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The intraurban spatial distribution of employment: which government interventions make a difference?

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Abstract

An annual panel of employment at the census tract level for the Atlanta region is used to estimate the change in a tract's share of regional employment as a function of a variety of tax incentive programs, different transportation infrastructure investments, and crime. The results show that neighborhood-based property tax abatements, job tax credits, and highway improvements increase a tract's employment share. Higher crime is found to reduce employment share.
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1. Introduction

The spatial distribution of employment within metropolitan areas has long been an important topic in urban economics. Early interest in the shift in this distribution away from the central city in favor of the suburbs stemmed from concerns over the fiscal problems of the central city (Heilbrun [16, p. 44]) and spatial mismatch in the labor market among blue-collar workers (Kain [20]). In recent years, interest in employment decentralization has grown as the result of two additional concerns—concentrated inner-city poverty (Wilson [32]) and urban sprawl (President's Council on Sustainable Development [27]).

In response to these concerns, state and local governments have adopted various policies designed to alter the spatial distribution of employment within metropolitan areas. These policies can be grouped into two categories—transportation infrastructure improvements and business tax incentives. While considerable evidence exists on how these policies affect

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the inter- and intrastate location decisions of firms, there is little direct evidence on whether they alter the spatial distribution of employment within metropolitan areas. In general, the results from both inter- and intrastate studies show that government policies, with the possible exception of highway facilities, do not have much of an effect on where firms locate. However, as the geographical area over which the firm chooses its location gets smaller, alternative locations become closer substitutes. Hence, policies may have stronger effect within a metropolitan area than they have within a state or broader region.

To obtain a better understanding of governments' effect on the intrametropolitan distribution of employment, we have assembled a panel database at the neighborhood level for the Atlanta region for the years 1985 to 1997. The panel contains annual employment estimates and measures of government interventions at the census tract level. Atlanta is an interesting case study because a variety of place-based tax incentive programs were initiated and both transit and highway infrastructure improvements were made over the course of our 12-year panel. Controlling for tract and time effects, we estimate the change in a tract's share of regional employment as a function of both tax incentive programs (commercial–industrial enterprise zones, housing enterprise zones, and job tax credit zones) and transportation infrastructure improvements (new rapid rail stations and major highway improvement projects). We also focus on crime as a locational determinant, because its spatial incidence within a metropolitan area can be affected by various government interventions (Thaler [28]).

2. Literature review

Studies that have investigated the economic development effect of tax incentives, transportation improvement expenditures, and to a lesser extent, crime have generally used either regional or state level data.[1] However, recent studies of urban enterprise zones have used data on communities located within the same state (Papke [26], Boarnet and Bogart [6], Bondonio and Engberg [9]). Among these studies, only Papke finds that enterprise zones have an important economic effect. She finds that unemployment claims within Indiana's zones declined by about 19 percent following designation. Another within-state (California) study by Boarnet [5] finds that changes in a county's output are positively associated with changes in road capital, but output changes are negatively associated with changes in road capital in other counties, suggesting that investments in transportation infrastructure redistribute economic activity across a state's counties.

Whether the results of these studies apply within metropolitan areas is not known. However, estimates of the elasticity of business activity with respect to tax liabilities suggest that tax differences within metropolitan areas have a larger impact on firm location decisions than tax differences between metropolitan areas (Bartik [2]). This finding is attributed to the fact that different locations within a metropolitan area are better substitutes than locations in different metropolitan areas (O'Sullivan [25]). If taxes matter more for

[1] For reviews of these studies see Fisher [14], Wasylenko [31], and Bartik [2].

intrametropolitan firm location decisions, tax incentive programs may also have larger effect within metropolitan areas. The same logic can be applied to conclude that crime and transportation infrastructure improvements may have larger impacts on the distribution of employment within metropolitan areas than on the distribution of employment across these areas.

Studies that focus on the distribution of employment within metropolitan areas are few in number, because employment data for submetropolitan areas are difficult to obtain. Luce [21] uses data on municipalities within the Philadelphia metropolitan area and finds that the number of jobs within a municipality depends on whether it provides direct access to highway and/or rail transport. Bollinger and Ihlanfeldt [7], however, find that in Atlanta employment is no higher in rail station areas than in areas without access to rail transport.[2] Regarding crime, Mark et al. [22] find no evidence that reported crimes per capita affect the growth rate of employment among the jurisdictions that make up the Washington, DC, metropolitan area. In an earlier study using a sample of 62 metropolitan areas, Mills and Price [24] find that central city crime rates do not have an effect on employment (or population) suburbanization.[3] Finally, although Wassmer and Anderson [30] do not use employment data, their study deserves mention because it focuses on the effects of economic development incentives offered by jurisdictions within the Detroit metropolitan area. They find that the local offer of property tax abatements does not exert a positive influence on commercial property value, but that commercial property value is higher in cities that use either tax increment financing or downtown development authorities.

