight and tax rate, exhibits a significant negative relationship with capital expenditures, supporting H1. With regard to *INCENT*, states with higher numbers of tax incentives seem to have higher capital expenditures, which supports H2. Because of the interactive and possibly offsetting relationships among these tax variables, however, we examine them in a multivariate framework and present the results of this analysis next.

Panel C of Table 2 reports pairwise correlation coefficients between the different variables used in the regression models. *BURDEN* is highly correlated with *RATE* and *PWT*, which is to be expected since it is the product of those two variables. But the correlations among most of the other explanatory variables are less than 0.30, suggesting that the regression results should not suffer from harmful multicollinearity. It is interesting to note that *INCENT* is negatively correlated with *PWT* and *BURDEN*, suggesting that incentives are likely complements to lower property weights or burdens.

RESULTS

Results for the Tax Rate Variables

Pooled and Panel Data Regression Results

Table 3 reports the regression results for five models of new capital expenditures, with coefficient estimates in the first row and related t-statistics (corrected for heteroskedasticity using White's (1980) standard errors) in parentheses in the second row. The first column presents the results of the basic regression model (Model 1). The coefficient for *BURDEN* is negative, the coefficients on *INCENT* and *INCENTSQ* are positive and negative, respectively, and all are significant at the .01 level.[15] Hypotheses 1 and 2 are supported; new capital expenditures are declining in the property tax burden, but increasing (at a decreasing rate) for states offering more incentives. Finally, the coefficient for *VALADD* is positive and highly significant, as expected.

[15] Wald tests confirm the joint significance (at or below the .05 level) of *INCENT* and *INCENTSQ* in all models presented.

TABLE 2
Descriptive Statistics

Panel A: Descriptive Statistics for the Variables Based on the Pooled Sample, 1983–1996 (616 state-year observations)

	Mean	Median	Minimum	Maximum	Std. Dev.
CAPX	1,876	1,271	35	41,034	2,459
LCAPX	13.86	14.06	10.45	17.53	1.19
PWT	28.96	33	0	50	8.67
RATE	7.28	7.13	2.76	13.8	1.94
BURDEN	2.09	2.15	0	4.04	0.73
INCENT	9.7	10	3	14	2.5
$VALADD_{t-1}$	24,382	15,432	501	178,000	27,292
$ENRG_{t-1}$	6.07	5.88	1.90	15.11	1.74
PUB_{t-1}	13,078	7,987	869	127,329	17,034

Panel B: State Tax Characteristics by Year, 1983–1996 (44 observations per year)

Variable	Range	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	Total	Mean *LCAPX*
PWT	<25	3	3	3	3	3	3	3	4	4	4	4	4	4	5	37	14.102
	25–32	6	6	6	8	9	9	10	10	13	13	17	18	19	24	142	14.588+
	>32	35	35	35	33	32	32	31	30	27	27	23	22	21	15	261	13.522**
RATE	<6%	11	11	10	10	9	7	10	9	9	9	9	9	9	9	89	14.131
	6–8.99%	22	22	23	24	25	28	25	26	24	24	25	25	25	25	171	13.634*
	>8.99%	11	11	11	10	10	9	9	9	11	11	10	10	10	10	180	14.157
BURDEN	<1.75	9	9	9	9	9	8	10	10	12	12	12	12	12	15	112	14.214
	1.7–2.5	25	24	24	22	22	22	21	20	19	19	23	24	25	24	211	13.888*
	>2.5	10	11	11	13	13	14	13	14	13	13	9	8	7	5	117	13.483**
INCENT	<8	21	20	15	12	11	11	9	8	6	6	6	5	4	4	70	13.236
	8–11	19	20	24	25	24	23	24	21	22	20	20	21	22	21	218	13.999+
	>11	4	4	5	7	9	10	11	15	16	18	18	18	18	19	152	14.117+
Mean *LCAPX*		13.42	13.63	13.71	13.61	13.69	13.73	13.89	13.94	13.89	13.96	13.99	14.08	14.18	14.33	13.86	

(*continued on next page*)

