Do Spatially Targeted Redevelopment Incentives Work? The Answer Depends on How You Ask the Question

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Abstract:
We compare several common program evaluation techniques in evaluating the Empowerment Zone (EZ) program, a large urban redevelopment program of primarily tax credits, run by the federal government. Studying the federal EZ program as a means to examine methodology is advantageous for several reasons. First, the federal program had an application process- generating a set of areas that were qualified, but did not receive the program, generating a comparison group that should not suffer from application bias. Second, the program also had pre-application rules for which areas would be considered- generating a rules-based group of comparison areas. Third, the program is fairly uniform across areas, so that program characteristics are not endogenous to local needs. Lastly, the geography where the program applies (and where rejected applicants are) is accounted for. We examine outcomes of the program under standard cross-section, difference-in-difference, difference-in-difference-in-difference, instrumental variable, and regression discontinuity style models, constructing a comparison groups using several alternatives. We examine the benefits drawbacks of each approach and how they may apply to evaluating spatially targeted policies more generally. Our results generally show wide-ranging estimates of program effectiveness, with both positive and negative point estimates and precise and imprecise results. We show robustness checks to examine sensitivity within evaluation method, and test assumptions of methods when possible. We conclude that caution should be taken when interpreting the results of any one evaluation method as definitive, and suggest that the true effect of the EZ program on outcomes of interest is likely uncertain.

JEL: H25; H32; R51

Keywords: Program Evaluation; Methodology; Economic Redevelopment; Employment; Firm Location

I. Introduction

Spatially targeted, or place-based incentive programs abound at all levels of government in both the United States and Europe. Although the details of these programs can be as different as the areas they are targeted to, the common theme is that they espouse benefits based on geographic location within an otherwise homogeneous unit of geography. Often program benefits come in the form of tax incentives (but also as grants, capital infusions, and other means) creating policy heterogeneity within an otherwise policy homogeneous unit.

Place based programs have been the subject of inquiry in a large and growing body of empirical studies by academics. Many of these studies focus on outcomes for residents or firms within the boundary created by the policy and examine outcomes such as wages, employment, poverty, property values, and business location. Neumark and Simpson (forthcoming) provide an exhaustive review of the current state of the literature and point out that most of the attention is given to zone-based programs like Enterprise or Empowerment Zones. Neumark and Simpson come to the conclusion that the evidence on the effectiveness on these programs is decidedly mixed. This view comes from a broad interpretation of the literature as a whole, as individual studies rarely leave much room for ambiguity– either they find positive (in some cases quite large) effects as in Ham, Imrohoroglu, and Swenson (2011), Freedman (2013) and Busso, Gregory, and Kline (2013); or negative, null or diffuse effects as in Oakley and Tsao (2006), Elvery (2009), Hanson (2009), Neumark and Kolko (2010), and Reynolds and Rohlin (2015).

The goal of this paper is to explore the role that evaluation technique plays contributing to the mixed finding on program outcomes in the literature on place based policies. We compare several common program evaluation techniques in evaluating the Empowerment Zone (EZ)

program, a large urban redevelopment program in the United States consisting of primarily tax credits, run by the federal government. The federal EZ is advantageous for this task because it has a mostly uniform set of benefits that do not depend on local variation in characteristics that are correlated with outcomes, the program generated a set of comparison areas through an application process and through rules based criteria, and the geography of targeted and application areas is well documented.

To understand how methodology might impact the mixed findings in the literature, we examine outcomes of the federal EZ under cross-section, difference-in-difference, difference-in-difference-in-difference, instrumental variable, and regression discontinuity style models, constructing a comparison groups using both program rules and rejected applicants. We examine the benefits drawbacks of each approach and how they may apply to evaluating spatially targeted policies more generally. We show robustness checks to examine sensitivity within evaluation method, and test assumptions of methods when possible.