3. Tax incentives within the Atlanta region

Three types of neighborhood-based tax incentives existed in Atlanta during the years covered by our panel—commercial–industrial enterprise zones, housing enterprise zones, and job tax credits. Each of these programs is described below.

3.1. Commercial–industrial enterprise zones

Over the years covered by our panel, enterprise zones were limited to the City of Atlanta.[4] The city's program, which began in 1983, provides property tax abatements to commercial and industrial properties that locate in census tracts designated as depressed.[5]

[2] Bollinger and Ihlanfeldt [7] control for non-station influences on employment and the non-random selection of station sites by including an extensive set of control variables. In contrast, the model described below controls for these factors using annual panel data with fixed effects.

[3] Cullen and Levitt [13], however, find that rising crime rates in central cities are correlated with central city depopulation. They do not investigate the relationship between crime and employment location.

[4] The terminology used in both the commercial–industrial and housing parts of the City of Atlanta's enterprise zone program designates the properties receiving tax abatements as zones rather than the census tracts that are eligible. We choose to employ the more common terminology that labels the eligible area as the zone.

[5] Commercial and industrial property encompasses employment in all types of industries. Hence, these zones do not target particular kinds of jobs.

The latter designation is based on the tract's poverty rate, unemployment rate, and job losses over the past five years. The tax abatements continue for 25 years, gradually declining from 100 percent in the first 5 years to 20 percent in the last 5 years. Over time, tracts that improve lose their eligibility, while others that decline are added to the program.[6] In addition, eligibility criteria were relaxed in 1990 and then strengthened in 1996. For these reasons there is time series variation within the tracts of our panel. From 1984 to 1989, 44 census tracts were eligible for abatements. To this total were added 26 tracts in 1990. In 1996 there was a total of 46 eligible tracts.[7]

3.2. Housing enterprise zones

In 1987, the City of Atlanta's enterprise zone program was extended to include housing construction and rehabilitation. The primary goal of the program is to attract population to economically depressed, underpopulated areas within the city. However, the expectation is that population growth in these areas will bring jobs; hence, employment expansion is a secondary goal of the housing enterprise program (City of Atlanta [12]).

As for commercial–industrial zones, property tax abatements are available within housing zones on a declining schedule, but their duration lasts only 10 years in comparison to the 25 years for non-residential investments. Similar but not identical criteria are used to define eligible tracts for the commercial–industrial and housing parts of the program. Specifically, while both rely upon the tract's poverty and unemployment rates, a third criterion differs between the two parts of the program: job losses for the commercial–industrial zones versus population for the housing enterprise zones. As a result of the differences in eligibility criteria, the two sets of eligible tracts overlap but are not identical. Again, over the course of our panel, there is intertemporal variation among tracts that are eligible for housing enterprise zone tax abatements. In 1987–1988, 17 tracts were eligible; since then 44 tracts have been eligible.

3.3. Job tax credits

In 1989, the state of Georgia established a job tax credit that allowed manufacturing firms in depressed counties to receive a credit against their annual state income tax obligation for each new job created. In 1994, the job tax credit program was expanded to include depressed census tracts in the state's urban areas. Eligible tracts are defined based upon three equally weighted factors: the unemployment rate, per capita income, and the poverty rate. Manufacturing firms in an eligible tract that create 10 or more jobs receive a $2500 annual credit for each new job for five years. A credit claimed but not used in any

[6] Firms that located in a tract when it was eligible for tax abatements do not lose their abatements if the tract subsequently becomes ineligible.

[7] Johnson [19] reports that in 1998 more than three million dollars of property taxes were abated as the result of participation in the City's commercial–industrial and housing enterprise zone programs. The effective rate of property taxation within the City of Atlanta is approximately 2%.

taxable year may be carried forward for 10 years. Of the 299 tracts in our panel, 81 are eligible for job tax credits.[8]

4. The Atlanta panel data

The study area is the seven-county Atlanta region. This region includes the City of Atlanta and the counties contained within the inner ring of suburbs of the Atlanta MSA. In 1990, roughly the midpoint of our panel, which covers the years 1985–1997, the Region accounted for 78 percent and 87 percent of the MSA's population and employment, respectively. Census tracts as they existed in 1980 are used to define neighborhoods. There were 299 census tracts in the Region in 1980. All data are measured using 1980 census tract geography.

The annual employment estimates for each census tract come from the Atlanta Regional Commission (ARC), which is the official planning organization for the Atlanta region.[9] These estimates are based upon firm-level ES-202 data from the Georgia Department of Labor. These are county-level data that only include jobs at establishments covered by unemployment insurance. ARC researches each employer and disaggregates the county level totals to specific employment locations. In addition, non-covered employment is accounted for by directly surveying non-covered establishments.[10]

Each of the independent variables included in the panel, along with its source, is described in Table 1. The three tax incentive programs are represented by separate dummy variables that equal one if the tract was eligible in a particular year and zero otherwise. The transportation infrastructure variables consist of two variables—real expenditure of completed road improvements in the tract and the percentage of a rapid rail station impact area in the tract. A rail station impact area is defined as a circle centered on the station with a radius of a quarter mile. A quarter mile (2.5 blocks) is used as the radius of the impact area because walking distance is most commonly defined as being within a quarter mile of a station (Bernick and Carroll [3], Cervero [11], Untermann [29]). Crime is measured as the total crime rate (reported crimes/population) for the jurisdiction in which the tract is located.[11] There are 53 jurisdictions identified within the panel (46 municipalities and 7 counties serving unincorporated areas).

