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DEDICATED TAXES AND RENT CAPTURE BY PUBLIC EMPLOYEES

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Introduction

This paper tests for evidence of rent capture by public-sector employees when a dedicated tax for a local public service is enacted. The use of special districts for the provision of specific public services has grown increasingly common in recent years. These districts provide services such as water, sewer, electric, or transit and typically generate some revenue in exchange for provision. When user fees do not cover costs, however, public funding from federal, state, and local governments makes up the difference. This research reports that alternative funding mechanisms and budgeting practices appear to provide different opportunities for public-sector unions to obtain higher wage rates and payrolls.

Two distinct types of local public funding are considered: general revenues and dedicated taxes. Districts receiving funding from general revenues typically compete with other districts, departments, or agencies through a budget process for a limited pool of funds raised by traditional taxation methods. In contrast, agencies that receive earmarked revenues from a dedicated tax are assured of funding without the need to justify their budget or level of service. Their funding falls outside the traditional local budget process, which is characterized by the comparison of costs and benefits of competing uses of public funds. This lack of competition potentially yields increased political and budgetary autonomy.[1]

Proponents of earmarked revenues hold that this type of funding permits long-term planning and more efficient operation. Managers who are not concerned with competing for funds on an annual basis can take a longer-term view with respect to capital budgets, planned expansions, and other operations.[2] Nevertheless, the permanent nature of the funding and the lack of checks and reviews through a normal budget process also creates opportunities for rent capture by public-sector employees and unions through higher wages, fringe benefits, and staffing levels. These higher costs potentially offset any efficiency gains from earmarking and argue for the use of traditional budget and financing methods.

This paper tests whether the enactment of a dedicated tax leads to higher payroll and wages for public employees. Data from the Section 15 reporting system administered by the Urban Mass Transportation Administration (UMTA) provide payroll and wage levels for a homogeneous type of employee--bus operators--for 165 public transit systems over the period 1982-1985. By 1985, 64 of these systems used local dedicated revenue sources to support their operations. I independently collected information on when these taxes were enacted and the historical circumstances leading to their enactment. Eight were enacted during the sample period, while the other 56 were enacted during the 15 years prior to 1982.

In the sample,'average hourly wage rates for public-sector operators rise from a pre-tax level of $7.97 to more than $10.00 in the two-year period following a tax enactment, and subsequently remain well above the average of $8.59 per hour for unionized systems with no dedicated taxes. In further analysis, pooled time-series, cross-section regressions provide a systematic look at the level and time path of payroll and wages following a tax enactment. The level of dedicated revenues and the time since tax enactment are used as independent explanatory variables, with further controls made for local private-sector wage rates, system size, unionization, and demographic characteristics.

A potentially serious objection to the analysis is that the existence of a dedicated tax may be endogenous to the local wage process. The history of these taxes in the local mass-transit industry, however, is discussed in detail and strongly suggests that the principal determinant for the existence of dedicated taxes for transit are state-level policies for transit funding. A more plausible channel for bias is the presence of unobserved fixed effects that are correlated with both public-sector wages and dedicated taxes, such as union strength, the voting power of public employees, tastes for good or bad government, or the local political' environment. The empirical analysis thus explicitly tests and controls for fixed effects.

The econometric analysis uses two procedures. The first is a generalized least squares **(GLS)** procedure that controls for cross-section heteroscedasticity and correlation of errors across time. The GLS results suggest that there are significant and permanent increases of 8 to 10 percent in payroll and wages following the enactment of a tax. These results, however, are potentially biased due to unobserved fixed effects that are correlated with both wages and the presence of a dedicated tax. To control for fixed effects, I use the standard "fixed-effects" or "within-groups" estimator, differencing the cross-section observations from their individual means and running ordinary least squares **(OLS)** on the transformed data.

Hausman specification tests strongly reject the null hypothesis of no correlated fixed effects. The fixed-effect results, however, suggest an even larger impact on payrolls and wages than do the GLS results. Payroll and wages rise a statistically significant 20 percent in the six years following the tax enactment, and this increase remains stable over time.

Simulations of the extent to which the payroll increase represents the capture of additional revenues resulting from the dedicated tax suggest that immediately following the enactment of a dedicated tax, transit systems expand significantly in size, revenues, and payroll. But while the higher size and payroll are stable over time, the additional revenues gradually diminish to the point that no significant difference exists after 14 years. Holding system size constant, the results show that total revenues are significantly higher in the five-year period following the tax enactment, but decrease steadily from an initial upward shift in the first year. Payroll, however, rises 20 percent in the five years following tax enactment, then drifts upward to a 30-percent higher level by year 10. Thus, the payroll share of revenues initially drops with the surge of new revenues. But as payroll increases, the payroll share then rises from 27 percent at year one to almost 40 percent by year 15 of the tax. In sum, the results suggest that. increases in labor costs eventually absorb all additional net revenues that result from enactment of a dedicated tax, and that these gains are maintained in spite of a gradual falloff in revenues over time.

The paper is organized as follows. Section I reviews previous work on ability-to-pay and rent capture and discusses how a dedicated funding source increases the potential for rent capture. Section II provides evidence on trends in the use of special districts and dedicated taxes nationwide. Section III discusses the data, including survey results on the use of dedicated taxes in the local mass-transit industry. Section IV presents the econometric evidence on changes in payroll and wage rates following the enactment of a dedicated tax. Section V provides additional econometric and simulation results on changes in size, revenues, and payroll share following tax enactment. Finally, section VI summarizes the conclusions and suggests areas for future research.

I. Abilitv-to-Pav and Rent Capture

Abilitv-to-Pay

The labor and industrial relations literature includes numerous studies on the effect of "ability-to-pay" on wage levels. Early studies hypothesized that, other things being equal, the managers of firms in less-competitive/highly profitable industries paid higher wages in order to lower labor turnover rates, enhance their public image (Weiss, 1966), improve worker morale (Slichter, 1950), or assure a queue of available workers who can be hired to meet increased product demand (Ross and Wachter, 1973). Such high wages in less-competitive industries, however, can also be interpreted as a capture of economic rents by labor. Furthermore, workers' gains from unionization are potentially higher in less-competitive industries because of the presence of rents to be captured and because of lower threat of entry by nonunion firms.

Empirical studies of labor's ability to share in any excess return due to product-market power employ cross-industry comparisons and have produced mixed results. Early analyses using industry concentration as a measure of market power include Rapping (1967), Masters (1969), Haworth and Rasmussen (1971), and Ashenfelter and Johnson (1972). These studies find no statistically significant relationship between market concentration and wages. In contrast, Pugel (1980) uses profits as a measure of concentration and finds that labor receives 7 to 14 percent of the total excess return. More recent studies by Clark (1984), Ruback and Zimmerman (1984), and Salinger (1984) also find significant evidence of union rent-sharing using cross-section data on profits of firms and lines of business. Finally, several studies suggest that union workers capture economic rents created by industrywide regulations.[3] Rose (1987), for example, finds significant declines in union wages resulting from deregulation in the trucking industry. In sum, a substantial body of empirical evidence suggests that private-sector unions are able to capture economic rents created through monopoly power and regulation.

Public-Sector Rent Capture

The analysis of rent capture also has a strong tradition in the public-choice literature. Niskanen (1971, 1975) posits that a bureaucracy maximizes the level of service it provides (and hence the size of its budget) subject to its production constraints and to the resources allocated by its political superiors. Since an agency negotiates with political leaders over a total budget as opposed to incremental units of service, and since the agency is often the sole provider of the service, it can use its monopoly power to establish a level of service greater than that desired by voters. While the service-maximizing model implies that bureaucrats minimize production costs per unit of service, models by Migue and Belanger (1974) and Orzechowski (1977) explicitly recognize that bureaucrats desire higher wages, fringe benefits, and staff levels and may use their monopoly powers to obtain them.

Public-sector unions may share in bureaucratic rents in the same way that private-sector unions share economic rents. Empirical studies, however, generally show that public-sector unions have a more moderate effect on wages than do their private-sector counterparts. Freeman's (1986) literature review suggests a public-union wage premium on the order of 5 , percent. Gyourko and Tracy (1988) control for endogenous choice of both government and union status and find a 4 percent public-union wage differential, versus 14 percent in the private sector. Although the ability of public unions to raise wages appears limited, Gyourko and Tracy also find (in a forthcoming article) lower property values in cities that pay their public-sector workers relatively high wages. This suggests that public workers' success in raising wages can be interpreted as rent capture from a community.

Earmarking and Rent Capture

The size of the overall governmental budget in Niskanen-type models is larger than socially optimal because budgetary procedures allow bureaus to act as price-discriminating revenue maximizers. However, the ability to use this market power is constrained by competition from other bureaus and by the preferences of legislative committees. Other constraints on local government spending, pointed out by Courant, et al. (1979), include voters' direct referenda on tax collections, and potential mobility to jurisdictions offering alternative expenditure-taxation packages.

The earmarking of revenues directly affects most of these constraints on public employee market power. First, the lack of alternative uses for earmarked revenues weakens the negotiating position of a political authority in relation to the designated receiving agency. Second, many dedicated taxes are virtually permanent because periodic voter reapproval is often not required and because the costs of repeal are high.[4] Permanent, exclusive access to funding should lower the variance of funding and therefore raise the expected present value of dedicated tax revenues as compared to general revenues. Finally, since most dedicated taxes accrue to regionwide special districts, voting with one's feet entails a relatively high-cost move: that is, to another metropolitan area, not just to a neighboring municipality.

II. Trends in Special Districts and Dedicated Taxes

Special districts are the fastest-growing form of local government in the United States and represent an increasingly important method of providing specific local public services, such as water, sewers, airports, parks, and mass transit. As shown in table 1, the number of special districts nationwide doubled from 14,405 in 1957 to 28,719 in 1982. They now account for 35 percent of all governmental units--including states, counties, municipalities, and school districts. Revenues in real terms rose nearly 500 percent in the same period, totaling $31 billion in 1982. Although fees collected from users comprise the largest share of revenues (64 percent in 1982), local dedicated taxes are historically the largest component of state and local assistance and are surpassed only by federal assistance in the late 1970s.

While the share of revenues accounted for by local dedicated taxes nationwide is not large overall, those districts that have special tax authority depend heavily on it for revenue. As shown in table 2, in 1977--the last year in which dedicated tax authority status was reported--districts with tax authority were similar in budget size to districts without tax authority, but received 25 percent of their total revenues from dedicated sources. While this was down from the 33 and 30 percent shares in the 1967 and 1972 census years, respectively, it represents the dominant source of public assistance in these districts. There is also general evidence of rent capture in districts with tax authority. Wages and salaries accounted for 30.9 percent of revenues in these districts as opposed to a 25.9 percent share in districts without tax authority.

While tax authority status by district was not reported in 1982, a rank ordering of special districts by states in terms of the statewide use of local dedicated taxes reveals a similar pattern. As shown in table 3, for special districts in states with the highest use of these taxes, quintile 5, dedicated revenues accounted for 25.1 percent of revenues, versus 0.3 percent for the lowest quintile. Wages and salaries represent a much higher portion of revenues in states where the use of dedicated taxes is widespread, accounting for 30.2 percent in the highest quintile versus an average of 20.5 percent in the lowest three quintiles, again suggesting that the use of these taxes presents opportunities for rent capture.

In summary, special districts are an increasingly common form of government, and dedicated taxes are the largest source of public funding for these districts from the state and local sector. In states in which these taxes are predominantly used, revenues from dedicated taxes exceed federal assistance as well. Wages and salaries in districts with dedicated tax authority on average constitute a higher share of revenues compared to districts without tax authority. This evidence of rent capture is seen in government censuses from 1967 through 1982.

There are several explanations other than rent capture, however, for the differences in wage shares. The heterogeneous output mix across special districts could produce the observed correlation if districts using dedicated taxes tend to produce output that requires labor-intensive technology. Or, districts with dedicated taxes may employ higher-skilled and higher-quality workers. Finally, districts that use dedicated taxes could be in higher-wage areas. To account for these factors, the analysis will control for prevailing wage rates and examine wages in districts producing a homogeneous output and employing homogeneous employees--bus drivers in local mass-transit systems.

III. Dedicated Taxes and Mass Transit

The local mass-transit industry is the focus of the empirical analysis for several reasons. First, the production processes involved are relatively homogeneous and their inputs (labor hours and vehicle miles) are measurable, facilitating comparisons of cost efficiency across transit providers. Second, the employees of these systems are also relatively homogeneous. In particular, it is assumed that the human capital of bus drivers is similar across transit systems. Finally, transit districts receive revenues from a wide variety of sources: fares, federal operating assistance, state and federal capital grants, local general revenues, and local dedicated taxes. This heterogeneity permits control for variations in operating conditions and measurement of the impact of revenue sources and institutional settings on wage rates. In particular, I look for evidence of wage changes that are systematically related to the enactment of a local dedicated tax.

Data

The data source for this work is the Section 15 Reporting System administered by UMTA. Section 15 of the Urban Mass Transportation Act (UMT Act) establishes a uniform accounting system for public mass-transportation finances and operations. All applicants and direct beneficiaries of federal assistance under Section 9 of the UMT Act are subject to this system and are required to file annual reports with UMTA.

Section 15 data for fiscal year (FY) 1979 through FY 1985 are available for some 435 transit systems and include detailed information on revenue sources, expenses, employees, and hours and miles of service provided. These data provide an extremely detailed view of a cross-section of local government entities that perform similar activities. The revenue data are broken down into revenues from both transit operations and public subsidies, including information on federal, state, and local contributions for operations and capital procurement. State and local revenues are broken down into those from dedicated taxes versus general revenues.

The expense data include information on payroll, fringe benefits, materials, and services for the areas of administration, operations, and maintenance. Data on labor hours for types of employees are provided as well. Using the expense and employee data, average hourly pay rates can be constructed for the different types of employees. Operating statistics include data on passengers, vehicle miles, and vehicle hours. The detailed data on employee hours, payroll, fringe benefits, and local revenue sources are of particular interest for this work.