We demonstrate that popular program evaluation methods yield different results for the same program. Even basic cross section methods that vary only by control group produce vastly different results– anywhere from a 25 percent loss to a 30 percent gain in jobs at firms in targeted areas, and between a 37 percent loss and an 11 percent gain in the number of firms in targeted areas. Difference-in-Difference results generally produce positive outcomes, but again these findings vary with the comparison group and are generally not precisely estimated. Triple difference estimation uniformly suggests a positive impact of the program on employment and the number of firms, and is generally precisely estimated, while using instrumental variables within the differencing framework produces large, positive, but statistically imprecise results. Finally, regression discontinuity designs, which are rarely used in this literature despite their

recent prominence in program evaluation more generally, produce a wide range of findings that are sensitive to the use of the forcing variable, bandwidth, and control function. Overall these findings suggest that the choice of methodology is influential in determining the outcomes of a program evaluation of the federal EZ program, and while some methods are almost surely superior to others, we believe a robust evaluation of spatially targeted policies should include extending beyond one type of estimation strategy. We view these findings as a caution to program evaluators in using any single method or even single evaluation to do cost–benefit analysis of these types of programs.

The remainder of the paper begins with a brief overview of the literature on place based policies with a focus on the method used for evaluation. The third section describes the details of the federal EZ program and describes why it is a good candidate to compare current program evaluation methodologies. The fourth section of the paper outlines each identification strategy separately and discusses the benefits and drawbacks of each. The fifth section of the paper summarizes the results across methodologies and the final section of the paper offers concluding comments.

II. Overview of Place-Based Policy Evaluations

Neumark and Simpson (forthcoming) do an exhaustive overview; we focus here on methodology choice and general outcomes.

III. The Federal Empowerment Zone Program

The federal government began to offer tax incentives to employers located in parts of economically distressed areas with the creation of the Empowerment Zone program, which was passed into law as part of the 1993 Budget Reconciliation (OBRA,1993, P.L.103-66). HUD designated parts of six cities and three rural areas as EZ. EZs were chosen from a group of applications made by state and local governments. Applications were considered for areas where at least 20% of the population lived in poverty and 6.3% were unemployed (GAO, 2004). From 78 nominees (Wallace, 2004), the federal government awarded EZ status to parts of Atlanta, Baltimore, Chicago, Detroit, Philadelphia/Camden, and New York. Rural EZs, which we do not include in our analysis here were formed in the Kentucky Highlands, Mississippi Delta, and the Rio Grande Valley in Texas. Zones were established as groups of census tracts.

The EZ program is primarily a set of tax incentives claimed by employers who operate inside of well-defined geographic boundaries and hire residents of those areas. For the original urban EZs, $100 million in the form of Social Service Block Grant (SSBG) funds accompanied the tax incentives. The largest component of the EZ program is the wage tax credit, which allows employers operating in the zone that hire residents of the zone to claim up to a $3000 tax credit per employee. Other tax incentives offered to firms operating in EZ designated areas include: an increase in the amount immediate expensing allowed, postponement of capital gains reporting, an increase in small business stock exclusion, and temporarily allowing state and local governments to operate outside of the normal restriction on tax exempt bonds are offered on behalf of EZ businesses.

Many of the nominees that did not receive EZ status were given a "runner-up" award called Enterprise Communities (EC)[1] a less generous overall package of assistance with a limited

[1] The runner-up group consists of parts of the following cities: Akron, OH, Albany, GA , Albany, NY, Albuquerque, NM, Birmingham, AL, Boston, MA, Bridgeport, CT, Buffalo, NY, Burlington, VT, Charleston, SC, Charlotte, NC,

set of tax incentives. The biggest difference between EZs and ECs is that EC employers cannot claim the wage tax credit and EC zones were typically allowed only $3 million in SSBG funds.[2]

In many ways, EC areas form a natural comparison group for EZ areas. These areas were nominated to be EZs by local governments, went through the application process and were deemed worthy of some form of assistance, so they may have some unobservable characteristics in common with EZ designated areas that are correlated with outcomes of interest that we would want to separate out from program effects. Indeed, several evaluations of the EZ program use EC applicants as a control group (Krupka and Noonan, 2009; Hanson, 2009; Busso, Gregory, and Kline, 2013; Reynolds and Rohlin, 2015).