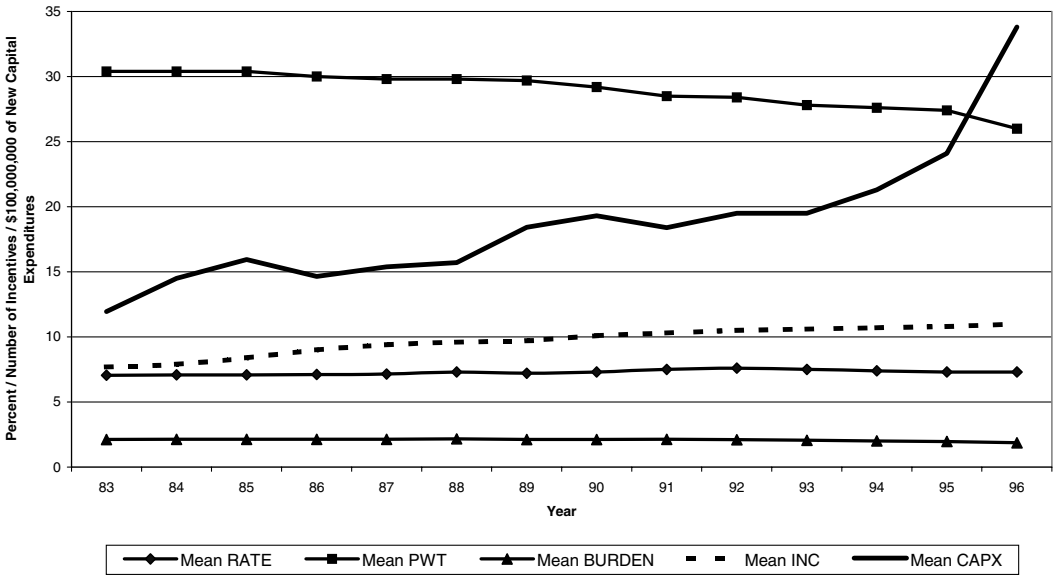
TABLE 2 (continued)

Panel C: Pairwise Correlation Coefficients Based on the Pooled Sample, 1983–1996 (616 state-year observations)[a]

	LCAPX	*PWT*	*RATE*	*BURDEN*	*INCENT*	$VALADD_{t-1}$	$ENRG_{t-1}$	PUB_{t-1}
CAPX	.694	−.092	.125	.021	.159	.814	−.006	.678
LCAPX		−.199	.003	−.172	.288	.775	−.055	.612
PWT			−.104	.682	−.265	−.140	−.035	−.099
RATE				.611	.028	.208	.222	.212
BURDEN					−.202	.053	.154	.079
INCENT						.183	−.133	.237
$VALADD_{t-1}$							.041	.898
$ENRG_{t-1}$								.048

* (+) Mean is significantly less than (more than) the mean of the first category at the .05 level of significance, one-tailed test.
** Mean is significantly less than the means of the first two categories at the .05 level of significance, one-tailed test.
[a] Correlations greater than .08 in absolute value are significant at the .05 level of significance (two-tailed test).

FIGURE 2
Trends in Tax Variables and Capital Spending



The basic model excludes many factors that are likely to influence capital investment by firms. To the extent that one or more of these excluded factors is correlated with the explanatory variables included in the model, the reported coefficient estimates will be biased. Models 2–4 attempt to mitigate this problem in different ways.

Business investment decisions may be affected by regional differences in labor, energy, and transportation costs, as well as by agglomeration economies (Papke 1991). Therefore, in Model 2 we control for geographical variation in these and other macroeconomic factors by including indicator variables for seven of the eight census regions (the Northeast region was omitted). The coefficients for these indicator variables are not presented; instead, the number of census regions with significant coefficients is indicated in the table. The coefficient estimates and statistical significance of the tax variables under this specification are virtually identical to Model 1.

As mentioned earlier, previous studies have found variables such as energy prices and public expenditures to be statistically significant in models of capital investment (e.g., Papke 1987; Weiner 1996). The influence of energy costs on capital expenditures is obvious. Public spending is included to capture the variation across states in the quantity/quality of public goods and services provided (Helms 1985; Mofidi and Stone 1990). For instance, states that provide better highways or better schools might appear more attractive to businesses; thus, public spending has an influence on business location decisions. The correlation matrix in Panel B of Table 2 shows that energy prices and public expenditures are both somewhat correlated with the tax variables included in the basic model; omission of the former may result in biased estimates for the latter.