Payroll and employee hour data were obtained for a homogeneous type of employee--bus operators--for 165 public transit systems from 1982-1985. The sample was limited to systems that operated only bus service (as opposed to subway, commuter rail, ferryboat, etc.), that operated at least five vehicles, that did not contract out service, and that provided complete information for all years of the survey.[5] These payroll and wage data were combined with revenue data to look for evidence of rent capture from dedicated taxes.

Survey Results on Dedicated Taxes

By the end of the sample period, 64 of the systems reported having local dedicated revenue sources to support their operations. The UMTA data report the exact dollar value of monies spent from dedicated sources, but provide little institutional information on the type of tax used. For this research, I have supplemented the UMTA data through a telephone survey of the managers or staff of transit systems reporting dedicated revenues. This information--the type of tax, year the tax was enacted, and historical circumstances surrounding the tax enactment--is summarized in table 4.

Property taxes were the most common type of tax observed (28 of 64 systems), followed by gasoline excise (17), sales (14), and payroll taxes (5) California, which accounts for 15 of the observed gasoline taxes, enacted a gasoline tax in 1972 that is administered by the state but returns money to the local level for transit based on the money collected in that area. For the purposes of this study and for the UMTA statistics, these funds are assumed to be local dedicated revenues.[6] Eight of the dedicated taxes were enacted during the sample period and the other 56 during the 15-year period prior to 1982. A majority were enacted between 1967 and 1973, when federal capital assistance was being provided for the creation of publicly owned and operated transit systems. Only five of the taxes observed required periodic voter approval, and none of the taxes was repealed during the sample period.

A potentially serious objection to the analysis is that the presence of a dedicated tax is the result of high public-sector wages. However, the process by which most of these taxes were enacted renders this possibility tenuous at best.

The historical circumstances surrounding the enactment of these taxes in the local mass-transit industry strongly suggest that funding policies at the state level were the major determinant of the existence of dedicated taxes for transit. In the late 1960s and early 1970s, the federal government provided capital assistance for the establishment of public transit systems subject to the provision of matching funds and operating assistance by state and local governments. States with established subsidy programs for transportation met the federal requirements from existing funding sources at the state level and from local general revenues. Many states without existing funding sources, however, encouraged the formation of special districts with taxing authority to meet the federal requirements. Thus, most of the taxes observed in this period were established when the district was created.

Transit systems in Pennsylvania--whose state government plays an activist role in mass transit--are barred by state law from enacting dedicated taxes. In contrast, neighboring Ohio provides no state funding for mass transit but allows voter referenda to grant new local transit systems the authority to enact taxes. Of the sample of eight transit systems in Ohio, seven had dedicated taxes. No local dedicated taxes exist in New York and Wisconsin, both of which have extremely generous state programs for mass transit.[7] Rural-dominated state legislatures in Texas, Kansas, Kentucky, Missouri, Oregon, and Washington, however, provide authority for the use of local dedicated taxes because of their aversion to providing financial support for inner-city bus service. (There are reportedly also anti-union and racial motivations to some of these decisions, since most transit systems are unionized and a disproportionate number of riders are black.)

Taxes enacted after 1974 typically were instituted after the transit system had been publicly owned and briefly supported by local general revenues. Circumstances surrounding these taxes are varied, but apparently the drain on local general revenues for operating assistance was larger than originally expected and new funding mechanisms were required. This was especially true in areas desiring to expand their systems. Groups supporting the expansion of transit and dedicated taxes in general included downtown business interests; transit-dependent populations such as the poor, handicapped, and elderly; and transit unions.

Finally, small, city-run systems in the South typically did not use dedicated taxes. The service areas of these systems were historically covered by the bounds of the city government, eliminating the need for a regional agency or special district. States reporting such service were North Carolina, Georgia, Alabama, Tennessee, and Mississippi.

Preliminary Empirical Evidence

Sample means (and standard deviations) for system size, revenues, and vehicle operator wage rates in 1985 are reported in table 5. The systems with dedicated taxes are, on average, three times as large as other systems, in terms of both miles of service delivered and number of vehicle operators. Revenues, however, are four times as large on average, suggesting that systems with dedicated taxes have larger budgets relative to service provided. A breakdown of revenue composition implies that the use of dedicated taxes precludes the use of local general-revenue sources. Among the dedicated-tax systems, earmarked revenues provide 40.4 percent of total funding, while local general revenues account for 1.9 percent. Systems without dedicated taxes, however, receive 23.7 percent of their funding from local general revenues and only 0.1 percent from earmarked sources.[8] Thus, these two groups of transit systems exhibit extreme differences both in local funding mechanisms and in composition of revenues, suggesting that comparison of wages and payroll between the two groups is a useful natural experiment.

Average wages paid to vehicle operators were 16 percent higher in systems using dedicated taxes versus those without. This finding provides only prima facie evidence of rent capture, because several other explanations exist for this differential. First, dedicated-tax systems may have a higher rate of unionization than nondedicated-tax systems. Second, operators in dedicated-tax systems could be higher-quality, more productive workers. Third, dedicated taxes may be more common in high-wage areas. Finally, there could be several unobservable fixed effects correlated with both high public-sector wages and the use of dedicated taxes: for example, strong public-sector unions, tastes for good or bad government, and political structure.

These concerns are addressed in turn below. In the case of union status, I found that 81 percent of the nondedicated-tax systems were unionized versus 97 percent of the dedicated-tax systems. Union status can be controlled for through the standard use of a dummy variable. No information is available, however, on the human capital of the public vehicle operators. I used the number of collisions and fatalities involving transit vehicles as measures of driver quality, but they had no significant explanatory value and do not affect the estimates presented here. I am left to assume that the quality of bus drivers is homogeneous across transit systems.

To measure regional variation in private-sector wage rates, I ran a standard human-capital wage regression on the universe of private-sector workers in the Current Population Survey, controlling for industry, occupation, and 94 distinct geographic areas interacted with union status. The average human-capital measures for motor vehicle operators were then used with these results to project a union and nonunion wage rate for vehicle operators in each geographic area. This procedure was repeated for each of the four years in the sample period. These estimated private-sector wages were then used as independent explanatory variables for vehicle operator wages in public transit systems.

Sample means for the private-sector wages and other demographic variables are reported in table 6. Wages average 4.6 percent higher for nonunion, private vehicle operators in areas using dedicated taxes versus those without, explaining part of the differential observed in public wages The areas using dedicated taxes also have higher populations, density, incomes, and population growth. Areas without dedicated taxes have higher poverty rates and a higher percentage of black population, reflecting the use of city financing in the South. No significant difference is observed in transit demand in these areas as measured by the percentage of people who drive to work versus take mass transit.

In general, the sample means suggest that wages are higher among transit systems that use dedicated tax rates. Vehicle operators in the dedicated tax systems receive, on average, a 12.3 percent, or $1.07 per hour, premium above the private-sector wage for nonunion drivers. Operators in systems without dedicated taxes receive a premium of only 1.2 percent, or $0.10 per hour. These statistics, of course, do not systematically control for demographic variables, unionization, economies of scale, and other observable variables influencing wage rates. Finally, as already discussed, a series of possible unobservable fixed effects associated with dedicated taxes and public-sector wages are also not controlled. The empirical analysis of the next section takes into account both these observable independent variables and unobservable fixed effects in order to test the robustness of this evidence of rent capture.

IV. Payroll and Wane Changes after Tax Enactment

While the sample period in this study is only four years, the dedicated taxes were enacted over a 17-year period. I exploit this variation in the age of taxes in order to make inferences regarding the time path of payroll, wages, and revenue following tax enactment. To this end, I construct a variable called YEAR that equals the number of years that have passed since a tax was first enacted. The values of YEAR for a system with a tax enacted in 1980, for example, are 2, 3, 4, and 5 for the 1982-1985 sample period years, respectively.

Table 7 reports the sample means (and standard deviations) for payroll, public operator wage, private nonunion operator wage, and system size. The means for systems without dedicated taxes are broken down by union status. For dedicated-tax systems, the means are broken down by the values of YEAR observed during the four-year sample period. There are 404 time-series cross-section observations for the nondedicated-tax systems, and 256 such observations for the dedicated-tax systems. Values are reported in 1985 dollars. Caution is needed in interpreting these results, as individual dedicated-tax systems appear in four different year groups, introducing correlation among the sample values.

The sample means do not suggest that dedicated taxes are enacted in predominantly high-wage areas or in systems that already have high wages. The average public wage in the pre-tax observations is $7.97 per hour versus an average $8.59 per hour for the unionized nondedicated-tax systems. Compared with the private-sector wages, these wages represent $0.20 and $0.55 premiums, respectively, suggesting that unions in the systems initially without dedicated taxes were not doing as well as their peers. In the year following tax enactment, however, the means suggest that public-sector wages jump to $9.17 per hour, a $1.13 premium, and peak at $10.55 per hour by year 4, a $2.34 premium. While the wages and premiums vary in the ensuing years, they remain above the average for the unionized nondedicated average and are above $9.00 in all years but 7 and those over 16. Examination of payroll per mile of service shows a 34-percent jump following the enactment of the dedicated tax.

In general, these sample means provide further evidence that enactment of a dedicated tax leads to rent capture by the vehicle operators Whether measured in payroll or wages, labor costs rise substantially in the years immediately following the enactment of the tax and remain above the labor costs existing in systems without dedicated taxes over time. These statistics, of course, do not systematically control for demographic variables, unionization, economies of scale, private-sector wages, and other observable variables influencing wage rates.

The econometric analysis uses pooled cross-section regressions with vehicle operator payroll and wage rates as dependent variables. Wage rates are a commonly used measure of union success in delivering rents to workers, but are probably not the best measure in this study for two reasons. First, payroll measures reflect both wage rates and a union's ability to expand membership. Thus, comparing payroll changes with revenue changes stemming from a new tax provides a more complete measure of rent capture. Second, the wage rates used below were constructed from the payroll data and data on employee labor hours. While the payroll data come from balance sheets and are reportedly accurate, labor hours are more poorly measured, potentially introducing error into the wage measures. For these reasons, the discussion will focus principally on the payroll estimates, although the wage regressions are also reported and deliver qualitatively similar results.

The basic equation for estimation, shown in equation (1), uses the log of public operators' payroll (wage) as the dependent variable as follows:

(1) $$LPAY_{it} = \alpha + \beta_1 LSIZE_{it} + \beta_2 LPRIWAGE_{it} + \beta_3 DEDREV_{it} + \beta_4 DEDREV_{it} * DEDREV_{it}$$

$$+ \sum_{j=1}^{17} \beta_{j+4} YEARj_{it} + \beta_{22} DUM83_t + \beta_{23} DUM84_t + \beta_{24} DUM85_t + \sum_{k=1}^{6} \gamma_k X_{ki} + \epsilon_{it}$$

$(i = 1, \ldots, 165;\ t = 1, \ldots 4)$.

Time-variant explanatory variables include the log of miles of service provided (LSIZE), the log of the private-sector nonunion wage (LPRIWAGE), and sample-year dummies (DUM83, DUM84, DUM85). The impact of a dedicated tax is measured by the dollar amount of dedicated revenues collected per mile (DEDREV), DEDREV-squared, and a length j vector of YEARj dummies, j=1,...,17, measuring time since tax enactment. YEAR5 equals one, for example, when YEAR=5. The YEARj and DEDREV variables are set to zero for systems with no dedicated taxes. Finally, the specification includes a vector of six variables measuring union status and area demographics, which were not observed to change over the sample period. (In the case of the demographic variables, this was due to data limitations.) The variables collected for each system area were union status, log of population, log of population density, percent change in population between 1980 and 1984, percent of black population, log of per capita income, and the poverty rate.[9]

Two procedures are used. The first is a generalized least squares (GLS) procedure that controls for cross-section heteroscedasticity and time-wise first-order correlation of errors as discussed in Kmenta (1986). In the absence of correlated fixed effects, this procedure is efficient. If fixed effects are present and correlated with the independent variables, however, the GLS estimates are inconsistent. The second procedure used is the standard fixed-effects (FE) or "within-group" estimator discussed in Hausman and Taylor (1981). The cross-section observations are differenced from their individual means, and ordinary least squares (OLS) is run on the transformed data. All time-invariant variables are eliminated. In the absence of correlated fixed effects, the FE estimator is consistent but inefficient. If fixed effects are present, however, the FE estimator still yields consistent results. Hausman specification tests are then conducted to test the null hypothesis of no correlated fixed effects.

Generalized Least Sauares Results

Results for the GLS payroll regressions are reported in table 8a. In equation (1), which omits the dedicated tax variables, LSIZE has an estimated coefficient (standard error) of 0.981 (0.014), suggesting that payroll has a scale elasticity of nearly one. LPRIWAGE has an estimated coefficient of 0.455 (0.086), showing that nearly half of the variation in private-sector wages is reflected in the public-sector payroll wage. When the demographic variables are omitted (not shown here), the private wage coefficient is 0.578 (0.091). While my prior expectation was a relation closer to one, restricting the coefficient to one did not substantially affect the results that follow. Standard errors increased, but the estimated coefficients changed little and were still significant.[10]

The estimated coefficient for the union dummy is 0.325 (0.28), suggesting that unions raise payrolls by 38 percent. This is a large effect compared to most estimates of private and public union wage premiums. It reflects in part the use of low-wage, part-time operators by nonunion systems and, to the extent that the size and private wage variables are not perfect controls, the small size of these systems and their concentration in low-wage areas of the South. It also implies, however, that transit unions are in general successful in capturing rents for their members. Finally, the time dummies show an upward trend in wage rates and, among the demographic variables, population and density have positive and significant effects on payroll, while percent black, poverty, and income have negative effects.

Equation (2) in table 8a measures the impact of the dedicated tax on payroll by entering the level of dedicated revenues per mile, DEDREV, and DEDREV-squared into the specification. The estimated coefficient for DEDREV is 0.0640 and is statistically significant at the 99-percent confidence level with a t-statistic of 4.64. The estimated coefficient (standard error) for DEDREV-squared is -0.0023 (0.0026). Evaluated at the average value of DEDREV for dedicated-tax systems in 1985 ($1.33 per mile), these estimates suggest that use of a dedicated tax increases operator payrolls by 8.4 percent.

