The Impact of State Enterprise Zones on Urban Manufacturing Establishments

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Abstract

Since the early 1980s, the vast majority of states have implemented enterprise zones. This paper analyzes urban zones in six states, examining the factors that states use to choose zone locations and the subsequent effect of the zones on business activity and employment. The source of outcome data is the U.S. Bureau of Census' longitudinal research database (LRD), which tracks manufacturing establishments over time. Matched sample and geographic comparison groups are created to measure the impact of zone policy on employment, establishment, shipment, payroll, and capital spending outcomes. Consistent with previous findings, the difference-in-difference estimates indicate that zones have little effect, on average. However, by exploiting the establishment-level data to examine gross as well as net changes, the analysis finds that zones have a positive effect on the outcomes of new establishments and a negative effect on the outcomes of previously existing establishments.

INTRODUCTION

Since the early 1980s, the vast majority of states have implemented enterprise zones programs that target various economic development incentives toward specific blighted areas. Although the programs vary considerably from state to state, all provide business incentives to promote private investment and economic revitalization. To measure the influence of zone incentives on investment decisions, it is useful to examine gross flows—how business births, deaths, expansions, and contractions contribute to net changes. To do this, longitudinally linked manufacturing establishment data from the U. S. Census Bureau are used. They allow us to reconcile the positive claims from many surveys of zone stakeholders and the negative findings from the majority of impact evaluations. Although enterprise zone incentives affect job creation positively, they also increase job destruction, leading to a negligible or negative impact, on average.

This study investigates manufacturing establishment outcomes between 1984 and 1993 in the urban enterprise zone programs of six states: California, Florida, New Jersey, New York, Pennsylvania, and Virginia. Although the specifics of the six programs differ, all offer some type of capital subsidies and in-kind benefits, such as technical assistance and infrastructure improvements. With the exception of Penn-

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sylvania, which offers a broadly defined set of business incentives, all also offer explicit labor subsidies. The capital subsidies come with few restrictions in the six states, but of the states offering labor incentives, all except for California require the businesses to hire qualified zone residents or disadvantaged workers in order to receive benefits.[1] Because of the similarities in the broad categories of incentives offered in these six states, we analyze the programs together. Moreover, in a study of programs of a similar set of states, analysis of separate program features failed to yield any additional explanatory power (Bondonio and Engberg, 2000).

The empirical literature on zones is mixed.[2] A number of studies based on interviews and surveys of zone administrators and businesspeople have shown some success with job creation in enterprise zones (Elling and Sheldon, 1991; Erickson and Friedman, 1990a, b; GAO, 1988; HUD, 1986; Rubin, 1990). However, response bias is always a key concern with surveys and interviews (Bartik, 1991; Blair, 1995).[3] In addition, the use of surveys makes it difficult to establish the counterfactual: What would the outcome have been had there been no intervention?

Studies using statistical methods to create appropriate comparison groups have found more mixed results. Papke (1993, 1994), using annual time series data from zone and non-zone Indiana cities, found some evidence of a positive effect of the Indiana enterprise zone program on unemployment and inventories. Extending her time series, Papke (2000) concludes that Indiana's enterprise zone inventory tax credit may have led businesses to invest in inventory rather than in more productive machinery and equipment. Boarnet and Bogart (1996), using a method similar to Papke's, found no evidence that the New Jersey enterprise zone program increased economic activity in the designated cities. Dowall (1996) analyzed two California programs using shift-share analysis and also found little effect. In a national study of cities with populations of less than 50,000, Engberg and Greenbaum (1999a) found that enterprise zone policies had a small effect in moderately distressed cities but not in severely blighted cities. Examining urban enterprise zones in three states, Engberg and Greenbaum (1999b) found that zones did have some positive effects on home ownership and occupancy rate, but that the zones also had some negative effects on labor market outcomes. Greenbaum and Engberg (2000) found that zones have, at best, no effect on housing market, income, or employment outcomes in the urban areas of six states. Bondonio and Engberg (2000) found that zones have no effect on employment outcomes in five states, even when the monetary generosity of the incentives was taken into account. Using similar manufacturing data from the Census Bureau, Peters and Fisher (2002) found that enterprise zone incentives in 13 states create few new jobs and that the jobs created come at a high cost per job.

A serious shortcoming of many of the recent empirical studies is the reliance on net changes, which masks many of the continual changes that occur in the economy. By focusing on gross flows, we draw on the important work by Davis, Haltiwanger, and Schuh (1996), Dunne, Roberts, and Samuelson (1988, 1989a, b), and Eberts and Stone (1992), which all demonstrate that small changes in macro-level employment are composed of dramatic shifts in both job creation and job destruction rates at the firm, industry, and regional levels. The use of gross flow analysis is an important step forward in the evaluation of zone outcomes, although the analy-

[1] Greenbaum (1998) provides more detailed information about these programs, and information about how the programs compare to other states' programs can be found in Greenbaum and Engberg (2002).
[2] Wilder and Rubin (1996) and Peters and Fisher (2002) provide excellent reviews.
[3] When surveyed, businesspeople are loath to appear to be opposed to tax incentives, and zone administrators have incentives to overstate the contribution of the programs.

sis requires some modification to the models that have been used to measure net changes. We build on Papke's (1993, 1994) and Boarnet and Bogart's (1996) models, which allow enterprise zones to affect the level of employment. Because we are interested in gross flows, we specify a model that allows zones to affect employment growth rates rather than levels. We examine which flows, if any, zone programs affect, to gain a deeper understanding of how a program might change employment levels.

Employment-level and net-growth-rate models are both restricted versions of a general model in which zones have a time-varying effect on employment. A simple levels model allows zones to have an effect only in the year of designation, thereby creating a jump in the level. A simple growth-rate model, on the other hand, implies a permanent change in the growth rate leading to little immediate effect, but an increasing effect on the level as time passes. A slightly more general model allows zones to have an effect on the growth rate that varies over time. A jump in the level will appear as a dramatic increase in the growth rate immediately following designation, followed by a return to the original growth rate.

When estimating the effect of the zone incentives, care must be taken to distinguish outcomes that result from prior economic conditions in the zone areas from outcomes attributable to the zone policy. To help identify these outcomes, we create matched sample comparison groups of areas that have similar economic conditions but are not granted enterprise zones. We estimate the zone impact using a difference-in-difference procedure.

EXPECTED ZONE IMPACT

Effective incentives will reduce zone operating costs enough to induce additional investment in the zones, which should lead to both an increase in business formation and a reduction in business exits in zones. Often overlooked, retention may be as important to communities as attraction due to agglomeration economies and the associated costs to local residents and remaining businesses of the loss of a business (Voith, 1996). Outcome measures—such as growth in the number of establishments, sales, employment, payroll, and capital expenditures—would all be expected to be greater in successful zones than in comparable areas that did not receive zone incentives.

The effect on the growth rates of the outcome measures is unlikely to be uniform, however, because of the influence incentives have in distorting market choices. Zone incentives are likely to affect the prices and utilization of business' factor inputs through two paths. First, tax incentives and subsidies change the relative prices of inputs. Businesses respond by substituting toward the factors more heavily subsidized. Employment levels and wage changes are sensitive to the mix of labor versus capital incentives and to the elasticity of the supply of labor. Labor supply is generally found to be rather inelastic; thus, both labor and capital subsidies should have a larger impact on wages than on employment levels (Papke, 1993).

Second, factor input use is affected as the incentives are capitalized. Capitalization of zone incentives causes the prices for immobile factors of production, such as land, to rise. If factor markets are efficient, factor prices in a zone will be bid up until the rate of profit inside the zone is equal to that just outside of the zone. Even if factor prices increase enough to keep profits constant, the lower taxes and higher land prices should attract businesses that are more capital-intensive. Thus, land users will likely substitute capital for land, and the amount of capital invested in the zone should increase (Bartik, 1991).