In Model 3, we include variables for energy prices (*ENRG*) and public expenditures (*PUB*) as additional controls (instead of the regional dummy variables in Model 2). As with *VALADD*, we use one-year lagged values for both *ENRG* and *PUB* to mitigate against

TABLE 3
Regression Results of New Capital Expenditures

Coefficient Estimates from Pooled and Fixed-Effects Regressions of the Following Model for the Period 1983–1996 (t-statistics in parentheses; 616 state-year observations):

$$LCAPX_{it} = \beta_0 + \beta_1 \cdot BURDEN_{it} + \beta_2 \cdot INCENT_{it} + \beta_3 \cdot INCENTSQ_{it} + \beta_4 \cdot VALADD_{it-1} + \beta_5 \cdot ENRG_{it-1} + \beta_6 \cdot PUB_{it-1} + \varepsilon_{it}$$

Variable (Expected Sign)	Simple Pooled Models				Fixed Effects Model
	Model 1	Model 2	Model 3	Model 4	Model 3
Intercept	11.577 (26.21)**	12.315 (39.11)**	11.622 (31.81)**	12.295 (35.35)**	NA
BURDEN (−)	−0.315 (−9.57)**	−0.293 (−6.27)**	−0.275 (−7.43)**	−0.313 (−6.95)**	−0.097 (−2.37)**
INCENT (+)	0.437 (4.64)**	0.298 (4.14)**	0.391 (5.25)**	0.303 (4.40)**	0.137 (3.43)**
INCENTSQ (−)	−0.021 (−4.26)**	−0.015 (−3.79)**	−0.018 (−4.38)**	−0.014 (−3.77)**	−0.005 (−2.13)*
$VALADD_{t-1}$ (+)	0.033 (17.13)**	0.033 (22.84)**	0.050 (22.99)**	0.047 (23.01)**	0.026 (9.56)**
$ENRG_{t-1}$ (?)			−0.013 (−0.83)	−0.008 (−0.54)	−0.077 (−4.65)**
PUB_{t-1} (?)			−0.03 (−8.61)**	−0.024 (−8.04)**	−0.015 (−5.27)**
Number of Significant Census Regions[a]	NA	5	NA	5	NA
Adj. R^2	0.67	0.78	0.70	0.79	0.96

#, * and ** denotes significance at the .10, .05 and .01 levels (one-tailed test where directional predictions were made), respectively.
[a] Models 2 and 4 include indicator variables for 7 of the 8 Census regions. The Northeast region was omitted. The coefficients for constant terms are not shown.
The t-statistics are corrected for heteroskedasticity using White's (1980) standard errors.

potential endogeneity concerns. In Model 4, we include *ENRG* and *PUB*, as well as the regional indicator variables. The results from Models 3 and 4 are reported in the third and fourth columns of Table 3. The addition of these control variables does not appear to affect the sign or significance of the tax variables, although the magnitude of the tax coefficients is somewhat reduced. With respect to the control variables themselves, new capital expenditures are negatively related to both energy prices and public expenditures.[16]

Apart from energy costs and public expenditures, there are likely to be a whole host of state-specific factors that may affect capital spending, including state business climate,

[16] While the first result is intuitive, the latter requires further discussion. If the comparative level of public expenditures represents the quality or quantity of public goods and services in a state, then one would expect a positive relationship with new capital expenditures by manufacturers. However, to the extent that public expenditures are funded by taxes, higher per-capita public spending may simply reflect higher tax burdens, and thus discourage business expansion. It is this latter effect that appears to dominate in Models 3 and 4, which is also consistent with the fairly high positive correlation between *PUB* and *RATE* in Panel B of Table 2.

weather, quality of labor force, and endowments of natural resources. To the extent these state characteristics influence or are influenced by the state's fiscal policies, they are potentially correlated omitted variables in our model of new capital expenditures. In fact, the plots of the residuals from all of the pooled models show a high degree of correlation among the 14 observations from each state, suggesting the lack of independence among these observations. A fixed-effects specification should control for such influences to the extent they do not vary much over time.

The results of the fixed-effects regression using the 14-year panel estimated for Model 3 that includes all control variables (except the regional dummies) are presented in the final column of Table 3. The likelihood ratio and F-test for the fixed-effects model imply that, although most of the variation in new capital spending across states is explained by state fixed effects, the *X*-variables do provide significant additional explanatory power.[17] As far as the main tax variables are concerned, the coefficient for *BURDEN* remains negative and significant and the coefficient for *INCENT* is positive and significant. The coefficient for *INCENTSQ* is negative but insignificant.