Other variables included in the panel are the median income of the tract and the following variables measured at the jurisdictional level: property tax rate, sales tax rate

[8] As required by the rules of the Georgia Department of Community Affairs, Chapter 110-9-1, Job Tax Credit Program Regulations, the Georgia Department of Revenue must report annually the number of credits earned by county. It is not required that the credits earned from locating in a depressed census tract be reported and we were told by Department of Revenue personnel that these data are not currently collected. It was confirmed, however, that firms are participating in this program.

[9] These estimates are published by ARC in annual reports entitled *Employment*.

[10] No distinction is made in ARC's employment estimates between full and part-time jobs or between salaried and equity positions (owners, proprietors, and self-employed persons).

[11] While it may have been better to measure crime at the census tract level, these data are not publicly available. However, jurisdictional crime rates are available and for this reason firms may base their locational decisions on these data. If this is the case, the jurisdictional crime rate is the preferred crime measure.

Table 1
Variable descriptions

Variable	Description	Source
Tax incentives		
Commercial enterprise zone	Whether tract is eligible for commercial–industrial enterprise zone property tax abatements	City of Atlanta
Housing enterprise zone	Whether tract is eligible for housing enterprise zone property tax abatements	City of Atlanta
Job tax credit tract	Whether tract is eligible for state job tax credits	Georgia Department of Community Affairs
Transportation infrastructure		
Rail station	Percentage of MARTA rail station impact area in tract[a]	Digitized map
Real highway improvement exp.[b]	Expenditure on completed road improvements in tract measured in 100s of real dollars (1992 is base)	Georgia Department of Transportation
Crime rate	Total crime rate for the jurisdiction in which the tract is located	Georgia Bureau of Investigation
Real median income	Real median income of tract	Donnelly Marketing Services[c]
Property tax rate	Property tax millage rate for the jurisdiction in which the tract is located	Georgia Department of Revenue
Sales tax rate	Sales tax rate for the county within which the tract is located	Georgia Department of Revenue
Government expenditures[d]	Real per capita annual expenditure on police, fire safety, parks, and sewerage	Census of Governments

[a] A quarter-mile ring around each station is defined as the impact area.

[b] Highway improvement projects include new road construction, road widenings, bridge replacements, and major improvement projects done on intersections. Small projects, defined as those costing less than $8 million (summing across all affected tracts), are excluded.

[c] Donnelly's income estimates are updates of decennial census numbers obtained via multivariate modeling techniques that utilize data from telephone surveys and secondary resources.

[d] Values for these variables are interpolated for non-Census of Government years. Interpolations were based on constant percentage growth between non-census years. The percentage growth between census years was calculated for each variable in each jurisdiction. The rate was annualized and then applied to arrive at the interpolated values.

(varies only across counties), and real expenditures on police, fire safety, parks, and sewerage. The government expenditure variables are available only every 5 years and therefore had to be interpolated for intervening years.

5. Empirical model

In the model we estimate the dependent variable is the annual change in the census tract's share of regional employment. There are two questions that arise concerning this variable: (1) Why use the tract's employment share rather than total employment within

the tract, and (2) Why use share change rather than share level? Each of these questions is addressed below.

We focus on the change in employment share rather than the absolute or percentage change in employment for three reasons. First, our interest is in how tax incentives, transportation improvement expenditures, and crime affect the intraurban spatial distribution of employment, which can be directly estimated from a share change equation. Second, the change in a tract's employment will consist of two components—the portion due to tract specific factors, which is our interest, and the portion due to factors that have a common effect across tracts (e.g., a regional business cycle). Because the latter factors will affect the criteria used to determine the tract's eligibility for the various tax incentive programs we investigate, the estimation of an employment change equation could result in biased estimates if one or more the these factors are excluded from the model. Third, in a region such as Atlanta, where population and employment have grown dramatically over the period in question, average employment growth at the census tract level will be high. Similarly, because of growing population, many of the expenditure variables are growing as well. By using changes in share, the specific tract level relationships can be isolated from the spurious relationship caused by overall regional growth. While fixed effect models may be able to control for this, the share equations combined with first differences and fixed effects will better isolate the direct causal effect of interest.

The advantages of defining the dependent variable as the share change rather than the share level have recently been outlined by Mark et al. [22]. First, a change variable nets out systematic differences across tracts due to history or scale effects. Second, level variables, but not change variables, are highly persistent over time and therefore may result in spurious regression.