We follow the previous literature and use EC areas as one way to construct a control group to create a counterfactual for what would have happened in EZ designated areas if not for the EZ program. We also use the hard cut-off for poverty and unemployment limits in the program in a regression discontinuity design. Furthermore, as pointed out in Hanson (2009), EZs were designated as part of a contentious budget bill, suggesting that they may have been used as a political bargaining chip to gain a favorable vote. This is potentially advantageous from an identification standpoint as it means that as least part of EZ areas may have been chosen not based on a notion of future success or failure, but because of congressional representation.

Cleveland, OH, Columbus, OH, Dallas, TX, Denver, CO, Des Moines, IA, East St. Louis, IL, El Paso, TX, Flint, MI, Harrisburg, PA, Houston, TX, Huntington, WV, Indianapolis, IA, Ironton, OH, Jackson, MS, Kansas City, KS, Kansas City, MO, Las Vegas, NV, Little Rock, AR, Los Angeles, CA, Louisville, KY, Lowell, MA, Manchester, NH, Memphis, TN, Miami, FL, Milwaukee, WI, Minneapolis, MN, Muskegon, MI, Nashville, TN, New Haven, CT, Newark, NJ, Newburgh, NY, Norfolk, VA, Oakland, CA, Ogden, UT, Oklahoma City, OK, Omaha, NE, Phoenix, AZ, Pittsburgh, PA, Portland, OR, Providence, RI, Rochester, NY, San Antonio, TX, San Diego, CA, San Francisco, CA, Seattle, WA, Springfield, IL, Springfield, MA, St. Louis, MO, St. Paul, MN, Tampa, FL, Waco, TX, Washington, DC, and Wilmington, DE.

[2] The Boston, Oakland, Houston and Kansas City nominees were designated as Enhanced Enterprise Communities (EEC). EEC status gave these communities a more generous allocation of grant funds than the standard Enterprise Communities. Two nominees, Cleveland and Los Angeles, were awarded the status of Supplemental Empowerment Zone (SEZ) (GAO, 2004), which did not allow for all of the tax benefits of regular EZs, but included more generous grants than regular EZs ($450 million for Los Angeles, and $177 million for Cleveland).

We use this to create an instrument for EZ designation– member representation (and number of terms) on the powerful House Ways and Means committee.

IV. Methods of Identifying Program Effects

We focus on comparing methodology for identifying the effects of the federal EZ program on two outcomes: the number of employees working at firms located in EZ areas, and the number of firms located in EZ areas.[3] We examine these outcomes in both the short (two year) and long (six year) time horizon, as the impact of any program may change over time as markets react and information reaches more of the targeted group.

Cross Section Regression Comparisons

Cross section regression as an evaluation technique does not carry many advantages beyond simplicity, especially when data exists both before and after the program intervention and there are obvious control groups to make a difference-in-difference comparison with. We present cross section regression as a technique mainly as a benchmark and to demonstrate how the choice of control group impacts outcome measurement.

The basic cross section regressions take the form:

$$(1) \qquad \ln(Y_i) = \alpha + \beta(D = 1\ if\ EZ)_i + X'_i\delta + \varepsilon$$

We estimate (1) one year after the program takes effect (1996) and five years after it takes effect (2000). Y_i is either the number of employees at firms or the number of firm in census tract i.

[3] Other research examines property values (Hanson, 2009; Krupka and Noonan 2009; Busso, Gregory, and Kline 2013), rents (Busso, Gregory and Kline 2013), neighborhood demographics (Krupka and Noonan 2009, Busso, Gregory and Kline 2013), and wages (Busso, Gregory, and Kline 2013). Busso, Gregory, and Kline (2013) also estimate the impact of the federal EZ separately by place of residence and place of work.

We control for a series characteristics in 1990 levels and the change between 1980 and 1990, denoted by X'_i. These control variables are: the unemployment rate, poverty rate, percent non-white population, percent of female headed families with children, percent of population age 25 or older with at least a college degree, average age of housing stock (and this term squared), and the homeownership rate.