Discussion of the Results

Because the dependent variable is in log form, the estimated coefficients in the regression models can be interpreted as the percentage change in new capital expenditures for each one-unit change in the independent variables. The value of β_1 from Model 4 of the pooled regressions in Table 3 is approximately -0.31. This implies that a state whose *BURDEN* is one percentage point lower than another state (as it would be, for example, if the first state had a top statutory rate of 8 percent and a property weight of one-fourth, while the other state had a top statutory rate of 9 percent and a property weight of one-third), would experience 0.31 percent higher new capital expenditures, *ceteris paribus*. At the mean value for new capital expenditures, this represents an additional $6.2 million in new capital spending in the state with the lower income tax burden on property. In the fixed-effects model, the coefficient for *BURDEN* is much smaller. Using the β_1 estimate of -0.097, a state that lowers its income tax burden on property by one percentage point (as it would, for example, if it had a top statutory rate of 12 percent and it went from equal-weighting to double-weighting sales) would experience a little over $1.8 million in additional new capital spending. These cross-state and within-state estimates suggest that the income tax burden on property does have a statistically significant, but economically modest, influence on new capital investment. To put it in perspective, consider the corresponding loss in state corporate income taxes collected. In 1996, the mean *BURDEN* for the states in our sample was 1.87 percent. A one-percentage-point drop in *BURDEN*, to 0.87 percent, would reduce corporate income tax revenues (at least those related to property and payroll) in the average state by more than one-half. The mean amount of state corporate income taxes collected by the states in our sample was approximately $615.7 million. This potential revenue loss seems out of proportion to the capital investment gained, even if the new business does broaden the tax base.

Given the inclusion of *INCENTSQ* in the model, the effect on new capital expenditures of adding one more incentive can be calculated using estimates of $(\beta_2 + 2\beta_3 * INCENT)$. So, comparing a state with five incentives to one with six, and using the coefficients from the Model 4 pooled regressions, the state with one more incentive would experience approximately $(0.303 + [2 \times -0.014 \times 6]) = 0.135$ percent more in capital spending, or

[17] The Chi-square statistic for the likelihood ratio test that the *X*-variables provide incremental explanatory power over the state fixed effects is 304.59, which is significant at less than the .001 level.

just over $2.5 million at the mean for new capital expenditures. On the other hand, comparing a state with 11 incentives to one with 12, the state with 12 incentives would experience approximately −0.033 percent, or about $0.62 million less in new capital spending. Using the smaller coefficient estimates from the fixed-effects regressions, these differences would be a 0.077 percent ($1.44 million) increase when going from five to six, and a 0.017 percent ($0.32 million) increase when going from 11 to 12 incentives. Using the coefficients for *INCENT* and *INCENTSQ* from the Model 4 pooled regressions, capital spending increases with additional incentives up to a point around 11 incentives, which is approximately the mean and median number of incentives in our data. After that, additional incentives are associated with decreased spending. Using the estimates from the fixed-effects Model 3, capital spending increases with the addition of incentives up to 14, which is the maximum of the range of our data. Again, it should be pointed out that the revenue loss from these incentives might very well exceed the value of the increased investment.

Sensitivity Tests

To examine the sensitivity of our results to data choices and model specification, we conduct the following additional tests. First, we re-estimated the regressions including Michigan, Texas, and Washington, the three states with some type of income-based taxation on businesses (see footnote 14), increasing our sample from 44 to 47 states. Similarly, we re-estimate the regressions including all 50 states. In each case (results not shown), the coefficients for *BURDEN* are of smaller magnitude and significance, while the coefficients for *INCENT* and *INCENTSQ* are larger and more strongly significant. The basic conclusions, however, are unchanged.

Second, with regard to model specification, we experiment with alternatives to the *BURDEN* variable. As illustrated in the hypothesis development, a new capital expenditure in a state will result in a change in the corporate income tax owed in that state, all else held constant, equal to the change in the property ratio times the product of the property factor weight and the tax rate (*BURDEN*). Because firms making location decisions will likely consider both tax rates and apportionment weights as a package, we do not consider *BURDEN* to be an interaction of two independent variables. Indeed, we have no reason to believe that either the rate or the factor weight exerts an independent influence on the property location decision. Nevertheless, previous studies have examined the separate effects of the factor weights and the income tax rate. While Goolsbee and Maydew (2000) found that the payroll factor weight was far more influential than the tax rate in determining state employment, Lightner's (1999) study led to the opposite conclusion. Hence, we re-estimate the pooled and fixed-effects models, using *RATE* and *PWT* instead of *BURDEN* (results not shown). In the pooled models, the coefficients for both *PWT* and *RATE* are negative and significant. In the fixed-effects models, the coefficients are negative but, while *PWT* is significant, *RATE* is not, a result similar to Goolsbee and Maydew (2000). Likewise, we re-estimate the models using *PWT*, *RATE*, and *BURDEN* (results not shown). In this case, *PWT* and *RATE* are both mildly positive, while *BURDEN* is strongly negative (all are significant). The results of the sensitivity analysis seem to suggest that, while both *PWT* and *RATE* are influential, it is the product of *PWT* and *RATE* (that is, the property *BURDEN*) that best captures the relationship between capital expenditures and income tax factors, particularly for between-state comparisons.