Zone incentives are also likely to have indirect effects on the non-subsidized businesses. If establishments outside zone boundaries are put at a competitive disadvantage, this damage might outweigh any good the policy does inside of the zone. On the other hand, growth inside the zone might be a complement rather than a substitute to growth elsewhere. Indeed, businesses in the suburbs are likely to benefit by a strong urban core and the cessation of the spread of urban blight (Voith, 1996).

These theoretical questions can be better assessed by disaggregating net establishment, employment, shipment, payroll, and capital spending outcomes into the component gross flows. Net changes, for example, can mask investments made by new establishments that are offset by disinvestments made by establishments that move or close. Furthermore, by comparing the effect on gross flows inside the zones with the effects on areas just outside the zone, we can assess whether or not any changes in the gross flows due to zones are merely reallocations of nearby resources.

DATA

Annual measures of each are required to measure gross changes in the number of establishments, employment, payroll, and capital spending. The richest data have been collected for the manufacturing sector; the U.S. Bureau of Census' longitudinal research database (LRD) is an ideal data set. Although the analysis is restricted to just one sector of the economy, manufacturing jobs are often what economic development officials have in mind when they talk of creating jobs. These jobs generally pay well and have many forward and backward linkages.

Of course, changes in other sectors could either offset or add to local changes in the manufacturing sector. If labor markets are very geographically specific and very tight, the increased employment in one sector would necessarily be offset by local employment decreases in other sectors. On the other hand, in a looser labor market, increased manufacturing activity could be supplemented by local support activity in other sectors and by increased local demand for retail goods and services due to increased earnings of manufacturing employees. Without geo-coded establishment-level panel data from all sectors of the economy, it is impossible to know whether local changes in non-manufacturing sectors offset or add to the changes in the manufacturing sector.

The focus of this study is manufacturing outcomes for the years 1984 through 1993. To help identify comparison areas that are similar to the designated zone areas, we use a version of the Census Bureau's County Business Patterns and Decennial Census data. The enterprise zone data come from various primary and secondary sources as described below.

We analyze the effect of enterprise zones at the U.S. Postal ZIP code level. In many cases, enterprise zone boundaries do not correspond with common geographic entities such as census tracts, ZIP codes, municipalities, or counties. The choice of ZIP codes represents a compromise based on the ability to identify ZIP codes that overlap enterprise zones, and the availability of ZIP code information on business establishments. A ZIP code is categorized as a zone area if any portion of the ZIP code overlaps with a designated zone. Therefore, the analysis captures the effect of zone policies on the area immediately surrounding the zone, as well as on the area inside the official boundaries of the zone. Although zone size varies somewhat from state to state, the average zone in this data set is comprised of approximately 3.24 ZIP codes.

Business Data

The LRD, which contains data on U.S. manufacturing plants with five or more employees, was developed by the Census Bureau to better investigate changes in the U.S. manufacturing sector over time. The LRD combines the quinquennial Census of Manufacturers (CM) and the Annual Survey of Manufacturers (ASM). Because each plant location is assigned a unique identification number, each manufacturing establishment can be tracked over time. The data available for each establishment include: location, output quantities, and detailed information on the factors of production, such as the levels of capital, labor, energy, and materials used as inputs.

ASM panels are selected from the CM universe 2 years after each CM. The largest plants, those with at least 250 employees, are included in the ASM panel with certainty. For the smaller establishments, the probability of inclusion into a panel rises with plant size. For plants smaller than 250 employees, births and deaths can only be measured within an ASM sample. Therefore, analysis was limited to two ASM panels: 1984–1988 and 1989–1993.[4]

An unofficial Census Bureau tabulation of County Business Pattern data at the ZIP code level was used to measure annual net changes in business activity. The ZIP Code Business Patterns (ZCBP) tabulation includes annual counts of establishments categorized by U.S. Postal ZIP code, cross-tabulated by four-digit SIC industry classification, and employment class size.[5] We aggregate the employment data for the different size classes to the two-digit industry level for each year. By fitting a Weibull distribution to the establishment employment size counts, an implied average employment by size class is calculated and used to estimate the employment in each ZIP code each year for each two-digit industry. To estimate the average ZIP employment, the size class averages are multiplied by the number of establishments in each size class in each ZIP and summed over the size classes. Although this unofficial ZIP code tabulation has not been subjected to the Census Bureau's high-quality control checks, it has proven to be very informative in previous research (Bondonio and Engberg, 2000; Greenbaum and Engberg, 2000).

Enterprise Zone Data

The enterprise zone data come from a variety of sources. Information about which municipalities have zones, the designation dates, and the program features were collected from the coordinating agencies of the respective state. Detailed descriptions of program goals, incentives, eligibility criteria for participating businesses and zone designation criteria were compiled from various documents provided by each state's program office and from U.S. Department of Housing and Urban Development documents (HUD, 1992).

Many state enterprise zones are in rural communities, but we limit the focus of the paper to the large metropolitan areas of six states. These six states all started their zone programs early enough that outcome data could be collected to evaluate their performance. The sample was limited to states that maintain geographically focused programs by not designating excessively large numbers of zones. Louisiana alone has almost 2,000 zones. The decision to limit the focus to only urban zones represents interest in exploring the influence of enterprise zones on business out-

[4] See the technical appendix in Davis, Haltiwanger, and Schuh (1996) for more information about the LRD data.

[5] Official Census Bureau ZCBP data is unavailable for years prior to 1994.

comes. Rural zones are often very large and typically do not affect intra-city location decisions.

The source of zone location information varies from state to state. For example, the director of the Virginia program provided a list of census tracts covered by each zone. The corresponding ZIP codes were obtained using MABLE/Geocorr, a web-based geographical correspondence engine that determines the degree of overlap between two spatial units.[6] A few states, such as Florida, provided census block group maps with each zone outlined (State of Florida, 1993), but most states have no central source for zone maps. Maps were obtained by contacting each of the local zone administrators. For Florida and Pennsylvania zones, ZIP codes were obtained by matching their boundaries from paper maps. For the other four states, a representative at each zone provided either a list of zone ZIP codes or a map demarcating zone boundaries.

Housing, demographic, income, and unemployment information come from the 1980 Decennial Census STF3a files. These data were allocated to ZIP code using allocation factors from MABLE/Geocorr.

DESCRIPTIVE STATISTICS

The analysis is limited to the ZIP codes in the largest metropolitan statistical areas (MSAs) of California, Florida, New Jersey, New York, Pennsylvania, and Virginia. By focusing on large MSAs, we can better examine the effect of enterprise zone policy in areas that face similar economic challenges. The ZIP codes included are all part of three-digit ZIP codes whose population of at least 400,000 is more than half inside an MSA. Table 1 lists the 28 MSA that meet the population criterion, their population, the number of ZIP codes overlapping the MSA, and the subset of ZIP codes in the MSA that contain an enterprise zone. Approximately 10 percent of the urban ZIP codes either entirely or partially contain an enterprise zone, and two of the MSA have no zone.

States have limited resources to devote to economic development, and enterprise zones represent a way to direct those resources to the most distressed areas. The six states phased-in their urban zones throughout the 1980s and early 1990s (Table 2). Pennsylvania designated its earliest urban zones in 1983; Florida was the only state to designate all of its zones in the same year, 1986. Table 3 presents the mean of the socioeconomic and housing measures by future enterprise zone status.[7] Consistent with their designation criteria, the states placed their programs in the most distressed areas. Enterprise zone ZIP codes were more densely populated than the comparison ZIP codes in 1980. Zones also had lower per capita income, higher poverty and unemployment, lower high school graduation rates, and higher percentages of minority residents than non-zone ZIP codes. In addition, non-zone ZIP codes had higher home values, rental prices, and owner occupancy rates than zones.