We restrict the comparison area for estimating (1) in several different ways. First, we restrict the sample to include only EZ or EC designated census tracts. This restriction is our closest match to the primary specification used in Busso, Gregory, and Kline (2013), and arguably does the best job of limiting unobservable factors that would confound program estimates, but leaves a fairly small sample size. Second we estimate (1) restricting the comparison group to only census tracts in metropolitan areas that met the poverty and unemployment qualifications of being an EZ. Third, we estimate (1) restricting the sample by a propensity scoring method. We estimate how pre-program factors and political representation influence EZ designation and create a propensity score for each census tract in the U.S. We then restrict the control group to be only census tracts in the top 1 percent of the propensity score distribution.

We also estimate two versions of (1) that should obviously produce bias estimates. These include estimating (1) without restriction on the data and using all census tracts identified as being in a metropolitan area in the U.S as our control group and using only census tracts that border actual EZs as the control group.

Difference-in-Difference Comparisons

The federal EZ program lends itself nicely to a standard difference in difference comparison. Data across time and geographic areas is readily available, and the program makes clear designation of treatment areas as well as some natural control groups.

Our basic difference-in-difference estimating equation is:

$$(2) \quad \ln(Y_{i,t}) = \alpha + \beta_1(D = 1\ if\ EZ)_i + \beta_2(D = 1\ if\ After)_t + \beta_3(D = 1\ if\ EZ) * (D = 1\ if\ After)_{i,t} + X'_i\delta + \varepsilon$$

We estimate (2) for the same set of comparison areas that we outline in the cross section regression section. Three of these, the EC group, qualified areas, and the propensity score trimmed sample have legitimate reasons for inclusion as a control group. We also estimate (2) using two samples that have obvious problems as a control sample: all metropolitan area census tracts and census tracts bordering EZ areas.

Triple Difference Comparisons

Standard difference-in-difference estimation suffers from bias if the cities that were designated EZs were on a different growth path than comparison cities. Note that this would also be true if only the neighborhoods were on a different growth path. It seems plausible, especially given the small number of treated areas that the group of EZ cities could, on average, have been subject to differential change in outcomes of interest even in the absence of the program. For example, city living becoming sheik again in New York and Chicago, the Atlanta Olympic games and the on-going inner harbor redevelopment in Baltimore may have all contributed to differential growth in those treated cities even in the absence of EZ designation.

Triple difference estimation, where program effects are a comparison of how EZ tracts faired relative to other tracts within their city and between EC tracts and tracts in EC cities,

eliminates general city-level improvement as a potential confounding factor. The triple difference specification is:

$$
\begin{aligned}
(3)\quad \ln(Y_{i,t,c}) = {} & \alpha + \beta_1(D = 1\ if\ EZ\ or\ EC)_i + \beta_2(D = 1\ if\ After)_t \\
& + \beta_3(D = 1\ if\ EZ\ City)_c + \beta_4(D = 1\ if\ EZ\ or\ EC) * (D = 1\ if\ After)_{i,t} \\
& + \beta_5(D = 1\ if\ After) * (D = 1\ if\ EZ\ City)_{t,c} \\
& + \beta_6(D = 1\ if\ EZ\ or\ EC) * (D = 1\ if\ EZ\ City)_{i,c} \\
& + \beta_7(D = 1\ if\ EZ\ or\ EC) * (D = 1\ if\ After) * (D = 1\ if\ EZ\ City)_{i,t,c} + X'_i\delta \\
& + \varepsilon
\end{aligned}
$$

The same set of comparison areas as we used for the cross section and difference-in-difference estimation cannot be used for the triple difference specification, as we now must consider only areas within EZ and EC cities for differencing. We estimate (3) using three potentially legitimate control groups and one that should be subject to bias.

First, we estimate a standard triple difference between EZ areas and their own city with EC areas and their own city. Next, we re-estimate the standard triple difference, but exclude areas that border EZ areas as they may be subject to spillovers. Third, we limit the sample to areas within EZ and EC cites that met program qualifications, but were not part of the application. Finally, we limit the sample to only areas the border EZ and EC areas in an attempt to show how this choice might produce biased estimates due to spillovers.