Equation (3) of the table omits the level variables but includes the YEAR dummies in order to explore the time path of payroll following tax enactment. Payroll is significantly higher in YEAR3, YEAR4, and YEARS, with estimated coefficients of 0.082, 0.100, and 0.084, respectively. The estimated effect diminishes in YEAR6 and YEAR7 (where there are few observations), but returns to the 0.07 to 0.10 range for the following years and remains statistically significant through YEAR15. These estimates suggest that payroll rises 7 to 10 percent in the years immediately following a tax enactment and that these gains are relatively stable over time.

Equation (4) of table 8a controls for both the level of revenues and the time path. The estimated coefficients for the level variables change little from equation (2), and the estimate for DEDREV remains statistically significant with a t-statistic of 2.90. The YEAR dummies, however, no longer reveal a significant change in payroll over time. This result changes in the fixed-effects (FE) estimates, which I will now discuss.

Fixed-Effects Estimation Results

Results from the FE estimation are reported in table 8b, with equations (5) through (8) corresponding to equations (1) through (4) of the GLS estimates. Equation (5) again omits all dedicated-tax variables. The estimated coefficient (standard error) of the size variable is 0.711 (0.026). This is below the GLS estimates and suggests that economies of scale with respect to labor costs exist within transit systems. The private-sector wage variables and the time dummies, however, change little from the GLS estimates, and the time-invariant demographic and union variables, of course, are omitted.

The FE results for the dedicated-tax variables remain statistically significant and increase in magnitude from the GLS estimates. The estimated coefficient on DEDREV in equation (6), which omits the year dummies, is 0.0947 and has a t-statistic of 4.53. This suggests that the average dedicated tax of $1.33 per mile raises payroll by 12.1 percent. When the YEAR dummies are included instead of the level variables, however, allowing for payroll growth over time, the estimated increase is much larger As reported in equation (7), payroll grows by 22 percent in the four-year period following tax enactment, remains 20 to 23 percent higher through year 7, then increases to a 33 to 39 percent higher level in years 9 through 15, before falling off in years 16 and beyond.

Equation (8) includes both the DEDREV variables and YEAR dummies. The estimated coefficient for DEDREV is 0.0607, which is little changed from the GLS estimates, and has a t-statistic of 2.28. The YEAR dummies, however, in contrast to the GLS results, are statistically significant and reveal growth in payroll in the early years of the dedicated tax. For the average dedicated tax, the results suggest that payroll grows by 21 percent during the first four years following tax enactment, then reaches a 30- to 34-percent higher level in years 10 through 15.

Note that the FE estimates of the long-term impact of a tax rely on observed payroll changes occurring within individual systems during the four-year sample period. Because taxes were enacted at different times, an estimate of the cumulative impact of a tax lasting J years is feasible. The FE estimate for a tax lasting J years links together payroll changes observed in individual systems with taxes of age 1 through J. For higher values of J, therefore, the standard error of the total estimated change grows. This increasing imprecision is seen in the steady growth of standard errors of the FE estimates of the YEARj coefficients for higher values of YEAR. Indeed, for J > 15, the estimated payroll change is not statistically significant, although the point estimate is still large. Greater confidence can thus be placed in the projections for the early years of a dedicated tax, particularly those immediately following tax enactment.

Specification Tests

In the absence of correlated fixed effects, the GLS estimates are efficient, while the FE estimates are inefficient but consistent. If correlated fixed effects are present, however, the GLS estimates are inconsistent, while the FE estimates remain consistent. Hausman (1978) shows that a specification test can be constructed by comparing the two estimators. Under the null hypothesis of no correlated fixed effects, the difference in the estimated β's, $\beta_{FE} - \beta_{GLS}$, should be small relative to the difference in the covariance matrix of the estimates, $V_{FE} - V_{GLS}$. The Hausman test-statistic M, shown in equation (2) below, is distributed χ^2_q under H_0, where q is the number of potentially biased coefficients tested.

(2) $$M = (\beta_{FE} - \beta_{GLS})(V_{FE} - V_{GLS})^{-1}(\beta_{FE} - \beta_{GLS}) \sim \chi^2_q$$

Hausman tests of the GLS specifications (1) through (4)--FE specifications (5) through (8)--reject the null hypothesis of no correlated fixed effects at the 99.5-percent confidence level. The test statistics (critical values) are 125.2 (16.8), 131.0 (20.3), 49.8 (44.2), and 156.4 (46.8), respectively. The GLS results reveal a similar pattern of the effect of dedicated taxes, but of lower magnitude. Unobserved fixed effects appear to bias downward the estimated change in payroll that follows a tax enactment.

Wage Regression Results

Using wage rates as a dependent variable instead of payroll yields qualitatively similar results. The GLS regressions, reported in table 9a, show that including the DEDREV variables alone (equation (10)) results in an estimated coefficient (standard error) of 0.0598 (0.0119) for DEDREV and -0.0074 (0.0026) for DEDREV-squared, representing 6.8 percent higher wages for the average dedicated tax. The significant negative coefficient on DEDREV-squared suggests a decreasing ability to raise wages for higher revenue levels. Including the YEARj dummies alone (equation (11)) indicates a statistically significant 8.4 percent rise in wages from an initial level of $8.23 to $8.92 by year 2 of the tax. Wages remain in the $8.85 to $9.23 range between years 4 and 11, then decline to $8.69 in year 14. The higher levels are statistically significant throughout this period. Including both the DEDREV and YEARj variables (equation (12)) indicates that wages rise significantly with the level of dedicated revenues and also have significantly higher levels in years 4, 6, and 8 through 11, suggesting an upward time path after tax enactment.

The FE estimates for wages are reported in table 9b. Contrary to the GLS and payroll results, a statistically significant increase stemming from dedicated revenue levels (equations (14) and (16)) is no longer indicated. The YEARj dummies (equations (15) and (16)), however, show a statistically significant rise in wages following tax enactment. The increase is on the same order of magnitude and follows a similar pattern to the payroll FE estimates. As shown in figure 1, wages rise from an initial base of $8.23 to $10.00 by year 6 and remain at this level through year 12 before declining slightly. The higher wages are statistically significant in year 2 and in years 4 through 13. As with the payroll FE results, the 95-percent confidence interval surrounding the estimate grows with higher values of YEAR. In spite of the increasing imprecision of the FE estimates, the simulated wage path follows closely the wage rates observed in the sample means, raising confidence in the robustness of the results.

Hausman specification tests reject the null hypothesis of no correlated fixed effects at the 99.5-percent confidence level for GLS specification (9) and at the 95-percent confidence level for specifications (10) and (12)--corresponding to equations (13), (14), and (16) of the FE results. The test statistics (critical values) are 24.6 (16.8), 17.9 (14.1), and 39.2 (37.7), respectively. GLS specification 11, however, is not rejected. The test statistic is 1.2, versus a 95-percent critical value of 33.9. (The test statistic of this specification in the payroll regression showed a similar drop, but still rejected.) Given the overwhelming rejection of the GLS specification in all of the payroll regressions and in three out of four wage regressions, I use FE estimates in the next section to simulate the impact of a dedicated tax on system size, revenues, and the payroll share of revenues.

V. Simulation Results

To explore the implications of a dedicated tax for system size and revenues, I ran FE regressions using LSIZE and the log of revenues, LREV, as dependent variables, and the YEARj and other time-varying variables as independent variables. These results are reported in table 10 in equations (17) and (18), respectively. For comparative purposes and for the convenience of the reader, I also report in equation (19) the FE payroll estimate from equation (8). Finally, I use the payroll share of revenues (measured as LPAY - LREV) as a dependent variable to directly estimate changes in the labor claim on revenues. These results form the basis for the simulation exercises that follow. As with the other FE estimates, the standard errors grow for higher values of YEAR; therefore, the focus of the discussion will be principally on the years immediately following the enactment of a dedicated tax.

Equation (17) indicates that systems that enact a dedicated tax undergo a significant and large expansion in the six-year period following a tax enactment. As shown in figure 2, the simulation suggests that the average system grows 40 percent by year 3 and reaches a 45- to 52-percent higher level by year 6, at which point it stabilizes. Such a large expansion is consistent with the reports I received from transit managers who just recently enacted taxes. The taxes are often presented to the voters as an opportunity to increase service significantly. This large expansion in size naturally corresponds with higher revenues and payroll. I therefore conduct simulations allowing both for system expansion and for keeping the size of a system constant. In simulations of expansions, I use the point estimate of the size results from equation (17). As in the previous section, the base values for the simulations are the mean (log) values for systems without dedicated taxes in 1985.

The change in revenues that follows the enactment of a dedicated tax is estimated in equation (18) in table 10. Total revenues from all sources, as opposed to only dedicated tax revenues, is used as the dependent variable for several reasons. While I know the exact amount of total revenues spent and the percentage of those revenues from local dedicated taxes, the total amount collected from a dedicated tax is not reported. Some dedicated tax revenues may be stored in trust funds for use in expansions or in future years, especially in the early years of the tax. Furthermore, while I know the tax rates in most cases, I have no information on the size of the tax base, preventing direct measurement of its change or erosion over time. Finally, enactment of a dedicated tax usually coincides with a substitution away from other revenue sources--principally local general revenues, though sometimes fares are reduced as well. Because labor is concerned with its share of the total pie, examining labor's share in the change of total revenues is a natural measurement of rent capture.

Coinciding with the expansion in size, revenue estimates show a dramatic rise in the years immediately following tax enactment. (To simulate the change in revenues, I sum the increase resulting from the YEARj dummies with the increase implied by the assumed expansion path and the estimated LSIZE coefficient.) As shown in figure 3, revenues jump a statistically significant 40 percent during the first year of the tax, and remain at a 35- to 45-percent higher level through year 14 before declining in the last few years. A similar projection for payroll, from the estimates in equation (19), is shown in figure 4. Payroll rises 50 percent by year 4, then increases to a 60- to 80-percent higher level in years 8 through 14. The increase is statistically significant throughout the projection period. To compare the payroll and revenue increases, figure 5 charts the dollar amount of changes occurring in both on the same scale. Following tax enactment, payroll takes up a steadily increasing percentage of the additional revenues, absorbing 27 percent by year 3, 47 percent by year 9, and all additional revenues by the end of the projection period.

To net out the direct effects on revenues and payroll resulting from system expansion, or possible economies of scale, I also project revenues and payroll while holding system size constant. Figure 6 shows that the dedicated tax results in significantly higher revenues of 17 to 29 percent in years 1 through 4. In the years immediately following a tax increase, the system thus has much higher funding relative to the level of service provided prior to the dedicated tax. This increase can thus be viewed as a measure of "excess" revenues, which are a potential target for capture by labor unions.

A similar projection for payroll, shown in figure 7, suggests a steady and permanent increase following tax enaction. Payroll 'rises to a 28- to 39-percent higher level by year 10, and the higher level is statistically significant throughout the projection period. As shown in figure 8, the change in payroll appears to absorb all excess revenues by year

6. Payroll remains at this higher level in spite of the disappearance of excess revenues by year 14. Note that the point estimate of payroll edges down once excess revenues disappear. In general, these results show that all additional revenues are absorbed in payroll by year 6 of the tax, and that these gains are permanent in spite of the gradual disappearance of additional revenues resulting from the tax.

Finally, equation (20) in table 10 reports the direct impact of a tax on the payroll share of revenues. As shown in figure 9, payroll share takes an initial drop from 28.7 percent to 25 percent, corresponding with the initial surge in revenues. By year 6, however, payroll share rises to 33 percent, a statistically significant higher level, and steadily increases to 38 percent by year 15. Enactment of a dedicated tax thus appears to result in a steady increase in the labor share of revenues. It should be noted that the payroll share estimates fall well within the range observed in the raw data.

In sum, these results strongly suggest that the use of a dedicated tax system provides significant opportunities for rent capture by public-sector unions. Controlling for private-sector wages and system size, statistically significant growth is seen in wages, payroll, and the labor share of revenues. Within six years, the simulations suggest that higher payroll absorbs all additional revenues resulting from a dedicated tax. The payroll share of revenues continues to increase throughout the sample period, in spite of a fall in revenues.

VI. Conclusion

This paper provides a direct test of whether the enactment of a dedicated tax leads to rent capture by public employees. In a natural experiment provided by the wide variation in funding arrangements for local mass-transit systems, the empirical analysis reveals a systematic link between changes in the wages and payroll for a homogeneous type of employee--bus operators--and enactment of a dedicated tax. The results are robust across several specifications, and the simulation values of wages, payroll, revenues, and payroll shares from a hypothetical tax are well within the observed range of values.

Sample means show that hourly wage rates for public-sector operators rise from a pre-tax level of $7.97 to more than $10.00 in the two-year period following a tax enactment, and over time remain substantially above the average of $8.59 per hour for unionized systems with no dedicated taxes. Pooled time-series cross-section regressions are used to control for local private-sector wage rates, system size, unionization, and demographic characteristics. A generalized least squares (GLS) procedure is initially used and suggests that significant wage and payroll gains of 8 to 10 percent follow a tax enactment.

Although it is possible that the existence of a dedicated tax results from high local wages, history strongly suggests that the principal determinant for the existence of dedicated taxes for transit are state-level policies. The more plausible channel for bias is the presence of unobserved fixed effects that are correlated with both public-sector wages and the existence of dedicated taxes. This potential bias is explicitly tested and controlled for with the standard fixed-effects (FE) estimator, which reveals a statistically significant increase in labor costs of greater magnitude than the GLS estimates. Payroll and wages rise 20 percent in the six years following the tax enactment, and this increase remains stable over time. Hausman specification tests strongly reject the null hypothesis of no correlated fixed effects.