Our panel data also allow us to control for census tract fixed effects, which allow employment shares to grow at different rates across tracts and which controls for unobservable heterogeneity across tracts. The latter heterogeneity may include factors that have an independent effect on the change in employment share and these factors may be correlated with our policy variables. The inclusion of fixed effects minimizes bias that may result from omitted variables. Also included are year dummy variables measuring time effects, which are required when the dependent variable is a share variable.[12]

A final estimation issue concerns the non-random site selection of the government interventions we investigate. Our use of fixed effects along with the dependent variable measured in *change* in employment share controls for fixed effects in both the level and the growth rate. Since both the employment level and employment growth of a census tract are used (in part) to determine qualification for the enterprise zone programs, this could

[12] The need for year dummy variables can be seen by first recognizing that summing the changes in employment share across tracts equals zero for any year. Since the OLS regression imposes the restriction that the sum of the error terms is also zero, the adding up condition on the dependent variable (the change in share) will impose a restriction on the slope coefficients of the explanatory variables (x_{it}), unless the average values of these variables equal zero over the whole sample. The inclusion of year dummy variables is algebraically equivalent to calculating each x_{it} relative to the average in each time period ($\bar{x}_t$). Hence, the sum of the $x_{it} = 0$ in every year, and no restriction is imposed on the estimated coefficients. The year dummy variables measure the change in the average X values over time, rather than the change in the average value of the dependent variable (which is constant).

lead to bias in the coefficients from endogeneity of the enterprise zone. Use of fixed effects along with the growth dependent variable prevents this bias.

The basic model that we estimate can be expressed as

$$E\Delta_{i,t} = \alpha_i + \delta_t T_t + \beta_1 \mathrm{CEZ}_{i,t-1} + \beta_2 \mathrm{HEZ}_{i,t-1} + \beta_3 \mathrm{JTC}_{i,t-1} + \gamma_1 R_{i,t-1} + \gamma_2 H_{i,t-1} + \phi I_{t-1} + \mu_{i,t}, \quad (1)$$

where:

- $E\Delta_{i,t}$: change in the ith tract's share of regional employment in year t,
- α_i: tract-specific effect,
- T_t: year dummy variables,
- $\mathrm{CEZ}_{i,t-1} = 1$ if tract i was eligible for commercial–industrial enterprise zone property tax abatements in year $t-1$, otherwise 0,
- $\mathrm{HEZ}_{i,t-1} = 1$ if tract i was eligible for housing enterprise zone property tax abatements in year $t-1$, otherwise 0,
- $\mathrm{JTC}_{i,t-1} = 1$ if tract i was eligible for job tax credits in year $t-1$, otherwise 0,
- $R_{i,t-1}$: the percentage of a rapid rail impact area located in tract i in year $t-1$,
- $H_{i,t-1}$: real expenditure on highway improvements completed in tract i in year $t-1$,
- I_{t-1}: median income in tract i in year $t-1$.

Model (1) is estimated separately for all private employment and for manufacturing employment. The latter equation is estimated because we are interested in the effectiveness of job tax credits, which are targeted to manufacturing firms.

Except for T_t, the independent variables in model (1) all vary across census tracts and time. We also estimate two additional models: model (2) adds to model (1) three variables for which we have annual data that vary over jurisdictions: property tax rate, sales tax rate, and the crime rate; model (3) adds to model (2) local government expenditure variables that vary over jurisdictions but are available only every 5 years and therefore are interpolated for the intervening years: real expenditures on police, fire safety, parks, and sewerage.[13] Because employment does not respond instantly to changing circumstances, the explanatory variables in all three models are lagged one year.

The three models are estimated in order to examine how control variables may affect the results. In general, only limited control variables have been available in previous work, which resulted in estimated models similar to model (1). Our three specifications allow us to determine if differences between our results and those from previous studies are likely due to differences in geographic level observation or differences in control variables. Also, results which are stable across all three models are more informative than results that change from model to model. Finally, concerns can be expressed about the interpolation of the census of government variables. Comparison of results across models again allows some interpretation of how the interpolated variables affect estimation.

[13] Interpolations were based on constant percentage growth between non-census years. The percentage growth between census years was calculated for each variable in each jurisdiction. The rate was annualized and then applied to arrive at the interpolated values.