Third, in addition to the income tax burden on property arising from the apportionment formula, a firm's decision to locate property may also be influenced by other taxes, notably property taxes levied on real and personal property by state and local governments. Most states do not have a state-level property tax on real estate; instead, municipalities levy

property taxes that can vary widely from one municipality to the next within a state. A few states do have personal property taxes, but often exempt manufacturing equipment and/or inventories from such taxes. Hence, getting a clean measure for property taxes at the state level is difficult. Nevertheless, we re-estimate the models including a variable for property taxes (results not shown) based on the midpoint of the ranges of state and local property tax rates reported in Research Institute of America's (RIA) (1982–1996) *All States Tax Handbook* for each state-year. The property tax variable is not statistically significant, and its inclusion does not change the signs or magnitudes of the other variables.[18]

Finally, the fixed-effects model controls for state-specific characteristics that do not vary over time. It is possible, however, that there are macroeconomic factors unique to the time periods that do not vary across states. To the extent that these period effects are correlated with our independent variables, our results could be biased. We address this concern two ways. First, we run 14 annual cross-sectional regressions (results not shown). The coefficient estimates for *BURDEN*, *INCENT*, and *INCENTSQ* follow the hypothesized pattern in all but two years. There is no detectable time trend in the coefficient estimates. Further, the Fama and MacBeth (1973) t-statistics calculated on the cross-sectional means of coefficient estimates such that they are not affected by the cross-sectional correlation problem, as well as the Rosenthal (1991) Z-statistics that are measures of combined probability, indicate that all three variables are statistically significant at or below the 0.05 level across the 14-year span.[19] Second, we estimate a two-way fixed-effects model, which simultaneously controls for both state-specific and year-specific fixed effects. This specification results in reduced magnitude and loss of statistical significance for *BURDEN* and *ENRG*. (However, in the analyses that include interactions between the tax variables and unitary or throwback dummies, discussed later, *BURDEN* retains its statistical significance even in two-way fixed effects models.)

Results for the Tax Base Variables

Data Issues

To analyze the effects of unitary taxation and the throwback rule, we first classify states as either unitary or non-unitary and throwback or non-throwback. While determining whether a state has adopted the throwback rule is straightforward, the unitary/non-unitary classification is open to interpretation. There are varying degrees to which states require, allow, or optionally impose the unitary principle in the form of combined reporting.[20] In states where combined reporting is permitted but not mandatory, firms are most likely to file a combined report when it is to their advantage, while states are most likely to impose combined reporting when it will result in a higher tax liability. Since we are trying to identify those states in which imposition of the unitary principle results in a more onerous

[18] It was suggested that sales taxes and other forms of state taxation also should be included in the analysis, but measures for such variables would suffer from the same shortcomings as the property tax variable, leading to a compounding of ''noise.'' Most measures of business climate are based heavily on income taxation, and would thus overlap our other tax variables.

[19] The Fama and MacBeth (1973) t-statistics for *BURDEN*, *INCENT*, and *INCENTSQ* are −5.67, 1.58, −1.36, respectively. We also use two Z-statistics, Z1 and Z2, based on Rosenthal (1991) as measures of the combined probability that the coefficient in question could be greater than/less than zero in N different samples if the population mean was zero. $Z1 = \Sigma_t/\sqrt{\Sigma\ df/(df-2)}$, and $Z2 = \Phi\sqrt{(N-1)}/\sigma(\phi)$, where t is the t-statistic for each yearly regression, df is the degrees of freedom in each regression, N is the number of yearly regressions, ϕ is the standard normal deviate corresponding to the statistical significance of t, and Φ and $\sigma(\phi)$ are the mean and standard deviation of the N realizations of ϕ.

[20] We thank the discussant for prompting us to carefully think about the classification of states as unitary because of these variations.

tax burden, we classify as unitary only those states identified in CCH's (1982–1996) *Multistate Income Tax Guide* as requiring combined reporting for all unitary businesses.[21]

Further, it is possible that states' choice of tax rates or incentives depends on their definition of the tax base, suggesting potential interactions between the tax rate and tax base variables. To determine whether the subsamples are different enough that they merit being examined separately, we perform the Chow test for a structural break between the two groups. The test statistics are significant at less than the 0.001 level (the F-statistic is 44.06 for the unitary/non-unitary states and 30.35 for the throwback/non-throwback states), confirming that the relationship among the variables differs across the subsamples. Consequently, we analyze each subsample separately and Table 4 reports the results of this analysis; footnotes to that table list the states that fall in each group.