The FE results are used in simulations that measure the extent to which the payroll increases represent the capture of additional revenues resulting from a dedicated tax. The results suggest that in the period immediately following the enactment of a dedicated tax, transit systems expand significantly in size, revenues, and payroll. Holding system size constant, the results show that payroll absorbs all additional revenues by year 6 of the tax. The payroll share of total revenues takes an initial drop with the surge of new revenues, but as payroll and wages steadily increase, the payroll share rises from 27 percent at year 1 to almost 40 percent at the end of the 17-year projection period.

The results,suggest that enactment of a dedicated tax leads to significant rent capture by public-sector unions in the local mass-transit industry. They support the argument that traditional budgeting methods that weigh the costs and benefits of competing uses of funds act as a check on public employee power. Thus, efficiencies that result from earmarked funding may be offset by increased labor costs.

These results should be treated with some caution, however, for the following reasons. First, the simulation of payroll and revenues for a hypothetical tax lasting up to 17 years was estimated from only a four-year sample period. Future work will entail collecting wage and payroll data prior to 1982 to see if the wage gains observed in the sample period also occurred in earlier years. Second, while the assumption that bus drivers are homogeneous seems reasonable, I have no direct measures of human capital with which to test this assumption. Changes in the quality and composition of employees, however, are reported to occur in the administrative area following a tax enactment. Systems with dedicated taxes appear to hire highly paid managers and additional planning, marketing, and public relations personnel. (The general manager of Bi-State Transit in St. Louis is reportedly the highest-paid public employee in Missouri, with the exception of the governor.) One possible approach in future work will be to examine whether changes in the composition of administrative staff result in higher-quality service or cost-effectiveness.

In general, the evidence of rent capture appears to be robust for the local mass-transit industry and confirms differences in wages and salary share of revenues observed in the aggregate data on the use of dedicated'taxes in special districts. While the local mass-transit industry provides a clean experiment due to its homogeneous output, inputs, and production processes, examination of districts producing other types of outputs would provide additional confirmation. Also of interest would be examination of dedicated taxes at the state and federal levels. Examination of the impact of earmarking for education at the state level is a possible avenue for future research.

Footnotes

1. For an extreme example of political empire-building resulting from a dedicated revenue source, see the Caro (1984) account of Robert Moses and the toll revenues of the Tri-borough Bridge Commission in New York.

2. For a general discussion of earmarking, see Gold, et. al. (1987)

3. For for a general discussion of economic rents and regulation, see Joskow and Rose (forthcoming). Studies of airline deregulation include Card (1986).

4. None of the dedicated taxes observed in this study was repealed during the four-year sample period.

5. Thirteen systems that provided data that were obviously "wrong" or inconsistent over time were also omitted. For example, one system reported having no expenses for fuel or tires. Others reported having no operating employees. Still others reported large swings in the size of operations and revenues. In general, the payroll, revenue, and operating data such as mileage are of high quality, while the data on employee hours and passengers is less so.

6. Omitting California from the sample results in larger standard errors but does not substantively change the results.

7. Wisconsin subsidizes operating expenses at a 36-percent matching rate.

8. Certain systems without dedicated taxes receive trace amounts of revenues earmarked from local sources such as parking meter fees, license fees, and other local charges.

9. Demographic variables were collected from the U.S. Census Bureau (1986).

10. Other specifications tested included those entering crashes, fatalities, and the private-sector union wage. Coefficients on these variables were insignificant and did not affect the results.

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Table 1
Trends in Special Districts:
Number of Units and Revenue Composition

	1957	1967	1977	1982
Number of units	14,405	21,264	25,987	28,719
Percent of total governmental units	14.1	26.2	32.5	34.9
Revenue~ (1982 $, millions)*				
Total	6.280	11.838	21.795	30.961
Federal aid	315	763	3,723	4,405
State aid	140	475	1,274	1,810
Local general revenues	119	747	1,555	2,057
Local dedicated taxes	1,213	1,841	2,597	2,846
Own source revenues	4,493	8,013	12,644	19,843
Revenues (percent)				
Total	100	100	100	100
Federal aid	5.0	6.4	17.1	14.2
State aid	2.2	4.0	5.8	5.8
Local general revenues	1.9	6.3	7.1	6.6
Local dedicated taxes	19.3	15.5	11.9	9.2
Own source revenues	71.5	67.7	58.0	64.1

* Calculated with GNP deflator for state and local purchases.

Sources: U.S. Census Bureau, Census of Governments, and author's calculations.

Table 2
The Use of Dedicated Taxes:
Special District Revenues and Wages

	Census Year	Districts With Dedicated Tax Authority	Districts Without Dedicated Tax Authority
Total revenues (1982 $, millions)*	1967	5,371	6,466
	1972	6,837	8,527
	1977	10,102	11,693
Dedicated tax revenues (percent)	1967	33.1	1.1
	1972	30.3	1.1
	1977	25.4	0.3
Wage and salary share of revenues (percent)	1967	30.7	26.8
	1972	32.8	28.4
	1977	30.9	25.9

* Calculated with GNP deflator for state and local purchases

Sources: U.S. Census Bureau, Census of Governments, and author's calculations.

Table 3
Composition of 1982
Special District Revenues (Percent):
States Rank-Ordered by Use of Local Dedicated Taxes

Quintile	Total	Federal Aid	State Aid	---Local---- Gen. Rev.	Ded. Taxes	Own Source	Wage and Salary Share
1	100	24.3	8.9	5.8	0.3	60.7	21.9
2	100	17.8	3.6	3.8	2.3	72.4	18.7
3	100	18.5	5.2	6.6	7.5	62.3	21.0
4	100	22.8	5.2	2.4	11.4	58.2	24.1
5	100	13.6	5.5	5.6	25.1	50.1	30.2
National average	100	14.2	5.8	6.6	9.2	64.1	25.2

* Unweighted averages, Hawaii excluded. Quintile 3 has 9 states.

Sources: U.S. Census Bureau, 1982 Census of Governments, and author's calculations.

Table 4
Dedicated Taxes for Local Mass-Transit Systems

Year Enacted	State	System Name	Type of Tax	Periodic Renewal ?
1984	FL	Ft. Lauderdale-Browrd Cnty TA	gasoline	no
	IA	Waterloo-Black Hawk Cnty TA	property	no
	TX	Austin TA	sales	no
	TX	Fort Worth CITRAN	sales	no
1983	FL	Gainesville-RTS	gasoline	no
1982	FL	Hillborough Area RTA	sales	no
	OH	Parkrsbrg-Mid-Ohio Valley TA	property	no
	WV	Huntington-Tri-State TA	property	yes (3 year)
1981	MI	Lansing-Capital Area TA	property	no
	OH	Columbus-Central Ohio TA	sales	yes (5 year)
	WA	Spokane TA	sales	no
1980	WA	Tacoma-Pierce Cnty TS	sales	no
1978	KY	Ft. Wright-TA No. Kentucky	payroll	no
	TX	San Antonio-VIA Metro Tr.	sales	no
	TX	Houston-MTA	sales	no
	WV	Wheeling-Ohio Valley RTA	property	no
1976	IA	Sioux City TS	property	no
1975	FL	S Daytona-E Volusia TA	sales	no
	IN	Indianapolis PTC	sales	no
1974	CO	Denver-RTD	sales	no
	KY	Louisville-TA River City	payroll	no
1973	FL	Pinellas Suncoast TA	property	no
	IA	City of Dubuque-Keyline TS	property	no
	IN	Gary PTC	property	no
	KS	Topeka MTA	property	no
	MI	Ypsilanti-Ann Arbor TA	property	no
	MO	St. Louis-Bi-State TA	sales	no
	OH	Cincinnati-SORTA	payroll	no
	OR	Portland-Tri-County MTD	payroll	no
	OR	Eugene-Lane County MTD	payroll	no
	WV	Charlestn-Kanawha Vly RTA	property	yes (5 year)
1972	CA	Sacramento RTD	gasoline	no
	CA	Los Angeles-SCRTD	gasoline	no
	CA	Monterey-Salinas TA	gasoline	no
	CA	Montebello Muni Bus	gasoline	no

Table 4 (cont.)
Dedicated Taxes for Local Mass-Transit Systems

Year Enacted	State	System Name	Type of Tax	Periodic Renewal ?
1972	CA	Santa Monica Muni Bus	gasoline	no
(cont.)	CA	San Diego TS	gasoline	no
	CA	Alameda-Contra Costa TD	gasoline	no
	CA	Oxnard-S Coast Area Transit	gasoline	no
	CA	Gardena-Municipal Bus	gasoline	no
	CA	Santa Barbara MTD	gasoline	no
	CA	Fresno TS	gasoline	no
	CA	Stockton MTD	gasoline	no
	CA	Bakrsfld-Golden Empire TD	gasoline	no
	CA	Riverside TA	gasoline	no
	CA	N San Diego TS	gasoline	no
	NE	TA of Omaha	property	no
	OH	Akron-Metropolitan RTA	property	no
1971	IL	Champaign-Urbana MTD	property	no
	IL	Rock Island County MTD	property	no
	IL	Greater Peoria Mass TD	property	no
	IN	Greater Lafayette PTC	property	no
	MO	Kansas City Area TA	sales	no
	OH	Canton RTA	property	yes (5 year)
	OH	Youngstown-Western Res. TS	property	yes (10 year)
1970	MN	Minneapolis MTC	property	no
	MN	Duluth TA	property	no
	OH	Toledo RTA	property	no
	UT	Salt Lake City-Utah TA	sales	no
1968	IL	Springfield MTD	property	no
	IN	Fort Wayne PTC	property	no
1967	IA	Cedar Rapids Bus Dept.	property	no
	IN	South Bend PTC	property	no
	KS	Wichita MTA	property	no

Source: Telephone survey by author.

Table 5
Transit System Summary Statistics:
Size, Revenues, Wages

Variable	Systems With Dedicated Taxes	Systems Without Dedicated Taxes
Number of observations	(64)	(101)
System size (miles, 000)	6,458 (12,452)	2,155 (3,075)
Vehicle operators	315.7 (656.3)	103.8 (156.4)
Revenues ($ millions)	27.9 (65.5)	7.0 (12.1)
Revenue composition (percent)		
Federal aid	21.5 (9.5)	28.2 (10.3)
State aid	8.5 (11.6)	14.8 (15.4)
Local general revenues	1.9 (6.4)	23.7 (15.0)
Local dedicated taxes	40.4 (16.3)	0.1 (0.6)
Own source (fares)	27.7 (8.9)	33.2 (10.7)
Unionized	63/65	82/101
Wages ($ per hour)	9.77 (1.78)	8.42 (1.80)

* 1985 sample means (standard deviations).

Sources: UMTA (1985) and author's calculations.

Table 6
Private-Sector Wages and Demographic Variables *

Variable Name	Areas With Systems Using Dedicated Taxes	Areas With Systems Not Using Dedicated Taxes
Private operator nonunion wage	8.70 (0.67)	8.32 (0.56)
Private operator union wage	11.02 (0.95)	10.23 (0.79)
Population (000)	821.3 (1147.7)	356.0 (331.3)
Density (pop. per sq. mi.)	531.8 (567.3)	491.4 (563.8)
Pop. growth (%change, 1980-84)	14.4 (74.9)	4.3 (5.0)
Income ($ per capita)	10,043 (4,013)	9,014 (1,588)
Poverty (% of pop.)	10.6 (2.9)	21.3 (92.2)
Black (% of pop.)	7.7 (5.4)	12.5 (12.4)
Drive to work (% of pop.)	68.3 (4.2)	67.7 (5.1)
Bus to work (% of pop.)	3.8 (2.2)	3.3 (2.6)

* 1985 sample means (standard deviations).

Sources: U.S. Census Bureau (1986) and author's calculations.

Table 7
Trends After Tax Enactment:*
Payroll, Wages, and Size

	Number of Obs.**	Payroll ($ per mi.)	Vehicle Operator Wages ($ per hr.) Public	Vehicle Operator Wages ($ per hr.) Private	System Size (000 mi.)
<u>Systems without dedicated taxes</u>					
Nonunionized	76	0.61 (0.13)	6.50 (1.34)	7.90 (0.64)	833 (775)
Unionized	328	0.86 (0.21)	8.59 (1.58)	8.04 (0.70)	2,465 (3,327)
<u>Systems with dedicated taxes</u>					
Pre-tax	17	0.74 (0.15)	7.97 (1.49)	7.77 (0.77)	2,581 (2,163)
Years after tax enactment					
YEAR=1	11	0.87 (0.26)	9.17 (2.07)	8.04 (0.49)	3,115 (2,691)
YEAR=2	8	0.88 (0.23)	10.28 (2.82)	8.31 (0.64)	3,485 (3,080)
YEAR=3	7	0.97 (0.23)	10.38 (2.66)	8.62 (0.70)	3,960 (3,241)
YEAR=4	8	0.99 (0.22)	10.55 (2.21)	8.21 (0.90)	6,860 (6,126)
YEAR=5	5	0.90 (0.14)	9.78 (1.30)	8.91 (0.97)	8,584 (8,749)
YEAR=6	5	0.88 (0.10)	10.00 (0.47)	8.33 (0.98)	9,198 (12,115)
YEAR=7	7	0.81 (0.22)	8.84 (2.07)	8.26 (0.95)	7,624 (10,296)
YEAR=8	5	0.91 (0.28)	9.02 (2.32)	7.97 (0.62)	7,051 (7,531)
YEAR=9	15	0.95 (0.26)	9.32 (2.15)	7.91 (0.70)	6,619 (7,542)

Table 7 (cont.)
Trends after Tax Enactment:
Payroll, Wages, and Size*

Years after tax enactment	Number of Obs.**	Payroll ($ per mi.)	Vehicle Operator Wages ($ per hr.)		System Size (000 mi.)
			Public	Private	
YEAR=10	31	0.92 (0.25)	9.73 (1.83)	8.32 (0.60)	8,676 (16,797)
YEAR=11	36	0.96 (0.20)	9.97 (1.68)	8.65 (0.89)	7,786 (15,855)
YEAR=12	38	0.97 (0.20)	9.89 (1.41)	8.74 (0.69)	7,576 (15,696)
YEAR=13	28	0.99 (0.23)	10.09 (1.44)	9.00 (0.67)	7,875 (17,519)
YEAR=14	13	1.01 (0.23)	9.42 (1.30)	8.22 (0.67)	4,471 (6,061)
YEAR-15	9	0.99 (0.28)	9.37 (1.33)	8.11 (0.94)	5,348 (7,128)
YEAR=16	5	0.97 (0.29)	9.19 (1.44)	8.14 (0.21)	1,353 (455)
YEAR > 16	8	0.86 (0.23)	8.92 (1.59)	8.07 (0.23)	1,471 (418)

* Means (standard deviations). All $ figures are in 1985 values, calculated using the Consumer Price Index.