The expected signs on the estimated coefficients of the tax incentive, transportation infrastructure, and crime variables are all unambiguous, with the exception of rapid rail transit. Locating near a rail station increases the firm's access to labor supply which is expected to reduce its offer wage. In addition, transit riders may increase consumer demand in station areas. These reasons suggest that tracts within walking distance of a rapid rail station will have greater growth in employment share. However, opposing these effects are two others that may act to repel employment from station areas—higher crime and a greater number of Black job applicants. Bowes and Ihlanfeldt [10], who also use Atlanta data, find that the density of neighborhood crime is higher in those census tracts whose centroids are within a quarter mile of a rail station. Block and Block [4] report similar findings for Chicago and New York. Using data from the Multi-City Study of Urban Inequality Employers Survey, Holzer and Ihlanfeldt [18] find that firms located within walking distance of public transit receive a higher percentage of job applications from Blacks. Prejudiced employers therefore may deliberately locate away from station areas to avoid black job applicants (Holzer [17, p. 95]).[14]

6. Results

The results from estimating the three models for total private employment and manufacturing employment are reported in Tables 2 and 3, respectively. For each variable we report the estimated coefficient, the estimated robust standard error, and the estimated change in the number of jobs within the tract implied by the estimated change in employment share caused by an assumed change in the variable.[15] For commercial enterprise zones, housing enterprise zones, and job tax credit tracts the assumed change is one (i.e., the tract changes from ineligible to eligible to receive the tax incentives). For real median income, the property tax rate, the sales tax rate, the crime rate, and the government expenditure variables the assumed change is a one-standard deviation increase in the variable. For rail stations the assumed change is going from the tract having no part of a rail station impact area to the tract having the average percentage of an impact area among those tracts that contain station areas (36 percent). For real highway improvement expenditures the assumed change is going from the tract not having any improvements to the tract having the average amount of improvements among tracts that have had improvements (16,622 hundred dollars of expenditure or 9.72 log points). All changes are computed using 1990 values.

[14] The hypothesis that employers may locate away from rail stations to avoid black applicants suggests that the racial composition of the residents of the census tract be included as an independent variable. In preliminary estimation, the percent of the population living in the tract that is black was included in all of our employment share regressions. It was never statistically significant. We choose not to include this variable in final estimation because we believe it may be measured with considerable error. The variable is based on annual field surveys conducted by the Atlanta Regional Commission.

[15] The estimated change in the number of jobs within the tract is computed by multiplying the assumed change in the independent variable by its estimated effect on the change in employment share and then multiplying by the total number of private sector jobs found within the Atlanta region in 1990 (1,180,157).

Table 2
Results for change in total employment share of the tract

Variables	Models		
	(1)	(2)	(3)
Commercial enterprise zone	0.0000721***	0.0000703***	0.0000648***
	(0.000023)[a]	(0.000028)	(0.000028)
	[85][b]	[83]	[76]
Housing enterprise zone	−1.07e−07	0.0000156	−1.22e−06
	(0.000025)	(0.0000269)	(0.000026)
	[0]	[18]	[−1]
Job tax credit tract	0.0000542**	0.0000429*	1.24e−06
	(0.000027)	(0.000027)	(0.000029)
	[64]	[51]	[1]
Rail station	−2.55e−06	−2.44e−06	−2.43e−06
	(2.32e−06)	(2.31e−06)	(2.33e−06)
	[−108]	[−104]	[−103]
Real highway improvement exp.[c]	5.52e−06*	5.68e−06*	6.84e−06**
	(3.59e−06)	(3.60e−06)	(3.63e−06)
	[63]	[65]	[78]
Real median income	0.0000391	0.0000241	0.0000130
	(0.000038)	(0.000038)	(0.000037)
	[29]	[18]	[9]
Property tax rate		1.04e−06	−8.24e−07
		(2.58e−06)	(3.58e−06)
		[9]	[−7]
Sales tax rate		−0.0000288	−0.0000311
		(0.0000353)	(0.000036)
		[−26]	[−28]
Crime rate		−0.0014775**	−0.0014639**
		(0.000666)	(0.000684)
		[−134]	[−133]
Real police exp.[c]			0.0000674
			(0.000105)
			[26]
Real fire safety exp.[c]			0.0001522*
			(0.000098)
			[60]
Real parks exp.[c]			0.0001041**
			(0.000057)
			[86]
Real sewerage exp.[c]			−0.000016
			(0.000012)
			[−19]
Constant	−0.0003826	−0.0000938	−0.0009547
	(0.000395)	(0.000467)	(0.000596)
Observations	3588	3588	3588
R-square	0.276	0.279	0.282

*,**,*** Indicates significance at the 10%, 5%, and 1% level, respectively. One tailed test on all variables except rail station and crime rate.

[a] Standard errors in parentheses.

[b] Number in brackets is the estimated change in the number of jobs within the tract from the change in employment share caused by a change in the explanatory variable. For commercial enterprise zones, housing enterprise zones, and job tax credit tracts, the change is 1. For real median income, property tax rate, sales tax rate, crime rate, and the government expenditure variables, the change is a one-standard deviation change in the variable. For rail stations and real highway improvement expenditures, the change equals the mean value for tracts that contain a rail station impact area and experienced a road improvement, respectively. All changes are computed using 1990 values.

[c] Indicates variables that are logged.