Because most of the zone incentives are targeted at businesses rather than residents, many of the zones were placed in ZIP codes well represented in the manufacturing industry. As Table 4 shows, establishments in 1982 in zone ZIP codes had significantly greater employment, a greater percentage of production workers, higher production worker wages, greater value of shipments per employee, lower

[6] <http://plue.sedac.ciesin.org/plue/geocorr/>

[7] The GDP implicit price deflator (base year of 1992) is used to inflate and deflate all monetary values (Council of Economic Advisors [U.S.], 1997).

Table 1. Metropolitan Statistical Areas.

MSA	Population	ZIP	EZ[a]
Albany–Schenectady–Troy, NY	874,304	78	1
Allentown–Bethlehem–Easton, PA	686,688	56	7
Bakersfield, CA	543,477	23	3
Buffalo–Niagara Falls, NY	1,189,288	78	3
Fresno, CA	667,490	43	6
Harrisburg–Lebanon–Carlisle, PA	587,986	49	8
Jacksonville, FL	906,727	42	7
Lakeland–Winter Haven, FL	405,382	21	2
Lancaster, PA	422,822	40	5
Los Angeles–Riverside–Orange County, CA	14,531,529	469	57
Miami-Fort Lauderdale, FL	3,192,582	119	30
New York, Northern NJ, LI, NY–NJ–CT	17,125,398	807	32
Norfolk–Virginia Beach–Newport News, VA	1,396,107	64	16
Orlando, FL	1,072,748	46	5
Philadelphia–Wilmington–Atlantic City, PA–MD–NJ	5,457,399	329	36
Pittsburgh–Beaver Valley, PA	2,242,798	201	25
Richmond-Petersburg, VA	865,640	59	10
Rochester, NY	1,002,410	91	0
Sacramento, CA	1,481,102	93	14
San Diego, CA	2,498,016	94	7
San Francisco–Oakland–San Jose, CA	6,253,311	263	16
Scranton–Wilkes-Barre–Hazleton, PA	734,175	77	14
Stockton-Lodi, CA	480,628	18	0
Syracuse, NY	659,864	66	6
Tampa–St. Petersburg–Clearwater, FL	2,067,959	83	10
Washington, DC–MD–VA	3,923,574	198	5
West Palm Beach–Boca Raton, FL	863,518	31	6
York, PA	417,848	44	4

[a] Number of ZIP codes that contain an enterprise zone.

Table 2. Zone starting dates by state urban enterprise zones.

State	First	Mean	S.D.	Number of ZIPs
California	1986	1988.3	2.5	103
Florida	1986	1986.0	0.0	60
New Jersey	1984	1984.7	0.5	31
New York	1987	1987.3	0.4	23
Pennsylvania	1983	1986.8	3.4	87
Virginia	1984	1987.6	4.3	28

cost of contract work, lower rents, greater energy intensity, and more capital intensive production than the did establishments in non-zone ZIP codes. Based on the 1982 levels, zone ZIP codes do not appear to be particularly unattractive places to do business. However, the data represent only the manufacturing sector, which has been steadily losing employees, and levels can be misleading; changes are also

Table 3. Means of 1980 census variables by future zone status.

Variable	Overall	Non-zone	Zone	Difference
Population density	2021.0	1892.2	3102.4	-1210.2***
(People per square km)	(4101.2)	(4117.9)	(3794.2)	
Per capita income	12863.1	13051.5	11281.5	1770.0***
(1992 dollars)	(3575.3)	(3644.3)	(2413.2)	
Poverty rate	0.116	0.111	0.159	-0.048***
	(0.076)	(0.074)	(0.075)	
Unemployment rate	0.064	0.063	0.077	-0.014***
	(0.028)	(0.028)	(0.025)	
High school graduates	0.518	0.523	0.468	0.055***
(Population 25 and older)	(0.092)	(0.092)	(0.076)	
Percentage black	0.124	0.117	0.188	-0.071***
	(0.165)	(0.163)	(0.172)	
Percentage Hispanic	0.099	0.093	0.150	-0.057***
	(0.122)	(0.116)	(0.151)	
Housing values	116317.7	118553.7	97638.4	20915.3***
(1992 dollars)	(50548.2)	(51865.9)	(32257.2)	
Rent	465.4	471.8	411.3	60.5***
(1992 dollars)	(103.0)	(104.5)	(69.0)	
Owner occupancy	0.461	0.467	0.409	0.058***
	(0.188)	(0.191)	(0.151)	
Number of ZIP codes	3242	2897	345	

* p-value ≤ 0.1 ** p-value ≤ 0.05 *** p-value ≤ 0.01
Standard deviations are in parentheses.

important. Table 5 indicates that both growth in employment and the number of manufacturing establishments were slower in the enterprise zones than in the comparison ZIP codes in the 2-year period preceding zone designation, although the difference is significant only for changes in establishments.[8]

Table 6 shows that over the decade of the 1980s, the zone ZIP codes continued to under-perform the non-zone areas. Among the census variables, population density grew more rapidly in the zone ZIP code areas. In all of the socioeconomic and housing measures, the non-zone ZIP code areas performed better than the zone ZIP code areas. For example, poverty and unemployment rates dropped in the non-zone ZIP codes, and they rose or remained constant in the zone ZIP codes. No clear picture emerges from the business data. In the zone ZIP code areas, employment density, percentage of production workers, and new machinery expenditures fell less, while shipments and value added grew more rapidly than in the non-zone ZIP code areas. However, in the non-zone ZIP code areas, employment per establishment, production worker wages, new building expenditures, and energy intensity fell less than in the zone ZIP codes. Also, the cost of contract work fell in the non-zone ZIP codes and increased in the zone ZIP code area, and building rents and capital intensity both grew more rapidly in the zone ZIP code areas.

[8] The number of observations was slightly less for these two variables because of differences in data availability between the ZCBP and the LRD. Regressions are estimated using only ZIP codes that are in both data sets.

Table 4. Means of 1982 business variables by future zone status.[a]

Variable	Overall	Non-zone	Zone	Difference
Employment density	0.145	0.162	0.008	0.154
(Thousands of workers per km2)	(5.817)	(6.154)	(0.047)	
Employment	54.3	50.9	82.4	–31.5***
(Workers per establishment)	(115.2)	(108.0)	(161.3)	
Production workers	0.703	0.702	0.713	–0.011*
(As percentage of employment)	(0.149)	(0.153)	(0.107)	
Prod. worker wages	11.2	11.1	11.9	–0.8***
(Hourly wage in 1992 dollars)	(6.5)	(6.7)	(3.7)	
Value of shipments	123.6	121.1	143.9	–22.8**
(Shipments per employee in thousands of 1992 dollars)	(138.5)	(135.6)	(159.7)	
Value added	58.5	58.2	60.9	–2.7
(Value added per employee in thousands of 1992 dollars)	(47.4)	(48.6)	(36.1)	
Cost of contract work	3.274	3.340	2.723	0.617*
(Cost per employee in 1992 dollars)	(9.688)	(10.082)	(5.335)	
New building expend.	0.886	0.830	1.355	–0.525
(Expenditures per employee in thousands of 1992 dollars)	(3.924)	(3.619)	(5.871)	
New machine expend.	3.276	3.234	3.627	–0.392
(Expenditures per employee in thousands of 1992 dollars)	(9.473)	(9.855)	(5.296)	
Building rents	0.246	0.252	0.198	0.054***
(Thousands of 1992 dollars)	(0.441)	(0.461)	(0.200)	
Energy intensity	0.022	0.022	0.026	–0.004**
	(0.030)	(0.030)	(0.031)	
Capital intensity	37.3	36.185	46.651	–10.466***
(1992 dollars)	(59.1)	(58.535)	(63.310)	
Number of ZIP codes	3242	2897	345	

* p-value ≤ 0.1 ** p-value ≤ 0.05 *** p-value ≤ 0.01
Standard deviations are in parentheses.
[a] Measured using Census of Manufactures data.