** Time-series/cross-sectional observations: 165 transit systems were observed over a 4-year (1982-85) period for a total of 660 observations.

Source: Author's calculations.

Table 8a *
Generalized Least Squares Payroll Regressions
Dependent Variable: Log of Operators' Payroll

Independent Variables	(1)	(2)	(3)	(4)
LSIZE	0.981	0.968	0.977	0.977
(log miles)	(0.014)	(0.013)	(0.013)	(0.013)
LPRIWAGE	0.455	0.372	0.355	0.328
(log private wage)	(0.086)	(0.086)	(0.093)	(0.091)
DEDREV	---	0.0640	---	0.0626
($ per mile)	---	(0.0138)	---	(0.0216)
DEDREV*DEDREV	---	-0.0023	---	-0.0022
	---	(0.0026)	---	(0.0035)
Years after tax enactment				
YEAR1	---	---	0.016	-0.016
(=1 if YEAR=1)	---	---	(0.027)	(0.025)
YEAR2	---	---	0.047	-0.031
(=1 if YEAR=2)	---	---	(0.036)	(0.038)
YEAR3	---	---	0.082	-0.006
(=1 if YEAR=3)	---	---	(0.038)	(0.041)
YEAR4	---	---	0.100	-0.002
(=1 if YEAR=4)	---	---	(0.037)	(0.041)
YEAR5	---	---	0.084	-0.003
(=1 if YEAR=5)	---	---	(0.040)	(0.045)
YEAR6	---	---	0.044	-0.029
(=1 if YEAR=6)	---	---	(0.041)	(0.047)
YEAR7	---	---	0.028	-0.054
(=1 if YEAR=7)	---	---	(0.041)	(0.047)
YEAR8	---	---	0.083	0.020
(=1 if YEAR=8)	---	---	(0.039)	(0.043)
YEAR9	---	---	0.107	0.035
(=1 if YEAR=9)	---	---	(0.025)	(0.032)
YEAR10	---	---	0.071	-0.0003
(=1 if YEAR=10)	---	---	(0.021)	(0.0281)

Table 8a (cont.) *
Generalized Least Squares Payroll Regressions
Dependent Variable: Log of Operators' Payroll

Independent Variables	(1)	(2)	(3)	(4)
YEAR11	---	---	0.091	0.012
(=1 if YEAR=11)	---	---	(0.020)	(0.029)
YEAR12	---	---	0.082	0.005
(=1 if YEAR=12)	---	---	(0.020)	(0.029)
YEAR13	---	---	0.085	0.003
(=1 if YEAR=13)	---	---	(0.021)	(0.030)
YEAR14	---	---	0.086	0.010
(=1 if YEAR=14)	---	---	(0.025)	(0.032)
YEAR15	---	---	0.091	0.020
(=1 if YEAR=15)	---	---	(0.042)	(0.044)
YEAR16	---	---	0.079	0.007
(=1 if YEAR=16)	---	---	(0.053)	(0.054)
YEAR17	---	---	0.081	0.010
(-1 if YEAR > 16)	---	---	(0.059)	(0.060)
DUM83	0.007	0.013	0.023	0.023
	(0.015)	(0.015)	(0.017)	(0.016)
DUM84	0.039	0.047	0.056	0.057
	(0.018)	(0.017)	(0.019)	(0.019)
DUM85	0.054	0.063	0.074	0.076
	(0.022)	(0.022)	(0.025)	(0.024)
Transit union	0.325	0.324	0.309	0.315
(-1 if yes)	(0.028)	(0.028)	(0.029)	(0.029)
Population	0.085	0.074	0.079	0.068
(log)	(0.015)	(0.016)	(0.015)	(0.015)
Density	0.094	0.100	0.099	0.105
(log)	(0.010)	(0.009)	(0.010)	(0.009)
% change in	0.00010	-0.00005	0.00011	0.000004
population, 80 84	(0.00020)	(0.00019)	(0.00017)	(0.000176)
Black	-0.0058	-0.0054	-0.0053	-0.0052
(% of pop)	(0.0007)	(0.0007)	(0.0007)	(0.0007)

Table 8a (cont.) *
Generalized Least Squares Payroll Regressions
Dependent Variable: Log of Operators' Payroll

Independent Variables	(1)	(2)	(3)	(4)
Income	-0.198	-0.162	-0.208	-0.181
(per capita, log)	(0.047)	(0.047)	(0.051)	(0.049)
Poverty	-0.00008	-0.00009	-0.00009	0.00009
(% of pop.)	(0.00004)	(0.00004)	(0.00004)	(0.00004)
Constant	0.139	0.102	0.471	0.331
	(0.434)	(0.440)	(0.476)	(0.468)
Buse R-squared	0.983	0.976	0.980	0.974
Mean dep. var.	7.219	7.219	7.219	2.093
Log likelihood	627.07	636.70	634.10	680.666
Estimated rho	0.823	0.825	0.817	0.767

* Estimated coefficients (standard errors). GLS prodedure used on cross-sectionally heteroscedastic and time-wise autoregressive model discussed in Kmenta (1986). Cross-sections restricted to have same autoregressive parameter. Buse R-Squared defined in Buse (1973).

Source: Author's calculations.

Table 8b
Fixed-Effects Payroll Regressions*
Dependent Variable: Log of Operators' Payroll

Independent Variable	(5)	(6)	(7)	(8)
LSIZE	0.711	0.700	0.682	0.686
(log miles)	(0.026)	(0.026)	(0.027)	(0.026)
LPRIWAGE	0.435	0.436	0.367	0.362
(log private wage)	(0.141)	(0.137)	(0.142)	(0.141)
DEDREV	---	0.0947	---	0.0607
($ per mile)	---	(0.0209)	---	(0.0266)
DEDREV*DEDREV	---	-0.0067	---	-0.0030
	---	(0.0032)	---	(0.0038)
Years after tax enactment				
YEAR1	---	---	0.118	0.051
(=1 if YEAR=1)	---	---	(0.030)	(0.036)
YEAR2	---	---	0.166	0.097
(=1 if YEAR=2)	---	---	(0.040)	(0.045)
YEAR3	---	---	0.203	0.123
(=1 if YEAR=3)	---	---	(0.043)	(0.050)
YEAR4	---	---	0.210	0.111
(=1 if YEAR=4)	---	---	(0.053)	(0.061)
YEAR5	---	---	0.196	0.099
(=1 if YEAR=5)	---	---	(0.065)	(0.074)
YEAR6	---	---	0.238	0.139
(=1 if YEAR=6)	---	---	(0.070)	(0.083)
YEAR7	---	---	0.203	0.103
(=1 if YEAR=7)	---	---	(0.071)	(0.083)
YEAR8	---	---	0.266	0.161
(=1 if YEAR=8)	---	---	(0.086)	(0.098)
YEAR9	---	---	0.331	0.215
(=1 if YEAR=9)	---	---	(0.086)	(0.099)
YEAR10	---	---	0.286	0.171
(=1 if YEAR=10)	---	---	(0.088)	(0.101)

Table 8b (cont.)
Fixed-Effects Payroll Regressions*
Dependent Variable: Log of Operators' Payroll

Independent Variable	(5)	(6)	(7)	(8)
YEAR11	---	---	0.301	0.178
(=1 if YEAR=11)	---	---	(0.090)	(0.103)
YEAR12	---	---	0.307	0.178
(=1 if YEAR=12)	---	---	(0.091)	(0.106)
YEAR13	---	---	0.310	0.178
(=1 if YEAR=13)	---	---	(0.094)	(0.108)
YEAR14	---	---	0.291	0.157
(=1 if YEAR=14)	---	---	(0.097)	(0.111)
YEAR15	---	---	0.281	0.143
(=1 if YEAR=15)	---	---	(0.102)	(0.117)
YEAR16	---	---	0.248	0.107
(=1 if YEAR=16)	---	---	(0.110)	(0.124)
YEAR17	---	---	0.210	0.068
(=1 if YEAR > 16)	---	---	(0.111)	(0.125)
DUM83	0.015	0.007	0.024	0.022
	(0.028)	(0.025)	(0.026)	(0.026)
DUM84	0.052	0.043	0.058	0.059
	(0.028)	(0.028)	(0.029)	(0.029)
DUM85	0.073	0.059	0.081	0.081
	(0.036)	(0.035)	(0.037)	(0.037)
R-squared	0.673	0.690	0.693	0.699
Mean dep. var.	7.219	7.219	7.219	7.219
Log likelihood	806.96	824.51	828.07	834.78
Hausman test stat.	125.2	131.0	49.8	156.4

* Estimated coefficients (standard errors).

Source: Author's calculations.

Table 9a *

Generalized Least Squares Wage Regressions
Dependent Variable: Log of Operators' Wage

Independent Variables	(9)	(10)	(11)	(12)
LSIZE	0.102	0.093	0.090	0.089
(log miles)	(0.010)	(0.010)	(0.010)	(0.010)
LPRIWAGE	**0.237**	**0.134**	**0.126**	**0.117**
(log private wage)	**(0.069)**	**(0.070)**	**(0.078)**	**(0.078)**
DEDREV	---	0.0598	---	0.0363
($ per mile)	---	(0.0119)	---	(0.0167)
DEDREV*DEDREV	---	-0.0074	---	-0.0045
	---	(0.0026)	---	(0.0027)
Years after tax enactment				
YEAR1	---	---	0.022	0.003
(=1 if YEAR=1)	---	---	(0.024)	(0.024)
YEAR2	---	---	0.081	0.058
(=1 if YEAR=2)	---	---	(0.037)	(0.037)
YEAR3	---	---	0.027	0.007
(=1 if YEAR=3)	---	---	(0.040)	(0.040)
YEAR4	---	---	0.086	0.071
(=1 if YEAR=4)	---	---	(0.037)	(0.039)
YEAR5	---	---	0.084	0.059
(=1 if YEAR=5)	---	---	(0.040)	(0.044)
YEAR6	---	---	0.125	0.085
(=1 if YEAR=6)	---	---	(0.041)	(0.045)
YEAR7	---	---	0.073	0.033
(=1 if YEAR=7)	---	---	(0.039)	(0.042)
YEAR8	---	---	0.010	0.073
(=1 if YEAR=8)	---	---	(0.037)	(0.038)
YEAR9	---	---	0.098	0.066
(=1 if YEAR=9)	---	---	(0.022)	(0.025)
YEAR10	---	---	0.077	0.047
(=1 if YEAR=10)	---	---	(0.016)	(0.021)

Table 9a (cont.) *
Generalized Least Squares Wage Regressions
Dependent Variable: Log of Operators' Wage

Independent Variables	(9)	(10)	(11)	(12)
YEAR11	---	---	0.073	0.043
(=1 if YEAR=11)	---	---	(0.015)	(0.021)
YEAR12	---	---	0.068	0.036
(=1 if YEAR=12)	---	---	(0.016)	(0.021)
YEAR13	---	---	0.064	0.034
(=1 if YEAR=13)	---	---	(0.016)	(0.022)
YEAR14	---	---	0.054	0.028
(=1 if YEAR=14)	---	---	(0.020)	(0.024)
YEAR15	---	---	0.035	0.008
(=1 if YEAR=15)	---	---	(0.032)	(0.035)
YEAR16	---	---	0.025	-0.003
(=1 if YEAR=16)	---	---	(0.039)	(0.042)
YEAR17	---	---	0.055	0.025
(=1 if YEAR > 16)	---	---	(0.044)	(0.046)
DUM83	0.046	0.059	0.065	0.065
	(0.012)	(0.012)	(0.014)	(0.014)
DUM84	0.079	0.094	0.102	0.101
	(0.014)	(0.014)	(0.016)	(0.016)
DUM85	0.107	0.130	0.139	0.139
	(0.017)	(0.018)	(0.020)	(0.020)
Transit union	0.212	0.212	0.206	0.208
(=1 if yes)	(0.028)	(0.028)	(0.027)	(0.027)
Population	-0.036	-0.036	-0.033	-0.035
(log)	(0.011)	(0.012)	(0.011)	(0.011)
Density	0.032	0.039	0.041	0.042
(log)	(0.007)	(0.007)	(0.008)	(0.008)
% change in	0.00018	0.00009	0.00011	0.00005
population, 80-84	(0.00021)	(0.00020)	(0.00027)	(0.00027)
Black	-0.0029	-0.0026	-0.0026	-0.0025
(% of pop)	(0.0007)	(0.0007)	(0.0007)	(0.0007)

Table 9a (cont.) *
Generalized Least Squares Wage Regressions
Dependent Variable: Log of Operators' Wage

Independent Variables	(9)	(10)	(11)	(12)
Income	0.007	0.004	0.003	0.010
(per capita, log)	(0.037)	(0.037)	(0.039)	(0.039)
Poverty	0.000007	0.000003	-0.000002	0.000001
(% of pop)	(0.00003)	(0.00003)	(0.00003)	(0.00004)
Constant	0.797	1.077	1.102	1.074
	(0.350)	(0.351)	(0.371)	(0.372)
Buse R-squared	0.664	0.677	0.682	0.680
Mean dep.var.	2.090	2.090	2.090	2.090
Log likelihood	717.43	723.21	722.59	724.58
Estimated rho	0.814	0.808	0.804	0.804

* Estimated coefficients (standard errors). GLS procedure used on cross-sectionally heteroscedastic and time-wise autoregressive model discussed in Kmenta (1986). Cross-sections restricted to have same autoregressive parameter. Buse R-squared defined in Buse (1973).

Source: Author's calculations.