Univariate Tests and Regression Results

Panel A of Table 4 presents descriptive statistics for the new capital expenditures and the tax variables as well as univariate tests of differences in the means of these variables for each of the two subgroups. As predicted in H4, throwback states have significantly lower mean *CAPX* than their counterpart non-throwback states. We do not see this pattern, however, in the unitary/non-unitary subsamples. Unitary states have a significantly higher average tax rate, which translates into a higher average *BURDEN* than non-unitary states. In contrast, throwback states have lower average tax rates but higher property factor weights for an overall lower average *BURDEN* than non-throwback states. In terms of tax incentives, both unitary and throwback states appear to have a significantly lesser number of incentives available than their counterpart non-unitary and non-throwback states, respectively. This pattern again suggests that *INCENT* and *BURDEN* are complements rather than substitutes.

Panel B of Table 4 reports the pooled and fixed-effects regression results separately for unitary/non-unitary states and throwback/non-throwback states. These results are limited to Model 3 that includes the control variables *VALADD*, *ENRG*, and *PUB* (although the coefficient estimates for these variables are not presented in the table). While the pooled regressions show that *BURDEN* has a significantly negative impact on *LCAPX* for all groups, the coefficients imply that this effect is larger for unitary and throwback states than for their counterpart non-unitary and non-throwback states, respectively. Although these results are consistent with H3 and H4, the panel regressions that include a control for state fixed-effects show more clearly the hypothesized effects of unitary and throwback tax regimes. Specifically, whereas *BURDEN* has a significant negative effect on *LCAPX* in unitary and throwback states, its effect is positive and/or insignificant in non-unitary and non-throwback states.

With regard to tax incentives, the pooled regressions show that new capital expenditures are increasing (at a decreasing rate) in *INCENT* in both unitary and throwback states, but there is little or no association in non-unitary or non-throwback states. This result is partially reinforced by the fixed-effects regressions, except that in the unitary/non-unitary analysis incentives appear to have more of an impact in the non-unitary states. As a final check on whether the regression results for the unitary/non-unitary and throwback/non-throwback sub-samples are different, we conduct t-tests of differences in the coefficient estimates.

[21] For comparison purposes, we re-estimate the models including in our definition of unitary those states that may require combined reporting when separate reporting does not accurately reflect in-state income. (There is only one state that permits firms to choose combined reporting but does not retain the right to impose combined reporting.) Results are similar, but not as strong. We get the most dramatic results when using the list of unitary states from Williams et al. (2001), but were unable to confirm the basis by which they classify states as unitary.

TABLE 4
Descriptive Statistics and Regression Results for Subsamples of States Classified as Unitary/Non-Unitary and Throwback/Non-Throwback, 1983–1996

Panel A: Descriptive Statistics and Univariate Tests of Differences in Means of New Capital Expenditures (*CAPX*) and the Tax Variables[a]

	Unitary States	Non-Unitary States	Throwback States	Non-Throwback States
Number of States	13	31	26	18
State-years	182	434	364	252
Mean *CAPX*	1,606,999	1,988,524*	1,468,441	2,464,211*
Minimum	34,600	122,500	34,600	185,600
Maximum	14,093,000	41,034,300	14,093,000	41,034,300
Mean *RATE*	7.62	7.13+	6.90	7.83*
Minimum	4.00	2.76	2.76	4.49
Maximum	12.00	13.80	9.82	13.80
Mean *PWT*	29	29+	29.97	27.5+
Minimum	0	0	0	0
Maximum	33	50	50	33
Mean *BURDEN*	2.18	2.05+	2.075	2.11
Minimum	0	0	0	0
Maximum	3.17	4.04	3.168	4.04
Mean *INCENT*	9	10*	9.321	10.25
Minimum	3	4	3	4
Maximum	14	14	14	14

Panel B: Coefficient Estimates from Pooled and Fixed-Effects Regressions for Separate Subsamples of States[b]

Dependent Variable is *LCAPX*; the constant term and the coefficient estimates for the control variables *VALADD*, *ENRG*, and *PUBX* are not shown (t-statistics are in parentheses).