ECONOMETRIC ANALYSIS

The descriptive statistics are not adequate to assess the effects of zone designation on business activity. For the treatment group of ZIP codes that get enterprise zones, we employ a difference-in-difference approach, measuring the difference in the growth rates between a period before and after zone designation. The difference in growth rates is measured for the same periods for a comparison group of ZIP codes that never gets zones, and then the difference between the two differences is measured. If the enterprise zone incentives are effective, the pre- to post-designation change in growth rate in the enterprise zones should be larger than the corresponding changes in the comparison ZIP code area. Meaningful difference-in-difference estimates depend on the construction of proper comparison groups.

Table 5. Means of establishment and employment change[a] by future zone status.

Variable	Overall	Non-zone	Zone	Difference
Employment change	0.044	0.051	–0.018	0.069
	(0.560)	(0.757)	(0.383)	
Establishment change	0.076	0.093	0.017	0.076***
	(0.291)	(0.355)	(0.208)	
Number of ZIP codes	2771	2458	313	

* p-value ≤ 0.1 ** p-value ≤ 0.05 *** p-value ≤ 0.01
Standard deviations are in parentheses.
[a] Changes are measured using ZCBP data over the two-year period just prior to the state's first zone for all states except for Pennsylvania. Pennsylvania designated its first zone in 1983, and the first year of the available ZCBP data is 1981. Therefore, the 2-year change variables for Pennsylvania were calculated between 1981 and 1983 rather than between 1980 and 1982.

Table 6. Demographic and economic changes by future zone status.

Variable	Overall	Non–zone	Zone	Difference
Census Variable Changes: 1980–1990				
Population density	93.977	86.195	159.317	–73.121
Per capita income	5691.564	5852.201	4343.614	1508.587
Poverty rate	–0.008	–0.009	0.002	–0.011
Unemployment rate	–0.005	–0.006	0.000	–0.006
High school graduates	0.003	0.002	0.004	–0.002
Percentage black	0.003	0.002	0.008	–0.006
Percentage Hispanic	0.019	0.018	0.025	–0.007
Housing values	62382.570	64517.450	44560.950	19956.500
Rent	91.934	94.929	66.829	28.100
Owner occupancy	0.032	0.034	0.015	0.019
Business Variable Changes: 1982–1992				
Employment density	–0.114	–0.127	–0.002	–0.125
Employment	–15.609	–14.334	–26.317	11.983
Production workers	–0.069	–0.072	–0.051	–0.021
Prod. worker wages	–0.267	–0.191	–0.899	0.708
Value of shipments	2.231	1.868	5.255	–3.386
Value added	6.033	5.468	10.748	–5.280
Cost of contract work	–0.058	–0.090	0.212	–0.302
New building expend.	–0.275	–0.216	–0.773	0.557
New machine expend.	–0.370	–0.387	–0.229	–0.158
Building rents	0.777	0.790	0.669	0.122
Energy intensity	–0.004	–0.003	–0.006	0.003
Capital intensity	6.529	6.693	5.159	1.534

Definition of Comparison Groups

An appropriate set of comparison area ZIP codes should have initial characteristics that make them likely candidates to be designated as an enterprise zone (Friedlander, Greenberg, and Robins, 1997). The decision to limit the analysis to ZIP codes in urban areas was the first step in creating suitable comparison ZIP code areas. Using the set of urban ZIP codes, two different types of comparison ZIP code groups were established. The first method draws upon Rubin's work (1973, 1979) to select a set of matched ZIP codes chosen by initial characteristics. The second method selects comparison groups based only on geography—the outcomes in the zone ZIP code areas are compared with outcomes in ZIP code areas within and beyond a 5-mile radius of the zone.

To help ascertain the attributes of an area important to zone designation, we estimate a model of the probability that a ZIP code will be designated a zone as a function of the pre-designation characteristics (X_i). Later, we use these predicted probabilities to generate an appropriate set of comparison ZIP codes.

$$PZ_i = \Pr(EZ = 1 | X_i) = probit(X_i\beta) \qquad \text{(Eq. 1)}$$

Table 7 presents the estimated coefficients from stepwise probit regressions in which the probability that a ZIP code will be designated a zone is modeled as a function of the household and business characteristics and business growth in the ZIP early in the decade. Stepwise regression was used to identify variables included in the regression. The diversity of the variables loaded in the six states, as well as the differences in the coefficients, are an indication that states use different characteristics for designation. Among the census variables, all states targeted ZIP codes more densely populated and exhibiting economic distress such as having lower housing values or rental values. Among the business variables, states tended to target areas with fewer workers per square kilometer, but also places with more workers per establishment.

The estimated probability of zone designation in equation 1 can be thought of as a propensity score (Rosenbaum and Rubin, 1983, 1984). As Dehejia and Wahba (1999) showed, the propensity score can successfully be used to help create a matched sample of comparison ZIP codes. For each state, every zone ZIP code was matched with the non-zone ZIP code that has the smallest squared difference in propensity scores. The same non-zone ZIP code can be used as a comparison ZIP code for multiple-zone ZIP codes.

Before selecting the matched sample of ZIP codes, a number of ZIP codes were eliminated. Dehejia and Wahba (1999) found that limiting the estimation sample to only observations that have suitable matches likely improves impact estimates. The most prosperous ZIP codes are unlikely to ever designate zones; therefore, they make poor comparisons. For the least prosperous ZIP code areas, it is unlikely that there will be many similarly depressed ZIP code areas that were not designated zones. When there are no good comparison ZIP codes for the most distressed zone ZIP codes, the same, most distressed, non-zone ZIP code in that state would be matched to all of the most distressed zone ZIP codes. For most of those zone ZIP codes, that match would not be very good.

The predicted designation probabilities calculated from each state's own location equation was used to eliminate the most well off and most distressed ZIP codes. To eliminate the most prosperous ZIP codes, all ZIP codes were eliminated that have designation probabilities lower than the first percentile of the predicted designation probability among the zone ZIP codes. To eliminate the least prosperous ZIP codes,

Table 7. Probability of zone designation stepwise probit regressions estimates.