Table 9b *
Fixed-Effects Wage Regressions
Dependent Variable: Log of Operators' Wage

Independent Variables	(13)	(14)	(15)	(16)
LSIZE	0.028	0.023	0.013	0.013
(log miles)	(0.026)	(0.027)	(0.027)	(0.027)
LPRIWAGE	0.203	0.201	0.165	0.165
(log private wage)	(0.142)	(0.141)	(0.145)	(0.145)
DEDREV	---	0.0377	---	0.0047
($ per mile)	---	(0.0215)	---	(0.0274)
DEDREV*DEDREV	---	-0.0041	---	-0.0037
	---	(0.0033)	---	(0.0038)
Years after tax enactment				
YEAR1	---	---	0.052	0.044
(=1 if YEAR=1)	---	---	(0.031)	(0.037)
YEAR2	---	---	0.115	0.107
(=1 if YEAR=2)	---	---	(0.041)	(0.047)
YEAR3	---	---	0.043	0.034
(=1 if YEAR=3)	---	---	(0.044)	(0.052)
YEAR4	---	---	0.136	0.124
(=1 if YEAR=4)	---	---	(0.054)	(0.063)
YEAR5	---	---	0.124	0.115
(=1 if YEAR=5)	---	---	(0.067)	(0.076)
YEAR6	---	---	0.232	0.225
(=1 if YEAR=6)	---	---	(0.072)	(0.086)
YEAR7	---	---	0.171	0.163
(=1 if YEAR=7)	---	---	(0.073)	(0.086)
YEAR8	---	---	0.190	0.182
(=1 if YEAR=8)	---	---	(0.088)	(0.100)
YEAR9	---	---	0.204	0.195
(=1 if YEAR=9)	---	---	(0.088)	(0.102)
YEAR10	---	---	0.195	0.186
(=1 if YEAR=10)	---	---	(0.090)	(0.104)

Table 9b (cont.)
Fixed-Effects Wage Regressions*
Dependent Variable: Log of Operators' Wage

Independent Variables	(13)	(14)	(15)	(16)
YEAR11	---	---	0.195	0.185
(=1 if YEAR=11)	---	---	(0.090)	(0.107)
YEAR12	---	---	0.186	0.176
(=1 if YEAR=12)	---	---	(0.092)	(0.109)
YEAR13	---	---	0.181	0.170
(=1 if YEAR=13)	---	---	(0.095)	(0.111)
YEAR14	---	---	0.148	0.136
(=1 if YEAR=14)	---	---	(0.099)	(0.115)
YEAR15	---	---	0.132	0.120
(=1 if YEAR=15)	---	---	(0.105)	(0.120)
YEAR16	---	---	0.106	0.094
(=1 if YEAR=16)	---	---	(0.112)	(0.128)
YEAR17	---	---	0.092	0.080
(=1 if YEAR > 16)	---	---	(0.113)	(0.129)
DUM83	0.059	0.057	0.066	0.066
	(0.026)	(0.026)	(0.027)	(0.027)
DUM84	0.099	0.096	0.105	0.106
	(0.029)	(0.029)	(0.030)	(0.030)
DUM85	0.131	0.127	0.142	0.142
	(0.036)	(0.036)	(0.038)	(0.038)
R-squared	0.470	0.472	0.488	0.489
Mean dep. var.	2.090	7.219	7.219	7.219
Log likelihood	803.60	805.31	815.41	815.59
Hausman test stat.	24.6	17.9	1.2	39.2

* Estimated coefficients (standard errors).

Source: Author's calculations.

Table 10
Size, Revenue, Payroll, and Payroll Share
Fixed-Effects Regressions*

Independent Variable	(17) Log Size	(18) Log Revenue	(19) Log Payroll	(20) Payroll Share (Log Payroll - Log Revenue)
LSIZE (log miles)	---	0.664 (0.032)	0.682 (0.027)	0.018 (0.026)
LPRIWAGE (log private wage)	0.005 (0.212)	0.222 (0.170)	0.367 (0.142)	0.145 (0.139)
Years after tax enactment				
YEAR1 (=1 if YEAR=1)	0.124 (0.045)	0.254 (0.036)	0.118 (0.030)	-0.136 (0.030)
YEAR2 (=1 if YEAR=2)	0.269 (0.058)	0.159 (0.047)	0.166 (0.040)	0.007 (0.011)
YEAR3 (=1 if YEAR=3)	0.339 (0.063)	0.229 (0.052)	0.203 (0.043)	-0.026 (0.042)
YEAR4 (=1 if YEAR=4)	0.288 (0.078)	0.242 (0.063)	0.210 (0.053)	-0.031 (0.052)
YEAR5 (=1 if YEAR=5)	0.325 (0.096)	0.133 (0.078)	0.196 (0.065)	0.063 (0.064)
YEAR6 (=1 if YEAR=6)	0.418 (0.104)	0.096 (0.084)	0.238 (0.070)	0.142 (0.069)
YEAR? (-1 if YEAR-7)	0.417 (0.105)	0.056 (0.085)	0.203 (0.071)	0.147 (0.070)
YEAR8 (=1 if YEAR=8)	0.395 (0.127)	0.094 (0.103)	0.266 (0.086)	0.171 (0.084)
YEAR9 (=1 if YEAR=9)	0.383 (0.127)	0.144 (0.103)	0.331 (0.086)	0.187 (0.084)
YEAR10 (=1 if YEAR=10)	0.374 (0.130)	0.095 (0.105)	0.286 (0.088)	0.192 (0.086)

Table 10 (cont.)
Size, Revenue, Payroll, and Payroll Share
Fixed-Effects Regressions*

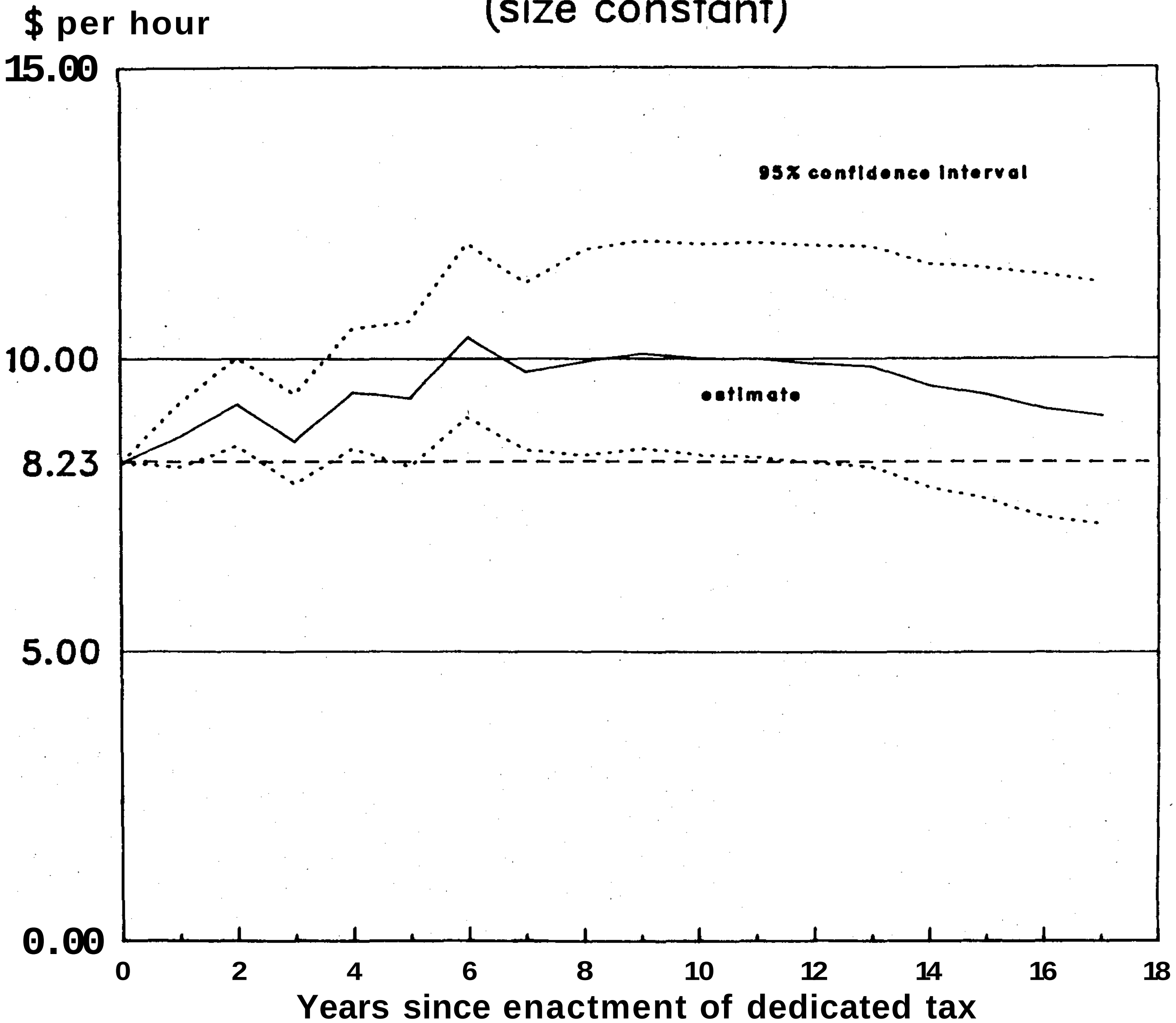
Independent Variable	(17) Log Size	(18) Log Revenue	(19) Log Payroll	(20) Payroll Share (Log Payroll - Log Revenue)
YEAR11 (=1 if YEAR=11)	0.364 (0.133)	0.072 (0.106)	0.301 (0.090)	0.229 (0.088)
YEAR12 (=1 if YEAR=12)	0.389 (0.135)	0.070 (0.109)	0.307 (0.091)	0.236 (0.089)
YEAR13 (=1 if YEAR=13)	0.391 (0.138)	0.031 (0.112)	0.310 (0.093)	0.279 (0.091)
YEAR14 (=1 if YEAR=14)	0.425 (0.144)	0.011 (0.116)	0.291 (0.097)	0.279 (0.095)
YEAR15 (=1 if YEAR=15)	0.382 (0.152)	-0.005 (0.122)	0.281 (0.102)	0.286 (0.100)
YEAR16 (=1 if YEAR=16)	0.307 (0.163)	-0.045 (0.131)	0.248 (0.110)	0.293 (0.107)
YEAR17 (=1 if YEAR > 16)	0.362 (0.165)	-0.140 (0.133)	0.210 (0.111)	0.350 (0.108)
DUM83	-0.024 (0.039)	0.048 (0.031)	0.024 (0.026)	-0.024 (0.026)
DUM84	-0.024 (0.044)	0.103 (0.035)	0.058 (0.029)	-0.045 (0.029)
DUM85	-0.013 (0.055)	0.150 (0.044)	0.081 (0.037)	-0.069 (0.036)
R-squared	0.070	0.631	0.693	0.107
Mean dep. var.	7.465	8.460	7.219	-1.240
Log-likelihood	421.08	710.34	828.07	845.47

* Estimated coefficients (standard errors).

Source: author's calculations.

Figure 1

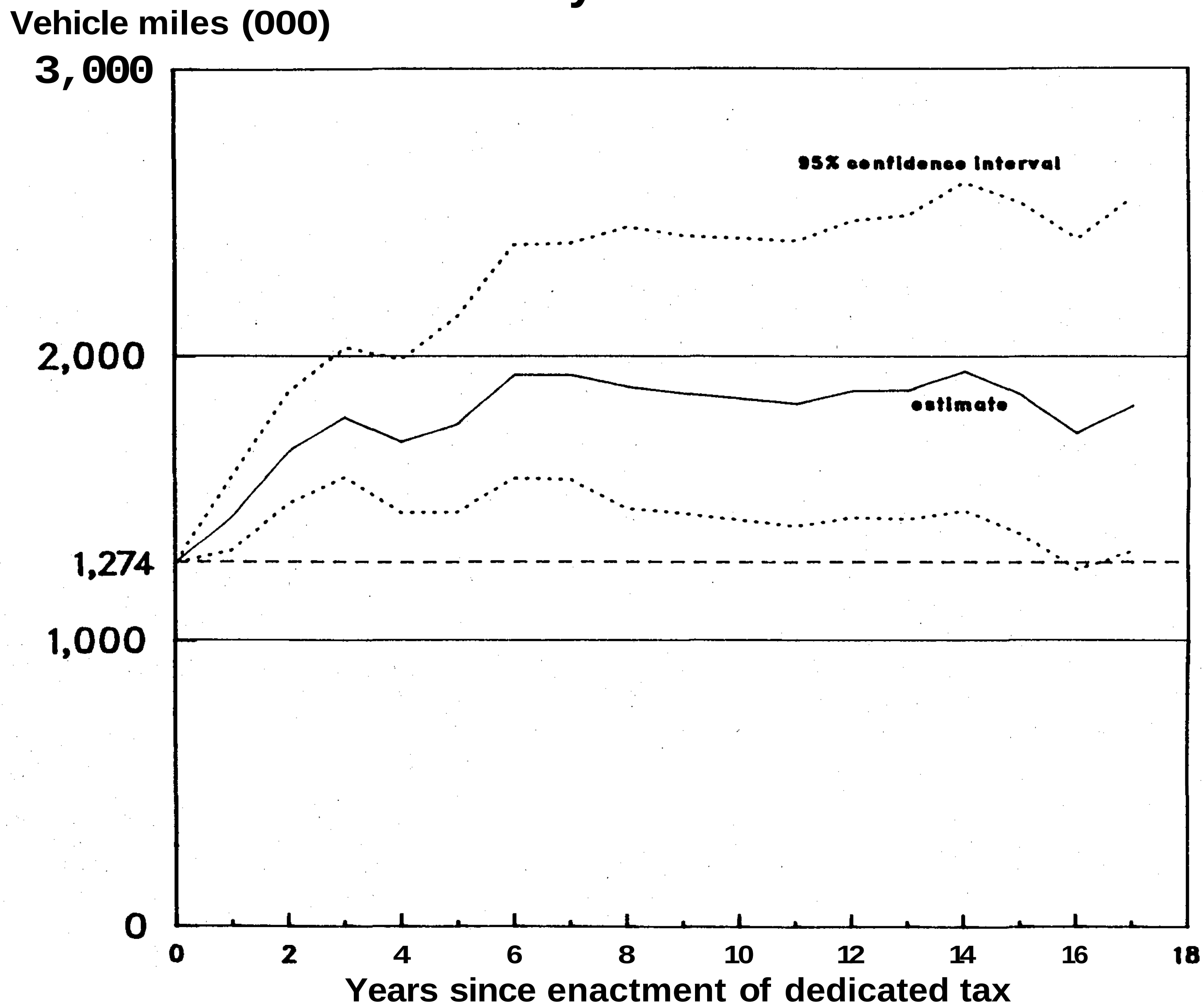
Wage Rates
(size constant)