Table 3
Results for change in manufacturing employment share of the tract

	Models		
Variables	(1)	(2)	(3)
Commercial enterprise zone	−0.0000262	−0.0000263	−0.0000368
	(0.000075)[a]	(0.0000749)	(0.0000368)
	[−4][b]	[−4]	[−5]
Housing enterprise zone	0.0000327	−1.79e−06	5.43e−07
	(0.000099)	(0.000101)	(0.000099)
	[5]	[0]	[0]
Job tract credit tract	0.0002115**	0.0002224**	0.0002306**
	(0.000107)	(0.000109)	(0.000112)
	[31]	[32]	[33]
Rail station	−4.74e−06	−5.08e−06	−5.16e−05
	(6.74e−06)	(6.74e−06)	(6.72e−06)
	[−25]	[−26]	[−27]
Real highway improvement	0.0000129*	0.000013*	0.0000132*
expenditure[c]	(9.62e−06)	(9.62e−06)	(9.87e−06)
	[18]	[18]	[18]
Real median income[c]	−0.0001986	0.0002248	0.0002227
	(0.000180)	(−0.000181)	(0.000180)
	[18]	[20]	[20]
Property tax rate		−2.57e−06	−8.82e−06
		(6.75e−06)	(0.000010)
		[−3]	[−9]
Sales tax rate		−0.0000941	−0.0000935
		(0.000103)	(0.000106)
		[−11]	[−9]
Crime rate		0.0035089***	0.0029269***
		(0.00121)	(0.00120)
		[39]	[33]
Real police exp.[c]			−0.0004169
			(0.000360)
			[20]
Real fire safety exp.[c]			−0.0003473
			(0.000375)
			[−17]
Real parks exp.[c]			−0.0000715
			(0.000189)
			[−7]
Real sewerage exp.[c]			−1.90e−06
			(0.000034)
			[0]
Constant	−0.0019305	−0.0020134	−0.0019668
	(0.001883)	(0.001534)	(0.00188)
Observations	3588	3588	3588
R-square	0.102	0.105	0.105

*,**,*** Indicates significance at the 10%, 5%, and 1% level, respectively. One tailed test on all variables except rail station and crime rate.
[a,b,c] See Table 2.

Consider first the results in Table 2 for total private employment.[16] Commercial enterprise zones have a positive and statistically significant effect (1% level, one-tailed test) in all three models.[17] The annual increase in share is about 0.00007, which translates into an increase of about 80 jobs per annum.[18] In contrast, housing enterprise zones do not have an effect on the growth in employment share in any of the models. These results suggest that these zones are not accomplishing their secondary goal of employment expansion. One reason for this is that population growth, which is the primary goal, may also not be occurring. Alternatively, jobs may not be following people into these areas. While the statistical significance of commercial and housing enterprise zones do not vary across the three models, the level of statistical significance of job tax credits falls moving across the three columns of Table 2, with the effect not significantly different from zero at even the 10 percent level in model (3). The weakness in the effects of the job tax credits relative to those of commercial enterprise zones on total employment is not surprising, given that the former program is targeted to manufacturing industries. As noted below, job tax credits do have consistently strong effects across the three models estimated for manufacturing employment share.

Turning next to the transportation improvement variables, tracts containing rail station impact areas lose employment share but this effect is never statistically significant. This result, which is consistent with the findings of Bollinger and Ihlanfeldt [7], suggests that the attractive and repellent forces of rail stations on employment are offsetting. In contrast, an average amount of highway improvements within the tract causes employment share to grow by about 0.00006 in the year following the completion of the improvements, which translates into a job gain of about 70 jobs.[19]

The final key policy-related variable is the crime rate. Higher crime in the jurisdiction within which the tract is located causes a loss in employment share and the significance of this effect falls just below the 1% level. A one standard deviation increase in the crime rate causes the average tract to lose 0.000114 employment share (or about 130 jobs) in the year following the rise in crime. The relative magnitude of the job loss from crime is large because a standard deviation increase in the crime rate represents a large change in crime. The overall variation in crime is high within the Atlanta region because there are large differences in crime between the city and suburbs: the city of Atlanta's crime rate, which has consistently been among the nation's highest, is roughly four times higher

[16] The residuals of all models were tested for spatial autocorrelation using a Moran's I test. The test statistic ranged from −0.98 for the short private shares specification through 1.05 for the long manufacturing specification. There is no strong evidence for spatial autocorrelation.

[17] As noted above, except for rail stations, there is a sign hypothesis for all of our government intervention variables. Hence, two-tailed tests of significance are used for rail stations and one-tailed tests are used for the other variables. A significance level down to the 10% level is used. As Mark et al. [22] have argued, the use of time and fixed effects allow a more liberal consideration of significant p values in determining statistical significance.

[18] The average share is 1 divided by the number of census tracts (299), which equals 0.0033.