Variable	CA	FL	NJ	NY	PA	VA
Census Variables in 1980						
Population density	0.192***	0.202***	0.348***	0.172***	0.310***	0.394***
	(0.039)	(0.056)	(0.113)	(0.055)	(0.071)	(0.139)
Poverty rate	5.249***					
	(1.915)					
Unemployment rate	–6.458					
	(4.183)					
High school graduates	–2.906**					
	(1.184)					
Percent Hispanic		3.938***				
		(0.956)				
Housing values	–0.508**		–0.712*			–2.475***
	(0.244)		(0.426)			(0.671)
Rent	–0.772	–1.737***	–2.409***	–1.584***	–1.205**	
	(0.536)	(0.589)	(0.862)	(0.534)	(0.503)	
Owner occupancy	1.210**	1.260*	1.738**		–1.522***	
	(0.568)	(0.763)	(0.833)		(0.570)	
Business Variables in 1982						
Employment density			–0.716***		–0.423***	–0.440*
			(0.169)		(0.102)	(0.268)
Employment	0.185***		1.774***		0.850***	0.903***
	(0.063)		(0.314)		(0.151)	(0.284)
Production workers			3.138**		1.704**	
			(1.437)		(0.870)	
Value added			–0.019**			
			(0.008)			
New building expenditures	0.016*		–0.387**			
	(0.010)		(0.177)			
New machinery expenditures			0.065			
			(0.043)			
Building rents		–1.619***				
		(.628)				
Energy intensity					6.442**	
					(2.665)	
Other Variables						
Establishment change		–1.284***				
		(0.359)				
Constant	8.250**	7.932**	6.000	6.492**	–2.222	18.898
	(3.402)	(3.481)	(4.682)	(3.229)	(3.148)	(6.997)
Log likelihood	–227.2	–109.6	–65.7	–83.3	–129.2	–36.7
N	792	276	450	608	482	142

* p-value ≤ 0.1 ** p-value ≤ 0.05 *** $p \leq 0.01$
Standard errors are in parentheses. The dependent variable is EZ (= 1 if zone, 0 else).
Variables not "loaded": per capita income, percent black, production worker wages, value of shipments, cost of contract work, capital intensity.

all of the ZIP codes were eliminated that have designation probabilities greater than the 99th percentile of the propensity scores among the non-zone ZIP codes. The remaining ZIP codes are the treatment zone ZIP codes that will be used and the pool of potential non-zone ZIP codes from which the matched comparison ZIP codes are chosen.

The propensity score is particularly convenient because it summarizes all of the covariates into a single number, which makes it easier to examine the "comparability" of the comparison group to the treatment group (Dehejia and Wahba, 1999). Table 8 shows the propensity score means for the treatment zone ZIP codes and for the matched sample of non-zone ZIP codes. For the matched sample, the mean propensity scores in each state are almost identical to those in the treatment group. Based on the propensity scores, the matched sample appears to be a suitable comparison group.

Difference-in-Difference Method

The difference-in-difference analysis is performed on five growth measures: total employment (job creation and retention); total dollar value of shipments (plant output); production worker payroll;[9] expenditures on new buildings and machinery (investments);[10] and number of establishments. The means and standard deviations of the five variables at the ZIP code and establishment levels are listed in Table 9. All of the variables are weighted by the inverse probability of selection into an ASM panel.[11] The dollar figures are measured in 1992 dollars or thousands of 1992 dollars. At the ZIP code level, payroll is measured by the mean hourly production worker wage times the total number of employees.

For employment, shipments, payroll, and capital expenditures, annual growth rates for each ZIP code are calculated. In addition, the LRD data allow those growth rates to be decomposed into changes attributable to four "types" of establishments: births, deaths, expanding establishments, and contracting establishments. Growth is defined

Table 8. Mean predicted zone designation probability matched sample.

	Non-Enterprise Zone			Enterprise Zone		
State	N	Mean	Std. Dev.	N	Mean	Std. Dev.
California	85	0.255	0.141	85	0.255	0.141
Florida	49	0.346	0.162	49	0.344	0.159
New Jersey	22	0.276	0.143	22	0.277	0.144
New York	20	0.072	0.034	20	0.072	0.034
Pennsylvania	67	0.411	0.193	67	0.412	0.195
Virginia	12	0.267	0.100	12	0.271	0.105

[9] Payroll could expand even as employment falls if higher-paying jobs are replacing lower-paying jobs.
[10] Ideally, the capital intensity measure used in the probit regressions would be used to better measure capital intensity relative to labor inputs. Unfortunately, the Census Bureau stopped measuring capital stock after 1989, so only new capital expenditures can be measured.
[11] Using unweighted data (not shown) did not change any of the paper's conclusions.

Table 9. Dependent variable definitions matched sample: 1984-1993.

Variable[a]	Description	Zip Code Mean	Per-Establishment Mean
Employment	Number of employees	5508.154	79.667
		(5011.408)	(202.677)
Shipments	Value of shipments (thousands of 1992 dollars)	779,572.062	12,087.981
		(799,898.312)	(40,899.145)
Payroll	Mean production worker hourly wage times employment (1992 dollars)	67,072.234	11.889[b]
		(69,013.281)	(3.477)
Capital spending	New building and machinery expenditures (thousands of 1992 dollars)	21,527.137	386.441
		(30,669.648)	(1330.974)
Establishments	Number of manufacturing establishments	27.708	1
		(44.939)	(0)

[a] All variables are weighted by the inverse probability of selection into an ASM panel.
[b] The Per-Establishment mean for payroll is the mean production worker wage.
Standard deviations are in parentheses.

$$G^j_{it} = \frac{E^j_{it} - E^j_{it-1}}{\sum_j E^j_{it-1}} \qquad \text{(Eq. 2)}$$

where G^j_{it} is the growth rate for ZIP i in year t. E is the outcome measure, such as employment, and j represents the type of establishment. The numerator is the difference between this year's and last year's employment in a ZIP code for a particular type of establishment, and the denominator is the sum of all employment for all establishments in a ZIP code in the previous year.

The net annual change in employment (or other outcome) can be expressed as the sum of the gross changes of the growth rates due to establishments that have opened, closed, expanded, or contracted during the past year. Births are defined as establishments that have positive employment in the current year but had zero employment in the previous year. Deaths are defined as establishments that have zero employment in the current year but had positive employment in the previous year. Deaths include both shutdowns and moves. An identification number attached to a physical location identifies establishments in the LRD. Therefore, it is conceivable that over time the same "establishment" could be classified as both a death and a birth. Expanding establishments are defined as establishments in the current year that have had stable or positive total employment, shipment, payroll, or capital spending growth since the previous year. Similarly, contracting establishments are defined as establishments that had greater total employment, shipments, payroll, or capital spending in the previous year.

As an example for the employment outcome, consider a hypothetical ZIP code that increased employment from 5500 jobs in 1989 to 6000 jobs in 1990. This change in employment is depicted in Figure 1. The net increase of 500 jobs masks

the fact that jobs are both being created and destroyed in the ZIP code. New establishments, or births, added 700 jobs, and expanding establishments added 300 jobs. This job growth was partially offset by the 400 jobs lost to establishments that closed in 1990 (deaths) and the 100 jobs that contracting establishments shed.

For the counts of the number of establishments of each type, growth is defined similarly:

$$G^j_{it} = \frac{N^j_{it}}{\sum_j N^j_{it-1}} \qquad \text{(Eq. 3)}$$

where G^j_{it} is the growth rate for ZIP code i in year t for establishment type j. N is the number of establishments.[12]

Because all of the establishment classifications are based upon values in the previous year, establishments in the first year of each ASM sample, 1984 and 1989, cannot be classified. Therefore, growth rates are calculated only for years 2 through 5 of each panel. Finally, all of the growth rates are weighted by the lagged values of their denominators. This prevents the smaller ZIP code areas from having inordinate influence.

Table 10 presents all of the ZIP code growth rates broken down by the establishment type (birth, death, expand, contract, and total) and by enterprise zone status.