Source: Author's Calculations

Figure 2

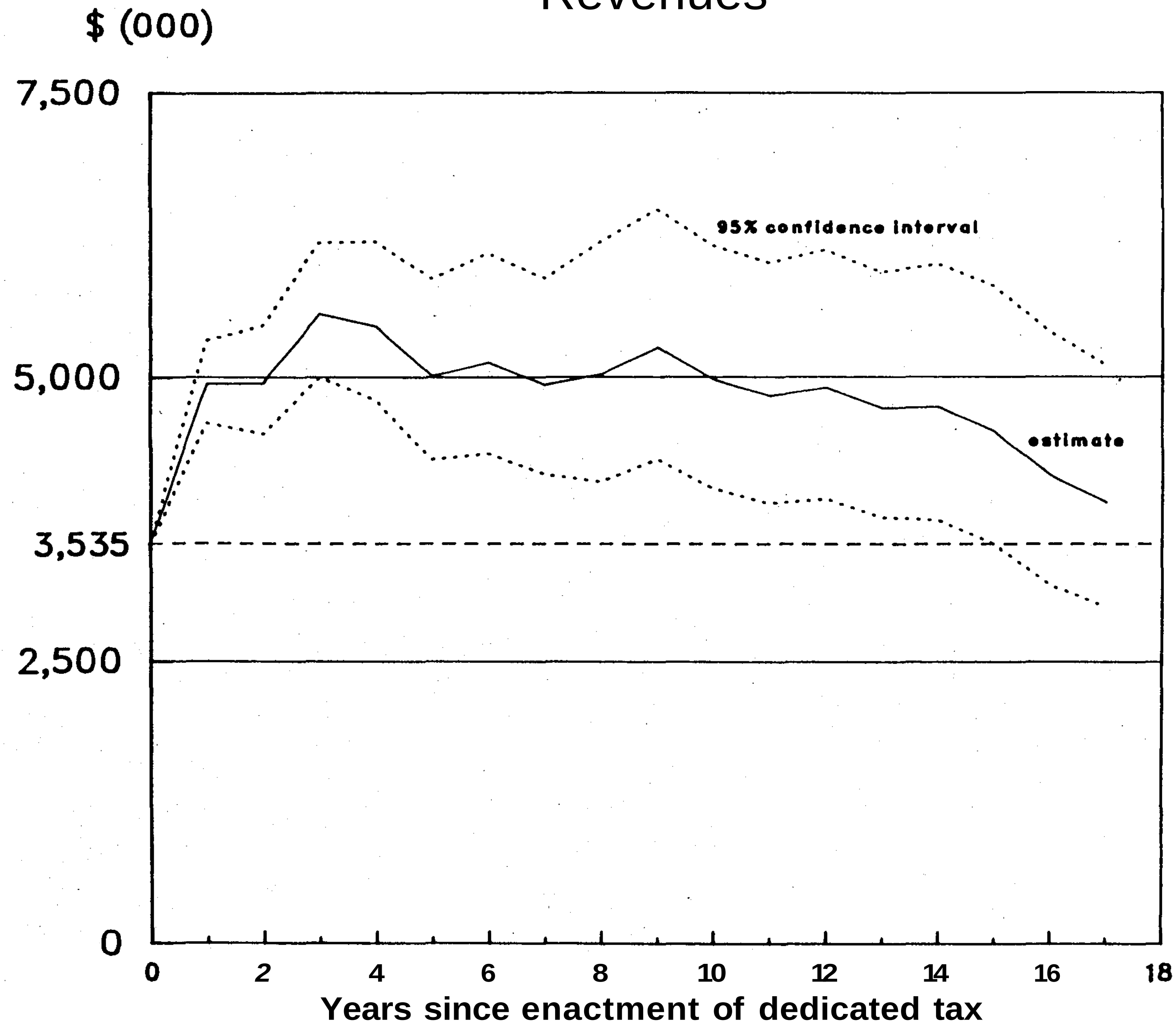
System Size



Source: Author's Calculations

Figure 3

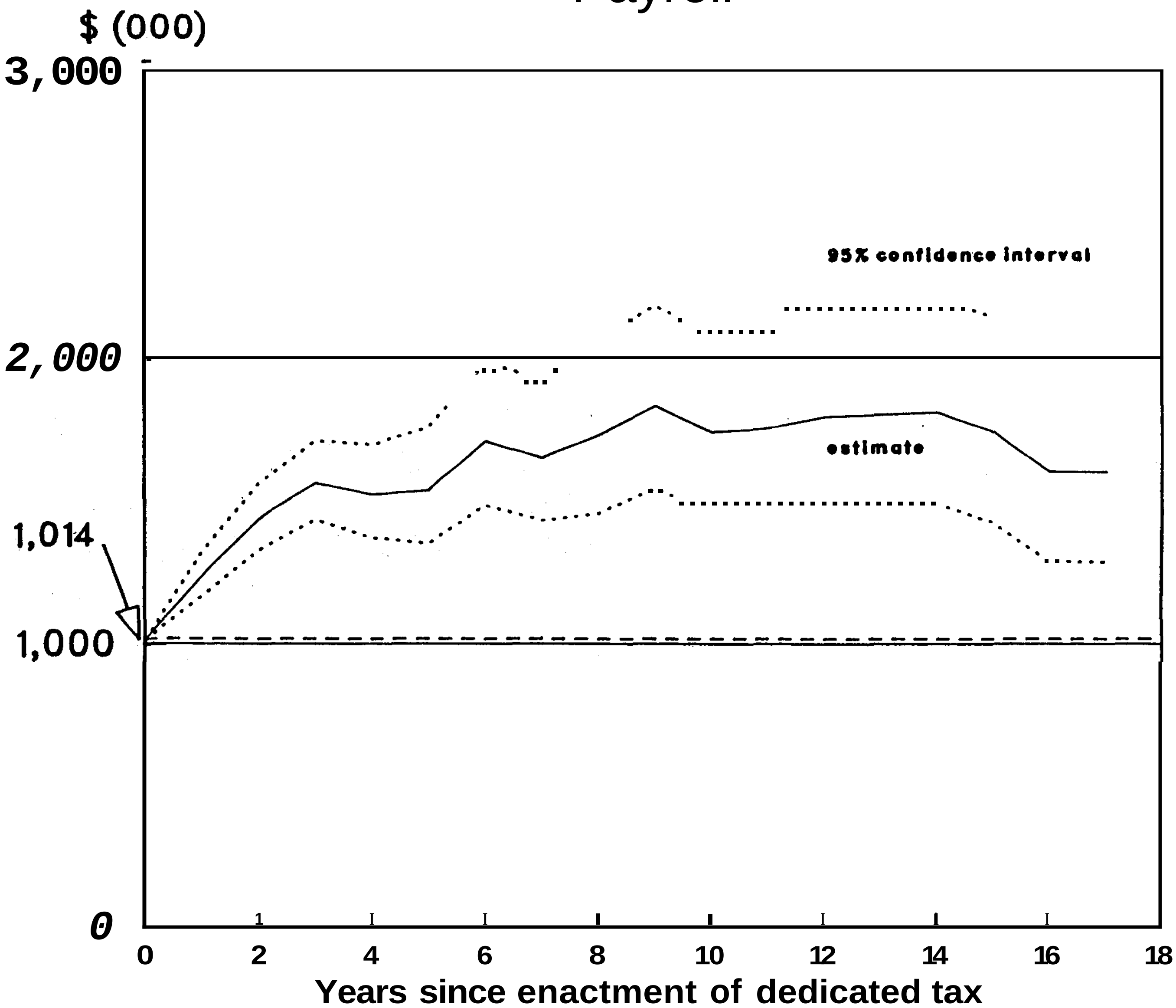
Revenues



Source: Author's Calculations

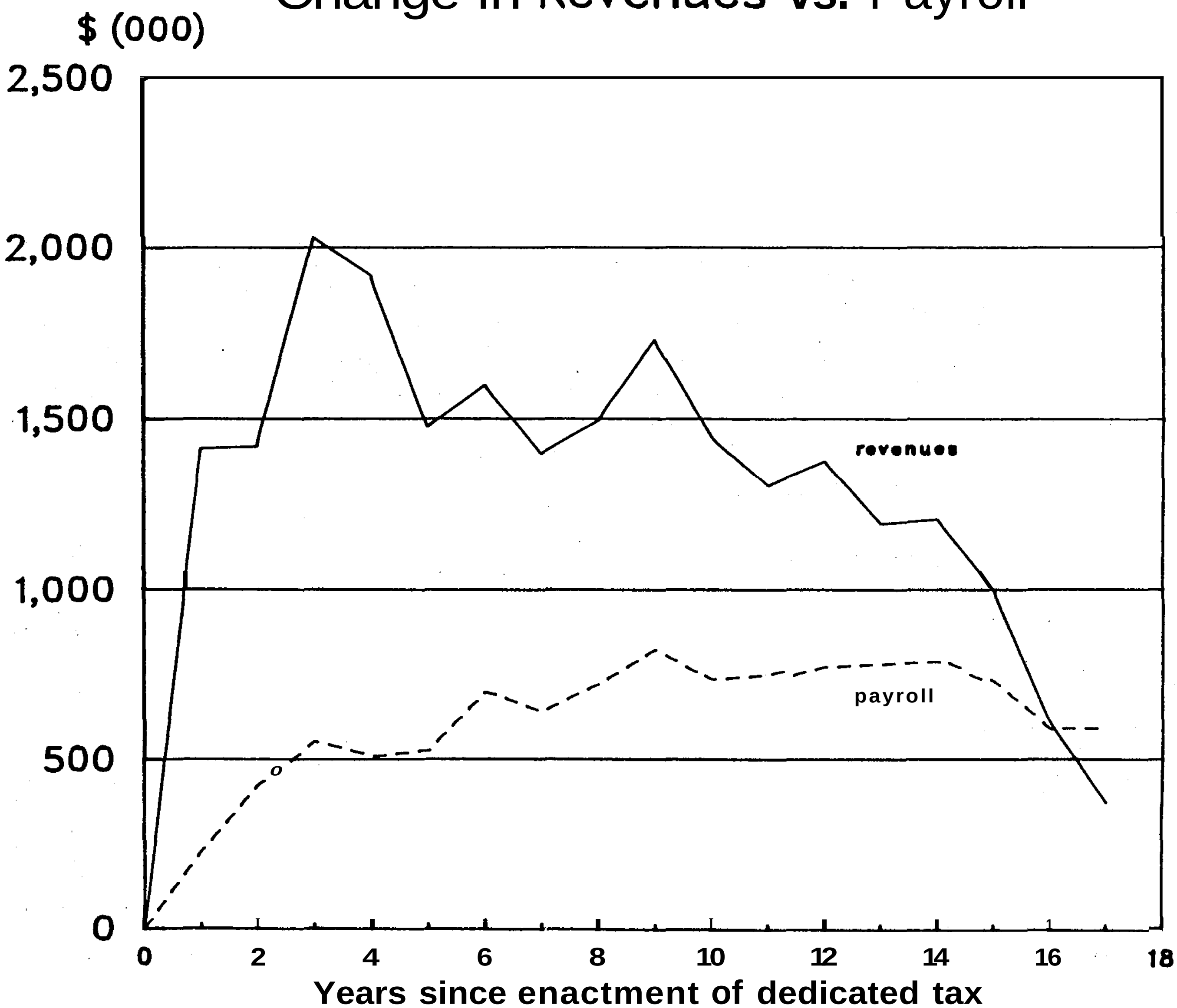
Figure 4

Payroll



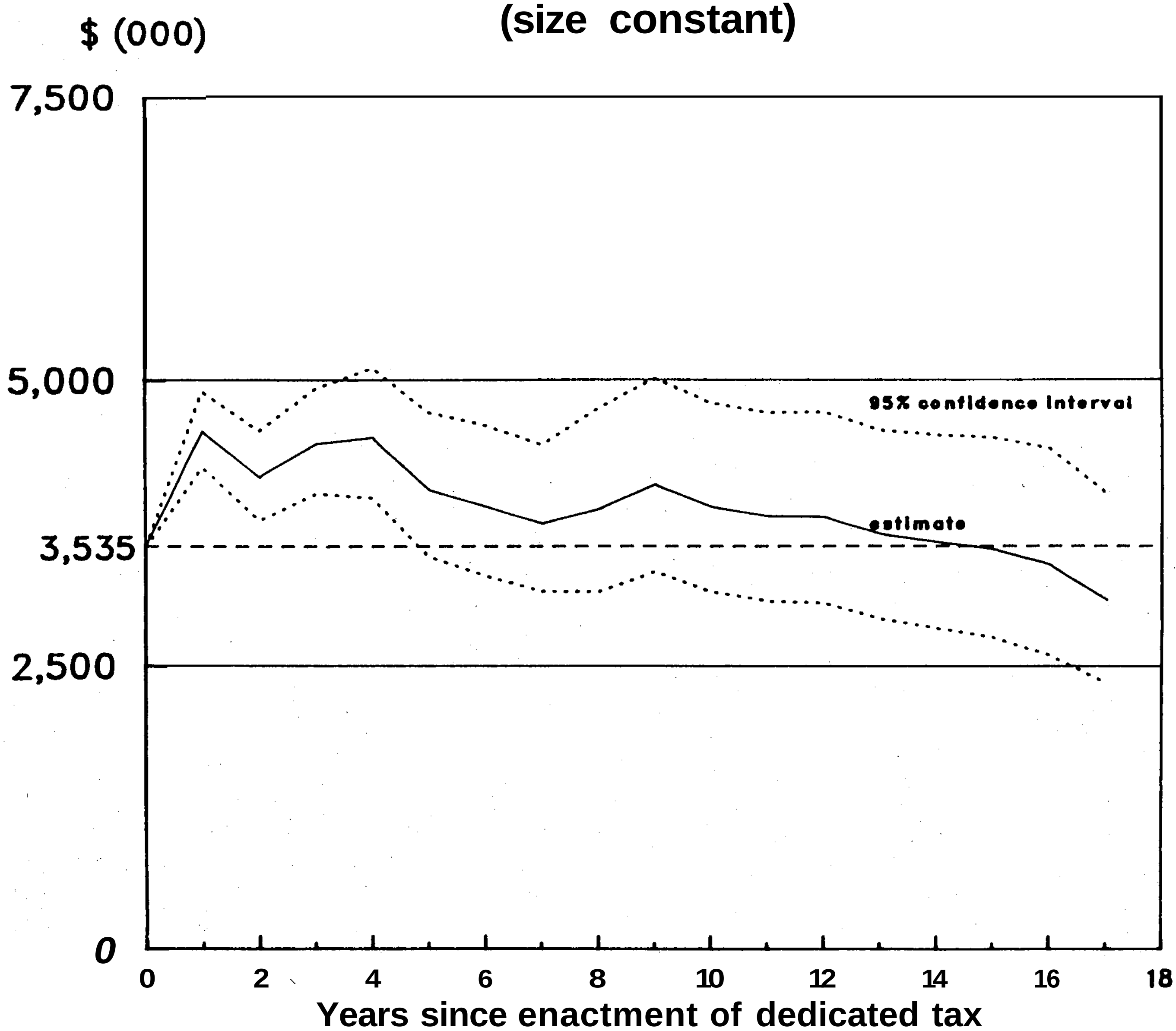
Source: Author's Calculations

Figure 5
Change in Revenues vs. Payroll



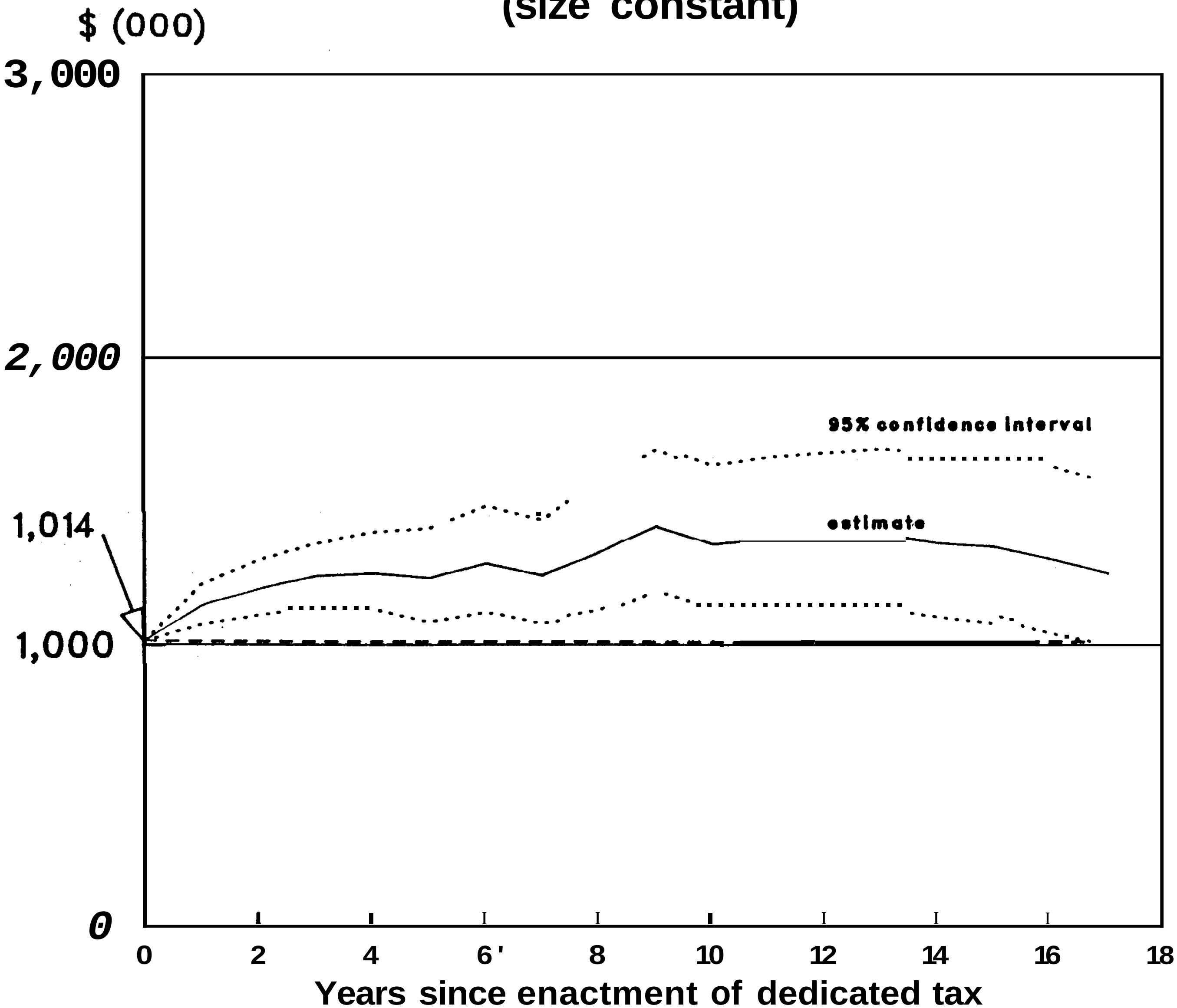