[19] In addition to the road improvements variable, we examined other measures of road capital. Measures of overall highway such as dummies for a highway in the tract or total road expenditures from Census of Government data were insignificant. Dummy variables for road improvements projects were typically significant and positive and qualitatively similar to the results reported here.

than the crime rate of the median suburban jurisdiction. From a policy perspective, a more reasonable assumed increase in crime is a standard deviation change based upon the within tract variation. This is about one-third the size of the standard deviation based upon the overall variation (i.e., the between plus within tract variation) and yields a job loss of about 40 jobs.[20]

Among the control variables, neither the property tax rate nor the sales tax rate registers a statistically significant effect on the change in employment share. These results suggest that the growth in the tract's employment share is not affected by the tax rates established by the jurisdiction within which the tract is located.[21] However, not too much should be made of these results. As suggested by the tax competition literature (Wilson [33]), intertemporal movements in tax rates are highly correlated across jurisdictions over the years covered by our panel, making it difficult to isolate their effect. Additionally, one would expect capital to be most affected by differences in property tax rates. The effect on employment is therefore ambiguous, and depends on whether labor is a gross substitute or gross complement of capital.

Of the government expenditure variables, higher expenditures on fire safety and parks are found to increase employment share, with park expenditures having the larger effect. A one standard deviation increase in the log of park expenditures increases employment share by 0.00007, which translates into about 86 more jobs. Greater park expenditures may attract employment if workers are willing to work for less at sites where park land is located nearby.[22]

Turning to the results obtained from estimating the change in the tract's share of regional manufacturing employment, the key finding is that job tax credits are found to have their intended effect of attracting manufacturing jobs to depressed census tracts. In all three models becoming eligible for job tax credits has a statistically significant effect at the 5% level on the change in manufacturing employment share. Share increases by about 0.0002, which translates into about 32 additional jobs per year. Other variables found to increase manufacturing share are highway improvement expenditures (at the 10% level) and the

[20] It is possible that the estimated effect of the jurisdictional crime rate on tract employment reflects changes in the overall desirability of the jurisdiction as a place to live and work. A jurisdiction that becomes more desirable may attract a larger share of the region's jobs and experience lower crime as the result of an improvement in the socio-economic status of its residents. To investigate this possibility 1990 data from the Census of Population and Housing were used to regress the total household income of the tract on the median income of the tract, the population of the tract, the squares of each of these variables and their interaction. This model, which explained 85% of the variation in tract total income, was used to predict the jurisdictional level of total household income for each year of the panel. The annual tract median incomes are those included in all of our estimated models and were obtained from Donnelly, Inc. while the annual tract population estimates are those published by the Atlanta Regional Commission. The latter are updates of decennial census numbers obtained from monitoring changes in housing inventories (via building and demolition permits) and conducting field surveys. When the lagged value of total income of the jurisdiction was added to our estimated employment models, it was never statistically significant with the expected positive sign and there was little change in the estimated crime coefficients or their levels of statistical significance. The results obtained on all other variables were also largely unaffected.

[21] Mark et al. [22] also find that differences in property tax rates fail to affect employment growth rates across jurisdictions within the Washington, DC. metropolitan area. However, in contrast to our results, they find that higher sales tax rates reduce employment growth.

[22] Parks may also attract customers to retailers and consumer-services establishments located nearby.

crime rate (at the 1% level). The finding that crime repels total employment combined with the finding that crime has a positive effect on manufacturing employment suggests that high crime areas may attract manufacturing because land prices are lower in these areas. In comparison to other industries, the location of manufacturing may be more sensitive to the price of land, because manufacturing sites require large amounts of land. In addition, manufacturing may be less affected by crime, because fencing and other private protection measures may be more effective.

In summary, based upon the results in Tables 2 and 3, it can be concluded that tax incentives, highway improvement expenditures and crime affect the spatial distribution of employment within the Atlanta region. These conclusions, however, are based on the assumption that changes in employment share are caused by changes in our set of government interventions and crime, and that changes in employment share do not cause either the interventions or crime. Another advantage of our use of panel data, besides those listed in Section 5, is that this assumption can be tested by conducting standard causality tests.

Two different causality tests were conducted—the classic Granger test and the Geweke, Meese and Dent (GMD) test based on the Simms representation (Hamilton [15, p. 305]). In the Granger test, the x variable is regressed on lags of both the x variable and the y variable. If the coefficient on the lagged value of y is significant, then y is said to Granger-cause x, and the interpretation of our models, that x causes y, may be in doubt. In the GMD test, which is a modification of the Granger test, y is regressed on its lagged value and on past, present, and future values of x. If the coefficient on the future value of x is significant, then future x values are related to the current y value and y is said to Granger-cause x, which would also cast doubt on our interpretation of our models. If the assumptions necessary for the Granger test to be valid are met, the Granger and GMD tests should yield similar conclusions. Hamilton [15] suggests that the Granger form of the test is the best approach. When the Granger and GMD tests coincide, the results are likely correct and other specification issues are not causing bias in the test.