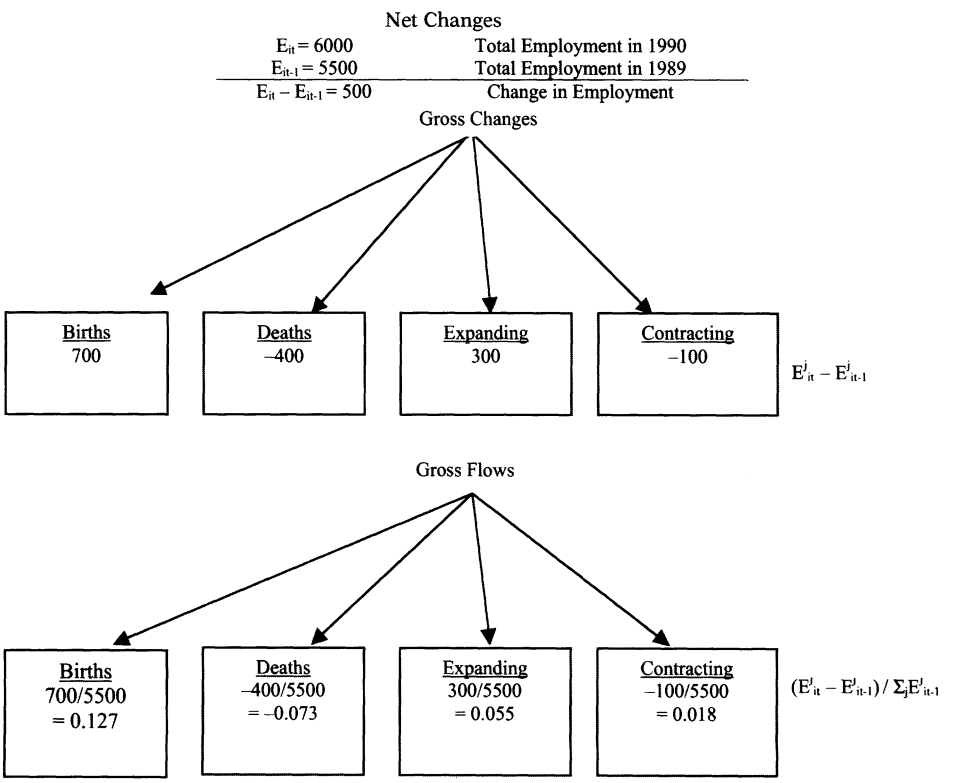

Figure 1. Example of decomposition.

[12] For the counts, the "expand" and "contract" are defined by the changes in employment.

For both the zone ZIP code areas and the matched comparison group, the growth rates are separated into mean pre-zone designation and post-designation growth rates. For the comparison ZIP codes, the designation date is the date of designation of the matched zone ZIP code. For each variable and establishment type, the overall growth rate is also reported.

Each of these four growth rates is an average over zone and comparison areas, before and after the designation date. These rates of employment growth for births can be seen in the Birth column. The birth employment growth rate for the comparison group fell from an average of just under 4 percent a year in years prior to designation to an average of just over 3 percent a year post-designation. For the enterprise zones,

Table 10. Annual growth rates by zone and establishment status matched sample: 1984–1993[f].

Outcome[i]	Zone Status	Designation Status[a]	Establishment Type Birth[b]	Death[c]	Expand[d]	Contract[e]	Total[h]
	Non-EZ	Pre	0.039	–0.034	0.051	–0.080	–0.025
Employment		Post	0.032	–0.043	0.057	–0.100	–0.054
	EZ	Pre	0.022	–0.026	0.074	–0.083	–0.013
		Post	0.031	–0.040	0.059	–0.095	–0.046
	Overall		0.031	–0.038	0.059	–0.093	–0.040
	Non-EZ	Pre	0.037	–0.034	0.076	–0.085	–0.006
Shipments		Post	0.028	–0.034	0.078	–0.088	–0.016
	EZ	Pre	0.025	–0.023	0.105	–0.078	0.030
		Post	0.022	–0.031	0.073	–0.092	–0.028
	Overall		0.027	–0.031	0.080	–0.088	–0.012
	Non-EZ	Pre	0.039	–0.034	0.065	–0.103	–0.034
Payroll		Post	0.026	–0.033	0.078	–0.124	–0.053
	EZ	Pre	0.017	–0.019	0.105	–0.098	0.006
		Post	0.024	–0.037	0.074	–0.122	–0.061
	Overall		0.026	–0.032	0.079	–0.116	–0.043
	Non-EZ	Pre	0.025	–0.024	0.295	–0.341	–0.046
Capital		Post	0.030	–0.019	0.301	–0.373	–0.061
spending	EZ	Pre	0.015	–0.009	0.349	–0.378	–0.023
		Post	0.023	–0.024	0.307	–0.349	–0.043
	Overall		0.024	–0.020	0.309	–0.359	–0.046
	Non-EZ	Pre	0.035	0.064	0.497	0.439	0.972
Establishments[g]		Post	0.038	0.085	0.512	0.408	0.958
	EZ	Pre	0.027	0.060	0.517	0.422	0.967
		Post	0.040	0.088	0.485	0.432	0.957
	Overall		0.037	0.080	0.501	0.423	0.960

[a] Designation status distinguishes between observations prior to and post-zone designation (or the matched ZIP code's designation date for non-zone ZIP codes).
[b] A "birth" is an establishment that has positive employment in the current year, but had zero employment in the previous year.
[c] A "death" is an establishment that has zero employment in the current year, but had positive employment in the previous year.
[d] "Expand" refers to establishments in the current year that have had stable or positive total employment (shipments/payroll/capital spending) growth from the previous year.
[e] "Contract" refers to establishments in the current year that had greater total employment (shipments/payroll/capital spending) in the previous year.
[f] No births, deaths, expanding or contracting establishments are recorded for the first year of an ASM panel (1984 and 1989).
[g] For the establishment counts, "Expand" and "Contract" are based on employment growth.
[h] "Total" refers to average annual growth rates for all establishments. The growth rates of the four types of establishments sum to the Total growth rate.
[i] Means are weighted by lagged employment/shipments/payroll/capital spending/number of establishments.

the average employment growth rate started at a lower pre-designation level (2 percent a year) and then increased to 3 percent a year. Payroll also showed a similar pattern of a post-designation decline in the birth rate for non-zones and a post-designation increase for zones. Shipments had a post-designation decline for both non-zones and zones. For capital spending and establishment counts, both non-zones and zones saw post-designation jumps in the birth rates.

To determine whether any of the pre- to post-designation differences between the zones and non-zones are significant, the following regression model is estimated:

$$G_{it} = \delta_0 + \delta_1 NONEZAFT_{it} + \delta_2 EZPRE_{it} + \delta_3 EZAFT_{it} + \varepsilon_{it} \qquad \text{(Eq. 4)}$$

where G_{it} is the growth rate in ZIP code i in year t, *NONEZAFT* is a dummy variable equal to 1 if the ZIP code is a comparison, non-zone ZIP code after zone designation and 0 otherwise,[13] *EZPRE* is a dummy variable equal to 1 if the ZIP code is a zone ZIP code prior to designation and 0 otherwise, and *EZAFT* is a dummy variable equal to 1 if the ZIP code is a zone ZIP code post-designation and 0 otherwise. Because the omitted category is the non-zone ZIP code areas prior to designation, the coefficient on *NONEZAFT*, δ_1, represents change in the growth rates of comparison ZIP codes post-designation. The change in growth rates of zones post-designation can be calculated ($\delta_3 - \delta_2$). The difference in these two changes is the difference-in-difference estimate.