Source: Author's Calculations

Figure 6
Revenues
(size constant)



Source: Author's Calculations

Figure 7
Payroll
(size constant)

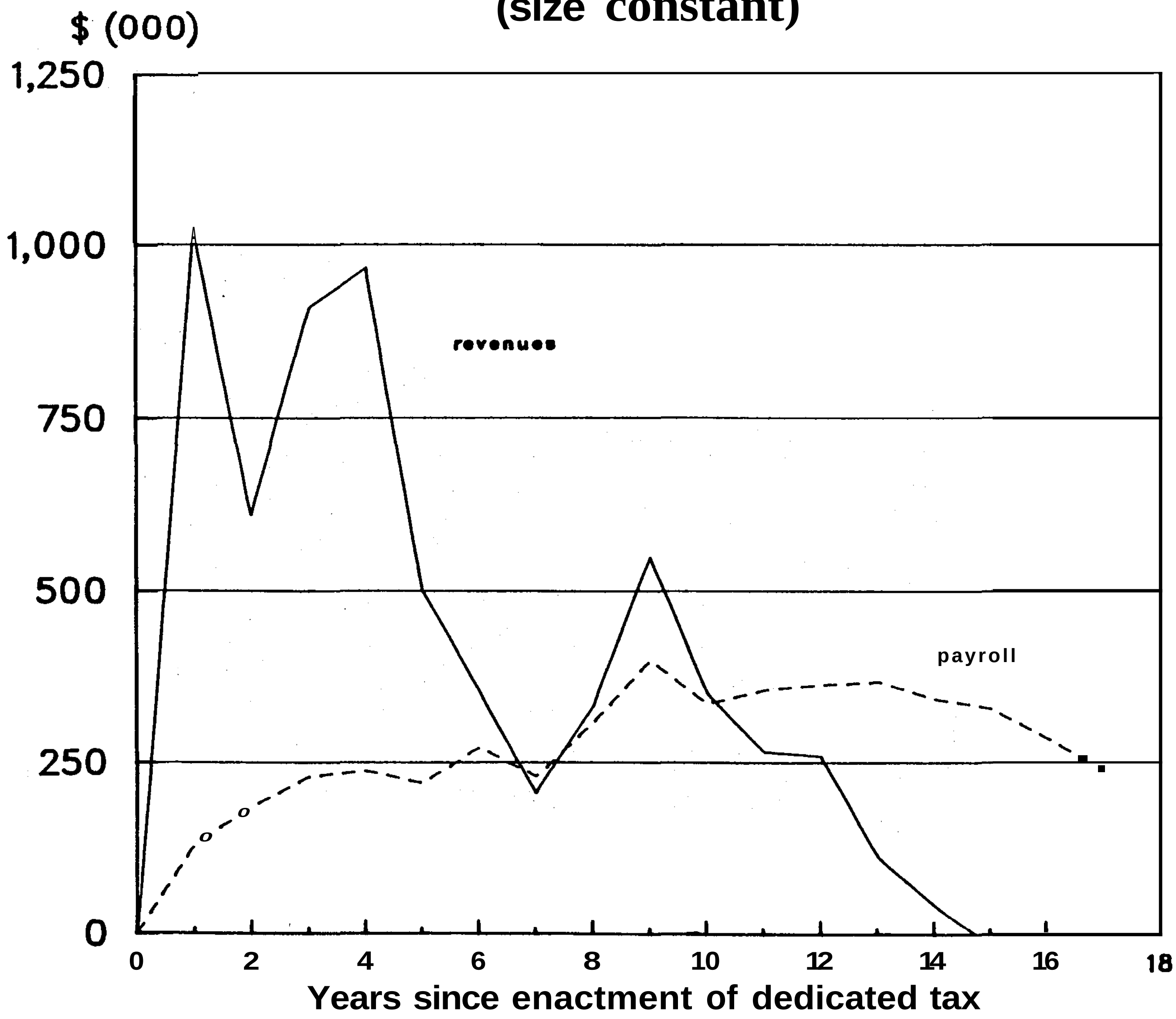


Source: Author's Calculations

Figure 8

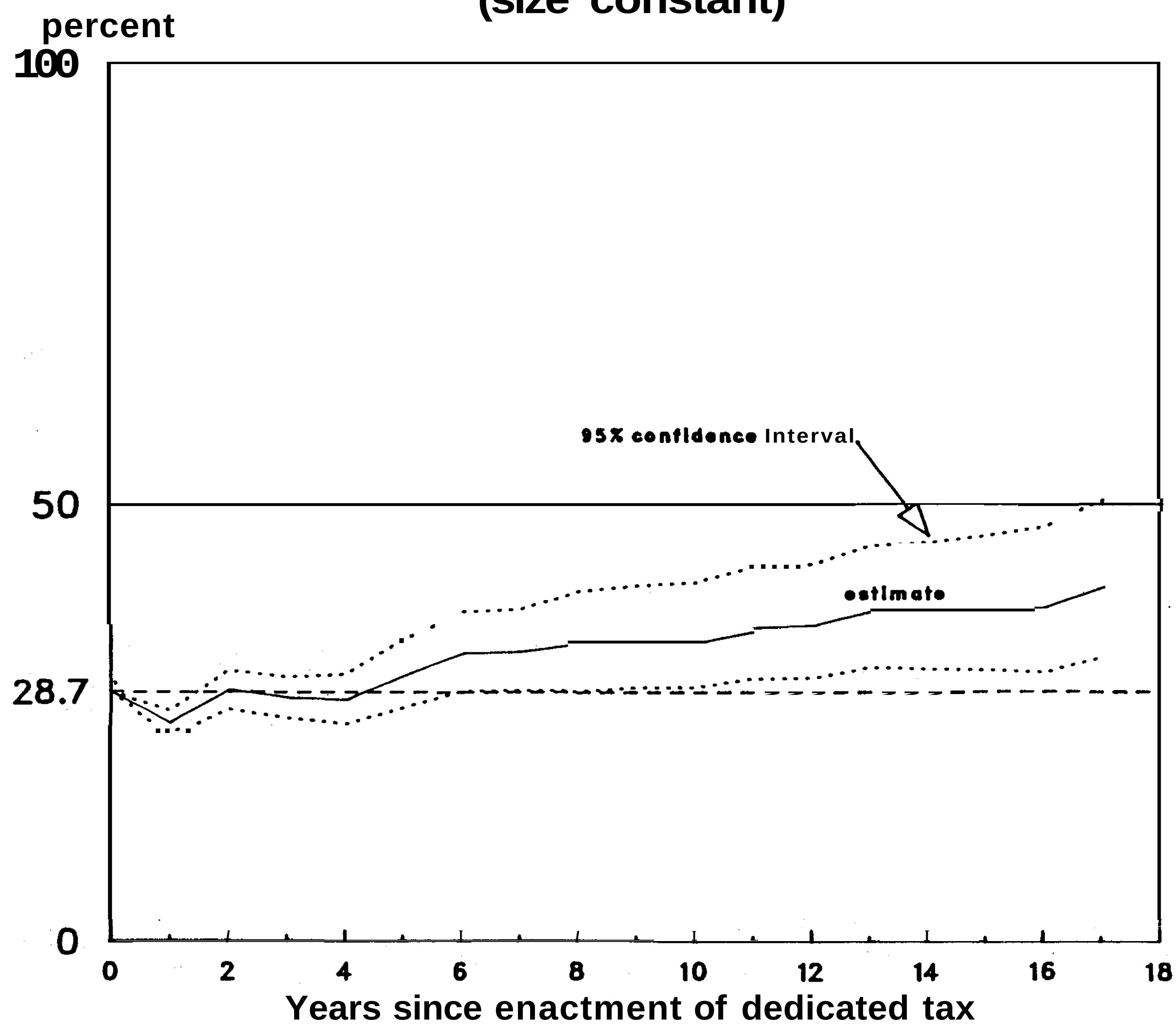
Change in Revenue vs. Payroll

(size constant)



Source: Author's Calculations

Figure 9
Payroll Share
(size constant)



Source: Author's Calculations