	Unitary(UNA) States	Non-Unitary States	Throwback States	Non-Throwback States
Number of States	13	31	26	18
State-years	182	434	364	252
Cross-Sectional Pooled Model:				
BURDEN	−0.261	−0.188	−0.213	−0.217
	(−2.85)**	(−6.56)**	(−4.15)**	(−5.61)**
INCENT	0.610	0.087	0.721	0.009
	(4.587)**	(1.34)#	(7.42)**	(0.01)
INCENTSQ	−0.029	−0.003	−0.036	−0.001
	(−3.89)**	(−1.00)	(−6.77)**	(−0.33)
Adj. R^2	.76	.77	.74	.75
Fixed-Effects (Panel) Model:				
BURDEN	−0.160	−0.051	−0.172	0.082
	(−2.34)**	(−0.98)	(−3.26)**	(1.31)
INCENT	0.110	0.140	0.118	0.011
	(1.53)#	(2.80)**	(2.11)*	(0.18)
INCENTSQ	−0.002	−0.006	−0.002	−0.0002
	(−0.51)	(−2.28)*	(−0.75)	(−0.07)
Adj. R^2	.96	.94	.95	.95

(*continued on next page*)

TABLE 4 (continued)

[a] * (+) denotes that mean of the first category is significantly less than (more than) the mean of the second category at the .05 level of significance, one-tailed test.

[b] #, * and ** denotes significance at the .10, .05 and .01 levels (one-tailed test where appropriate), respectively. The t-statistics are corrected for heteroskedasticity using White's (1980) standard errors.

States are classified as unitary/non-unitary and as throwback/non-throwback based on CCH's (1982–1996) *Multistate Corporate Income Tax Guide*:

Unitary states (UNA) =	Alaska, Arizona, California, Hawaii, Idaho, Illinois, Kansas, Maine, Minnesota, Montana, Nebraska, New Hampshire, North Dakota;
Non-unitary states =	Alabama, Arkansas, Colorado, Connecticut, Delaware, Florida, Georgia, Indiana, Iowa, Kentucky, Louisiana, Maryland, Massachusetts, Mississippi, Missouri, New Jersey, New Mexico, New York, North Carolina, Ohio, Oklahoma, Oregon, Pennsylvania, Rhode Island, South Carolina, Tennessee, Utah, Vermont, Virginia, West Virginia, Wisconsin;
Throwback states =	Alabama, Alaska, Arizona, Arkansas, California, Colorado, Hawaii, Idaho, Illinois, Indiana, Kansas, Maine, Massachusetts, Mississippi, Missouri, Montana, Nebraska, New Hampshire, New Mexico, North Dakota, Oklahoma, Oregon, Utah, Vermont, West Virginia (throw-*out* rule), Wisconsin; and
Non-throwback states =	Connecticut, Delaware, Florida, Georgia, Iowa, Kentucky, Louisiana, Maryland, Minnesota, New Jersey, New York, North Carolina, Ohio, Pennsylvania, Rhode Island, South Carolina, Tennessee, Virginia.

These tests confirm that the differences in the coefficient estimates in the regression models between unitary/non-unitary and throwback/non-throwback states are statistically significant.

Sensitivity Tests

Although the Chow tests of a structural break support analyzing the unitary/non-unitary and throwback/non-throwback subsamples separately, these specifications are inefficient because of the smaller sample sizes. Hence, we also estimate the regression models for all states together as in Table 3, but including a dummy variable for unitary (throwback) states and interacting that dummy with *BURDEN*, *INCENT*, and *INCENTSQ* (the dummy is omitted in the fixed-effects specifications). The results (not tabulated) yield similar insights as shown in Panel B of Table 4—the coefficients on the interactions between the unitary dummy and *BURDEN*, *INCENT*, and *INCENTSQ* are significantly negative, positive, and negative, respectively, indicating that tax factors are more influential to the capital investment decision in unitary states. The use of a throwback dummy and interactions yields similar results as the unitary dummy.

It could also be argued that the interaction between unitary and throwback tax regimes is relevant because unitary states that also impose the throwback rule likely have the most onerous income tax burden on property, whereas the non-unitary/non-throwback states have the least onerous. Descriptive statistics are consistent with these expectations—a 2×2 classification (not reported) shows that new capital expenditures are lowest in states that are both unitary and throwback and highest in states that are neither unitary nor throwback. However, there is a substantial overlap in the states that apply the two tax rules. Specifically, 12 of the 13 (92 percent) unitary states also impose the throwback rule, and 17 of the 18 (94 percent) non-throwback states are also non-unitary. Thus, any attempts to include both unitary and throwback dummies and interactions in a full-sample model are unsuccessful. The high correlation between the two classifications prevents a reliable identification of their separate and distinct effects.