For the births, deaths, expanding and contracting establishments, Equation 4 is estimated with negative binomial regressions. The changes in employment, shipments, and other measures can be thought of as counts of the number of new or destroyed jobs, etc. These distributions tend to be skewed and are not appropriately modeled by ordinary least squares. Poisson regressions are a natural choice. However, there are more ZIP codes with zero growth due to births, deaths, expanding, or contracting establishments than would be predicted by a Poisson. The negative binomial generalizes the Poisson by allowing for additional variation among ZIP codes that could account for these zeros. The dependent variable in these regressions is the change in the outcome, such as the change in employment for a ZIP code.[14] The exposure variable is the lagged denominator of the rate, such as lagged total employment.[15]

For the total (not decomposed) growth rates, tobit regressions were estimated. The negative binomial is not appropriate for these regressions because the "counts" are both positive and negative. The tobits provide a useful way of dealing with the limited dependent variable (Maddala, 1983). The growth rate used here is bounded below at –1. We also impose an upper bound of 1 on the growth rate. Only a small

[13] Since the non-zone ZIP codes, by definition, are never designated as zones, the designation date for each non-zone ZIP code is the designation date of its matched zone ZIP code.

[14] The dependent variable is always negative for the "Death" and "Contract" categories. To allow estimation of the negative binomial, we multiplied the dependent variables by –1. For both categories, all of the estimates of the changes in growth rates were then multiplied by –1 to restore the proper sign.

[15] Negative binomial regressions are appropriate in this application. The simple alternative would be to compare means (counts). Although they would be consistent, the estimates would not be very efficient. The methodology is analogous to using log wages rather than wages in a regression. The mean of the log of a skewed distribution is an MLE (and therefore efficient) whereas the mean is not. In this case, if the distribution is Poisson, which is likely given the data generation process, then the means will be ordered the same way the Poisson parameters are ordered. However, the Poisson parameters are more efficiently estimated, allowing better inference.

Justification for using a negative binomial stems from the data generation process. There is a mixture of processes: the underlying job generation process which we can model as Poisson and the sampling process that we can model as a gamma. The sampling process leads to an over-abundance of zeros since first there must be a sampled firm in the given category and only then is the change in employment used in the regression.

fraction of the ZIP codes had average annual growth rates that more than doubled from one year to the next.

Matched-Sample Results

Table 11 reports the differences ($\delta_3 - \delta_2$) and δ_1 and from the negative binomial regressions, as well as the difference in those differences. These differences can be interpreted as percentage changes. ($\delta_3 - \delta_2$) is reported in the EZ rows, δ_1 is reported in the Non-EZ rows, and the difference-in-difference estimate, ($\delta_3 - \delta_2$) – δ_1, is reported in the Difference rows. A Wald test is used to test whether the differences are significantly different from zero.[16]

As an example, again consider employment growth. The entries for Employment in the Total column indicate that the change in the average employment growth rate fell by 3.1 percent for the zones and fell by 2.7 percent for the comparison areas post-designation. The entry of –0.004 in the Difference row indicates that there is virtually no difference in the changes in the growth rates between the zones and the non-zones.

Analysis lacking the establishment-level data would conclude that enterprise zones had no impact on employment within the zones. By decomposing the data into the four establishment types, it becomes clear that the conclusion would be incorrect. The employment growth rate due to births grew by 63.2 percent post-designation in zones, and it fell by 53.9 percent in the non-zones. The difference in the differences (1.162) is significant at the 0.01 level. Although the zones appeared to encourage employment in the zones post-designation, they appeared to stunt the growth of employment in expanding establishments in the zones after designation. The growth rate of the expanding establishments fell by 11.8 percent post-designation in the zones, while it jumped by 48.7 percent in the non-zones. The difference –0.606 is significant at the 0.01 level. Shipments, payroll, and the number of establishments show similar patterns of the zones positively affecting growth rates due to new establishments, but negatively affecting the growth rates due to the expanding establishments. Looking at total changes, the zones had significant negative impacts on both shipments and payroll. In both cases, the change in the growth rates fell by more in the zones. For new capital spending, there was no significant total impact, although zones had a significant negative impact both among the births and the expanding establishments. Interestingly, zones performed worse after designation in all of the outcome measures except for capital spending (Total column). However, for changes in all of the other outcomes among new establishments, the zones performed better after designation (Birth column). Thus, zones appear to be most successful at encouraging increased business activity among new establishments.

Model Sensitivity

To examine how sensitive the results are to modeling the zone effect as changing growth rates versus causing a jump in levels (as in the Papke and Boarnet and Bogart models), we estimated a model that nests both the jump in levels model and our

[16] As mentioned above, the skewed nature of the growth rate distribution makes the tests on these negative binomial coefficients preferable to a simple difference in sample means test.

Table 11. Change in average annual growth rates after zone designation by zone and establishment status matched sample: 1984–1993.

Outcome	Zone Status	Establishment Type Birth[a]	Death[a,c]	Expand[a]	Contract[a,c]	Total[b]
Employment	EZ	0.623	–0.395	–0.118	–0.007	–0.031
	Non–EZ	–0.539	–0.669	0.487	–0.003	–0.027
	Difference	*1.162***	*0.273*	*–0.606****	*–0.005*	*–0.004*
Shipments	EZ	0.272	–0.414	0.000	–0.027	–0.056
	Non–EZ	–0.977	–0.614	0.957	0.093	–0.005
	Difference	*1.249****	*0.200*	*–0.957****	*–0.121*	–0.051***
Payroll	EZ	0.475	–0.437	0.026	–0.021	–0.063
	Non–EZ	–0.726	–0.687	0.443	0.041	–0.013
	Difference	*1.201****	*0.250*	*–0.417****	*–0.062*	*–0.050****
Capital	EZ	0.281	–0.817	–0.076	0.050	0.028
Spending	Non–EZ	1.394	–0.616	0.673	–0.009	–0.019
	Difference	*–1.113****	*–0.201*	*–0.749****	*0.060*	*0.047*
Establishments	EZ	0.480	0.394	–0.035	–0.008	–0.032
	Non–EZ	0.120	0.431	0.003	–0.067	–0.044
	Difference	*0.360***	*–0.037*	*–0.037*	*0.059*	*0.012*

* p-value ≤ 0.1 ** p-value ≤ 0.05 *** p-value ≤ 0.01

[a] Table entries for "EZ" and "Non-EZ" represent the coefficients $(\delta_3 - \delta_2)$ and δ_1 respectively from the negative binomial regressions of the change in employment (shipments/payroll/capital spending/number of establishments) regressed on a set of dummy variables measuring the timing of zone designation:

$$G_{it} = \delta_0 + \delta_1 NONEZAFT_{it} + \delta_2 EZPRE_{it} + \delta_3 EZAFT_{it} + \varepsilon_{it}$$

"Difference" is the difference between the change in the EZ and non-EZ growth rates: $(\delta_3 - \delta_2) - \delta_1$. Significance levels refer to the results of a Wald test testing the null hypothesis that differences are equal: H_0: $(\delta_3 - \delta_2) - \delta_1 = 0$.

[b] The table entries for the "Total" column represent the results of Tobit regressions.

[c] The dependent variable in the "Death" and "Contract" regressions is always negative. In order to estimate the negative binomial, the dependent variables were multiplied by –1. Therefore, the reported coefficients in these regressions were multiplied by –1.

change in growth rate model by allowing for time varying growth rates. We allow the growth rate to change at the time of designation and then to change again 2 years later. If the jump in levels specification is a better model, there will be a significant change in the growth rate at the designation date and then another equal but opposite change back to the original growth rate two years later.[17] If the growth rate model is superior, there should be a significant change in the growth rate at the time of designation and not further change after 2 years.[18] Both types of models nest the null hypothesis of no impact.

Examining employment changes, the results are mixed. The growth model fits the data better for the change in employment due to births, but the jump-in-levels model fits the data better for the total change in employment. The estimates indicate that for employment growth due to births, zone designation led to increased growth in the first 2 years that continued in the third year and beyond. However,

[17] We allow 2 years rather than one to account for possible imprecision in the designation date and small delays in implementation.