Both the Granger and GMD tests were conducted for each of our y variables (the change in total private employment share and the change in manufacturing employment share). The x variables tested were each of the government intervention variables and the crime rate. In the Granger tests, one year lags of x and y were used, while in the GMD tests, one year lags of x and y were used along with a one-year lead of x. When testing a given x, the other variables of models (1) and (3) were alternatively employed as control variables in the test specification.

The null hypothesis tested here by the Granger and GMD test is that the dependent variable (change in share of total employment or change in share of manufacturing employment) does not Granger-cause the policy or crime variable. Acceptance of the null hypothesis indicates that there is no evidence to suggest that our results are due to reverse causality: employment growth causes changes in government policy or crime. For the change in total private employment share and the variables in model (1) we accept the null hypothesis at conventional levels in all cases. There is therefore no evidence that the change in employment share Granger-causes any of the x variables in the specification: the log of highway improvement expenditures, the percentage of the rail station impact area falling within the tract, the housing enterprise zone indicator, the commercial–industrial

enterprise zone indicator, and the job tax credit zone indicator.[23] The GMD tests yielded identical results. Using the variables in model (3) as control variables, we again accept the null hypothesis and conclude there is no evidence that employment share Granger-causes crime or any of the policy variables. As before, the GMD tests yielded identical results.

All of the above causality tests were replicated using the change in manufacturing employment share as the *y* variable. With one exception, the null hypothesis is accepted. The exception is that manufacturing employment share is found to Granger-cause the rail station variable. For the set of control variables defined by both models (1) and (3) and using both the Granger and GMD tests, the results suggest that greater growth in manufacturing share causes a rail station. However, the location of a rail station is commenced years in advance of its opening and our causality tests have relatively short time horizons. Hence, the increase in manufacturing share that precedes the opening of a station may be an anticipatory effect. As noted earlier, a number of studies have found that the opening of a rail station increases neighborhood crime, which is expected to lower land prices and thus attract manufacturing employment.

7. Conclusions

A number of arguments have been made in support of public policies that would have as their goal the redistribution of employment within urban areas in favor of depressed neighborhoods within central cities. There is, therefore, considerable interest in whether government actions can alter the share of regional employment found within targeted neighborhoods. We find strong evidence that this is the case for the selected actions that we consider. Neighborhood-based tax incentive programs that provide property tax abatements and job tax credits against the state income tax are both found to affect the neighborhood's share of regional employment. Making road improvements within the neighborhood also is found to increase employment share. Besides offering tax incentives and making road improvements, our results also suggest that local governments can increase a neighborhood's employment share by reducing crime within the jurisdiction within which the neighborhood is located. Crime deterrence may therefore represent an effective economic development strategy.

The findings in this paper are stronger than those commonly reported in the literature that has dealt with governments' effect on employment location. However, to our knowledge, our study is the first to use a panel database at the neighborhood level. Neighborhoods within a metropolitan area are inherently closer substitutes than neighborhoods in different

[23] The results for highway improvement expenditures should be interpreted with care. The causality tests indicate that past changes in employment share do not cause road improvements. However, there remains the possibility that improvements are made in anticipation of future changes in employment share. If our specification omits a variable that determines employment growth and is predictable by policy makers, this could result in the positive relationship, but would not imply a causal relationship. Highways are different than the other policy variables in that plans are very forward looking, while enterprise zones, for example, are typically determined through backward looking processes.

regions or even neighborhoods within the same state. Hence, it is not surprising that we find stronger effects.

Regarding future research, there is an obvious need for further analyses of the effects of government policies on the intraurban spatial distribution of employment. The construction of employment panels of submetropolitan areas like the one used here for other metropolitan areas can be done using employment data from state unemployment insurance records, but this will not be an easy task. Nevertheless, absent social experiments, the use of panel data is the best approach toward dealing with non-random site selection. Another area of future research that is suggested by our results is that having found that there may be benefits from government's attempts to alter the spatial distribution of employment, there are now the issues of the magnitude and distribution of these benefits and the accompanying costs. In previous work where benefits are found to be small, the issue of benefit-cost calculations is moot, since the policy implication is that the program be stopped. Finally, transit-oriented development is increasingly emphasized as a strategy for reducing automobile dependence and the negative externalities that this dependence causes within urban areas. However, this development has been slow to occur in Atlanta and in other metropolitan areas that have rail systems of relatively recent vintage (Bollinger and Ihlanfeldt [7]). Achieving greater transit-oriented development may be possible if both the attractive and repellent forces of rail stations on employment can be identified in future studies.[24]

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[24] Another area for future research, suggested by a referee, is to use employment panels of submetropolitan areas to study the emergence, development and specialization of intrametropolitan employment centers. In the absence of panel data, research on employment centers has relied on a series of snapshots of intrametropolitan structure rather than a systematic look at the evolution of the structure. The importance of studying the latter evolution is underscored by recent findings indicating that there are a set of employment centers that account for between 20 and 50 percent of intrametropolitan employment (see Anas et al. [1] for a survey; Bogart and Ferry [8] or McMillen [23] for more recent studies).

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