CONCLUSIONS

Our results, based on state-aggregated data from 1983–1996, consistently suggest that the state corporate income tax burden on property has a statistically significant negative effect on new capital expenditures by corporations in the manufacturing sector, whereas the number of available investment-related tax incentives has a significant positive, though declining, influence on incremental capital spending. In further analysis that takes into account tax-base differences among the states, we find that these effects are more pronounced in the subsample of states that impose unitary taxation or the throwback rule. Thus, although our study provides evidence that firms tend to locate property in states where they are subject to lower income tax burdens, our results suggest that, on a relative basis, the advantages of regimes that are non-unitary or do not employ the throwback rule may well offset the effects of any differential tax rates, apportionment formula weights, or general investment-related tax incentives. Triangulating our empirical findings with prior analytical and simulation studies (Papke 1996; Williams et al. 2001) suggests that the unitary and throwback requirements are most influential on the location of capital investment, followed by apportionment weights and tax rates, and investment-related incentives have the least impact.

While our results are robust to making comparisons across states or within states over time, the magnitude of these responses is modest, at best; that is, regardless of statistical significance, the economic significance of our coefficients is almost negligible. It is likely, however, that the magnitude effects are understated—in part because of the fundamental characteristics underlying property investment decisions and also because of certain research design limitations. First, property, by nature, is not very mobile; once established, a firm would require large tax incentives to overcome the costs of relocation. In addition, since large capital expenditures often are budgeted for years in advance, the timing of investment decisions is likely influenced by past rather than current tax factors. While we only lagged the control variables, there may also be a considerable lag in firms' reactions to changes in tax policy.

Second, our variable measures are not without noise. Specifically, the incremental capital expenditures variable includes routine replacements and additions, which are generally tied to the location of existing property and dilute the role tax factors may play in such decisions. Similarly, a simple count of tax incentives available in a state does not measure the construct with much precision. Third, because most capital decisions are made at the firm level, our use of state-level aggregated data potentially biases against finding significant results.[22] Finally, we assumed that tax burdens vary exogenously across jurisdictions. It has been argued, however, that state tax rates are likely endogenous since, through the competition for capital, they depend on unobserved state characteristics (Knight 2001). Although our fixed-effects models control for such characteristics to the extent they do not vary over time, the use of instrumental variables could further cure any remaining endogeneity concerns.

Notwithstanding these limitations and even allowing for empirical procedures that may overcome them, it appears that the magnitude of the relationship between new capital expenditures and state income tax factors is unlikely to become economically significant. Yet we believe that this is an important finding because it shows that the economic impact

[22] In a recent study using firm-level data from Georgia, which switched from an equally weighted three-factor formula to a double-weighted sales formula, Edmiston and Arze (2002) find the tax elasticities to be higher. Specifically, they estimate that the average firm increased its property in Georgia by 5.05 percent following the switch.

of changing state corporate income tax variables is not as large as policymakers might expect. Therefore, our results suggest that state tax policymakers should think carefully about using the corporate income tax code as the means for stimulating economic growth, especially in non-unitary and non-throwback states. Although we did not specifically examine the potential impact on state tax revenues, it seems very possible that any incremental capital investment resulting from a lowering of the state corporate income tax burden on property might be outweighed by the accompanying loss in tax revenues. Altering the apportionment formula or changing tax rates has broad-based implications for all firms doing business within the state. As the number of states employing apportionment formulas more heavily weighted toward sales increases, these apportionment formulas no longer provide the same level of incentives to corporations. If, as the literature on tax competition suggests, states are altering their apportionment formulas and/or rates to remain competitive with other states that have already done so, then our results reflect the ''race to the bottom'' characterized in other studies (Goolsbee and Maydew 2000). In the long run, a new equilibrium will be reached where all states are collecting less corporate income tax revenue and potentially providing fewer public goods as a result.

Similarly, investment-related tax incentives may also not play as large a role in aggregate economic growth as the political rhetoric might suggest. Since we do not have firm-level data, we cannot speak to the impact that targeted tax incentives have on individual firm decisions. While such incentives when limited to new businesses can contain revenue losses, the downside is that policymakers may have to contend with claims of inequity from existing businesses. Indeed the incidence of such controversy over tax fairness has reached heightened levels in recent years (Brunori 1997; Burstein and Rolnick 1996). Clearly, the economic benefits to be gained from business growth must be weighed against the costs of offering major tax incentives.

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