[18] Of course, other patterns are possible as well, such as no immediate change followed by a delayed change.

the tobit model for employment changes for all types of firms indicates a significant decrease in employment growth in zones relative to the comparison group in the first 2 years following designation, followed by a return to the previous growth rate. This finding is more consistent with the jump in levels model. For employment growth due to death, there is no significant change in the growth rate in either the first 2 years or beyond as would be expected from the estimates reported in Table 11. For firms that are expanding, the negative consequences of designation are delayed until after the first 2 years. For firms that are contracting, the growth rate in neither the first 2 years nor the later years is significantly different from the pre-designation rate, but the change between the two post designation periods is significant.

Use of the growth-rate model rather than the jump-in-levels model provides a number of advantages advancing the research on the impact of zone programs. Fundamentally, the use of the gross flows analysis requires use of a growth rate method. The results show how important it is to examine the gross flows because net flows can mask much of the churning—the openings and closing, expansions and contractions—that goes on even in stable industries. The change in growth rates model is also very appealing intuitively given that the impact of zone legislation is likely to be seen gradually over time rather than in a one-time jump due to the lumpiness to investment decisions. Indeed, the time varying model that replicated the one-time jump model did not show a convincing improvement, which was consistent with Bondonio and Engberg's (2000) analysis that did not find any support for a jump in levels model using some of the same data.

Geographic Comparison Group Outcomes

In addition to the matched sample, we also estimated the negative binomial and tobit regressions using comparison groups based upon geography. A 5-mile radius was drawn around the centroid of each zone ZIP code. All of the non-zone ZIP codes were placed within one of two "neighbor" categories—those close neighbors with ZIP centroids within 5 miles of a zone ZIP centroid (inner-ring) and the distant neighbors with ZIP centroids beyond 5 miles (outer-ring). This allows us to capture the influence of the zone policies on both close and more distant neighbors.

Unlike the very close matches achieved with the matched-sample approach, matching based on geography did not produce a set of comparison ZIP codes with similar characteristics. The same pattern emerged in all six states: the zone ZIP codes were the most distressed and the outer-ring ZIP codes were the least distressed.[19] Because of the large differences in pre-designation characteristics between the zone ZIP codes and each of the comparison rings, it is impossible to estimate an accurate measure of zone impact. Generally, the outer-ring ZIP codes continued to outperform the other two groups even after zone designation. However, the estimation of the differences in growth rates among the various geographic areas is very useful for providing evidence as to whether enterprise zone programs displaced economic activity from nearby areas. The lack of a large positive impact on business outcomes in the zone ZIP codes relative to the impact in

[19] Complete descriptive statistics and regression results from the geographic comparison group analysis are reported in Greenbaum (1998).

the inner-ring of ZIP codes supports the conclusion that little displacement is occurring.

CONCLUSION

We find that California, Florida, New Jersey, New York, Pennsylvania, and Virginia placed their enterprise zones in very distressed ZIP codes of their largest MSAs. To measure the effect of the enterprise zones on manufacturing business outcomes in those distressed urban areas, a carefully selected set of comparison ZIP codes was identified. In the first case, every zone ZIP code was matched to the most similar non-zone ZIP code in the same state. In the second case, comparison areas were selected based upon geography. Longitudinally linked establishment-level data allow for the important measurement of gross flow changes. Using the treatment zone ZIP codes and comparison non-zone ZIP codes, difference-in-difference estimates were calculated to measure the differences in pre and post designation growth rates between the zones and comparison areas.

Matched sample difference-in-difference estimates indicate that zones lead mostly to a churning of economic activity. Zones did lead to new business activity inside the zones. The number of births and employment, payroll, and shipments due to those births all increased significantly in the zones post-designation. However, zones appeared to be less successful at retaining existing activity. Among existing expanding establishments, employment, shipments, payroll, and capital spending all grew significantly more rapidly in the matched comparison areas. The estimates based upon the geographic comparison groups do not provide evidence of a zero-sum game stealing of businesses.

With regard to the factors of production, the results were also mixed. Although all six states' zone programs contain capital subsidy provisions, capital spending grew faster in the comparison areas than in the zones, both in new and ongoing manufacturing establishments. Five of the states (all but Pennsylvania) subsidize labor as part of their zone programs. Payroll did increase in zones relative to the non-zones in new establishments, but it fell in ongoing establishments. The net effect was a greater payroll decrease in zones than in the matched comparison group. Therefore, the decline in zone employment in ongoing establishments appears not to be offset by a shift to higher wage jobs.

Prior research on enterprise zones has been inconclusive. A number of studies using survey results have shown that zones have created many new jobs. On the other hand, more analytic studies using comparison samples, but measuring only net changes, have attributed much less employment growth to zone programs. The regression results in this paper are consistent with both sets of previous findings. By separately analyzing the gross employment changes of plants that are birth, death, expanding, or contracting establishments, we show that zones have different effects on the different types of establishments. Consistent with some other econometric results, we find that zones have no effect on overall employment growth. Consistent with the survey results, we find that zones do have an effect on employment growth among new establishments. However, that employment growth is offset by employment losses among ongoing establishments. Findings for shipments, payroll, and number of establishments are similar.

A number of explanations are possible for the negative effect on existing establishments. Perhaps the new businesses are merely displacing previously existing businesses. Because many of the zone subsidies are tied to the number of new hires or the amount of new investment, new establishments will receive a much larger

total subsidy than existing establishments. If new establishments compete with existing establishments in local product or factor markets, the new establishments' lower costs may give them a competitive advantage, allowing them to outbid existing establishments for new customers or new inputs. Contracting establishments would be unaffected given that they are likely to serve different product markets than new and expanding establishments[20] and because they are less likely to be competing for new inputs.

The politics of state economic development programs provides another possible explanation for these patterns. Local politicians and policy professionals are eager to trumpet new jobs and new activity in the zones because this evidence of "success" serves to help to continue or expand the programs. Jobs lost in the zones are often unlikely to be attributed to the zone policies. Because of this, the zone incentives may be targeted more toward new establishments rather than toward existing establishments. If the zone incentives are then marketed more towards attracting new establishments, managers of existing establishments may not be aware of all of the programs' incentives. Furthermore, it may be that the blighted areas that do not receive zone designation (the matched comparison group) are compensated with contracts for state business or other types of public investment that are more tailored to existing establishments.

As mentioned, the empirical analysis focuses on the manufacturing sector of the economy for which high-quality geo-coded gross flow data are available. Fortunately for this analysis, state enterprise zone programs focus on the manufacturing sector (Peters and Fisher, 2002), and Erickson and Friedman (1990a) found in their survey of 357 zones across 17 states that the manufacturing sector accounted for 72.6 percent of the jobs created or saved by enterprise zones. The manufacturing sector is often targeted because the jobs have higher wages and are more stable than many jobs in other sectors. These features of the manufacturing sector do not suggest, however, that our findings are likely to be unique to this sector. We expect that an analysis of the effect of spatially targeted incentive programs directed toward non-manufacturing firms would yield similar results, were the data available for such an analysis.

Future research should seek to identify both the particular aspects of the zone programs that appear to be helping new establishments and the direct or indirect ways in which the programs are hurting existing businesses. In particular, it will be important to distinguish whether the programs are inherently biased toward new establishments, or whether the programs can be successfully modified to help the incumbent businesses. It will be important to attempt to identify why the programs are failing to help existing establishments to expand employment, shipments, payroll, and spending.

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[20] The industry structure related to the production of any particular product often goes through a typical evolutionary path in which periods of rapid entry and expansion of output are followed by industry shakeout and eventual shrinkage of the market (Gort and Klepper, 1982).

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