Instrumental Variables with Differencing

Triple difference estimates add a layer of sophistication to an EZ program evaluation that insulates program estimates from any confounding factors that differentially impacted the group of EZ cities. These estimates still leave open the possibility that EZ areas themselves were chosen for reasons that were directly related to future success or failure even in the absence of the program. This in effect means that the policy itself is endogenously determined. A potential way around this problem is to find an instrument for program assignment. An instrument

requires something that is correlated with EZ status, but not directly correlated with outcomes of interest.

Hanson (2009) points out that the vote on EZ legislation being particularly contentious may provide such an instrument and suggests the representation on the House Ways and Means committee (and the number of terms serving) as a potential instrument for EZ designation. This instrument is advantageous, as these congressional districts only partially overlap with EZ areas, essentially grouping them into areas that were endogenously designated and designated due to the exogenous influence of a powerful congressional member. The first stage regression is the following:

$$(4) \quad EZ_i = \alpha + \beta_1(D = 1 \; if \; Ways \; and \; Means \; Rep)_i + \beta_2(terms)_i + X'_i\delta + \varepsilon$$

Which leads to the second stage estimation for the difference-in-difference specification:

$$(5) \quad \ln(Y_{i,t2} - Y_{i,t1}) = \alpha + \beta_1(\widehat{EZ})_{i,t} + X'_i\delta + \varepsilon$$

And the second stage estimation for the triple difference specification:

$$(6) \quad \ln(Y_{i,t2} - Y_{i,t1}) = \alpha + \beta_1(D = 1 \; if \; EZ \; or \; EC)_i + \beta_2(D = 1 \; if \; EZ \; City)_c + \beta_3(EZ * \widehat{EZCity})_{i,c} + X'_i\delta + \varepsilon$$

For ease of use, we take the difference across time in the dependent variable to estimate (5) and (6). The reliability of (5) and (6) as unbiased estimates of the EZ program hinges crucially on House Ways and Means membership in 1993 not being directly correlated with employment or the number of firms in EZ areas.

Regression Discontinuity Design

As an alternative to differencing and instrumental variables estimation, we explore the use of regression discontinuity designs to estimate the effect of the EZ program. Because the EZ uses national cut-offs for both poverty and unemployment it lends itself well to regression discontinuity estimation. The effect of EZ designation in these models is measured by looking at outcome differences between EZ designated areas and other census tracts that are all within a narrow bandwidth around either cut-off. We estimate the following regression discontinuity specification:

$$(7)\quad \ln(Y_{i,t2} - Y_{i,t1}) = \alpha + \beta_1(D = 1\ if\ EZ) + \sum_{j=1}^{d}\left[\varphi_{1,j}(v_i - cp_t)^j + \varphi_{2,j}EZ_i(v_i - cp_t)^j\right] + \varepsilon\ \ if\ (cp_t - b) \le v_i \le (cp_t + b)$$

Where the coefficient of interest is β_1, and the terms inside the summation are the control function. We estimate (7) using both a linear control function (*j*=1) and a quadratic control function (*j*=2). We also estimate (7) using both the national unemployment cut-off (*cp*=0.063, and *v* representing tract-level unemployment) and the national poverty rate cut-off (*cp*=0.20, and *v* representing tract-level poverty rate). We estimate each cut-off and control function across three different choices of bandwidth around the cut-off (measured by *b*).

V. Data

Data on our outcomes of interest, employment at zone firms and the number of zone firms, comes from the Dun and Bradstreet (D&B) Marketplace database. The data consist of the fourth-quarter survey from the years 1994, 1996, and 2000. The D&B data are aggregated at the

zip code level. To map the zip code level data on local establishments to census tracts, we use a correspondence to match the geography of the EZ and EC designated areas. The correspondence determines what percentage of each zip code lies in a given census tract and assigns that percent of zip code employment or establishments to the census tract. Our list of EZ and EC census tracts was obtained through personal correspondence with the Department of Housing and Urban Development and is also partially available through that department's Web page. Each EZ or EC designated area is made of several census tracts, we treat census tracts as the unit of observation—not an entire EZ or EC, which would severely limit the number of observations in our data.

VI. Results

We demonstrate that popular program evaluation methods yield different results for the same program. Even basic cross section methods that vary only by control group produce vastly different results– anywhere from a 25 percent loss to a 30 percent gain in jobs at firms in targeted areas, and between a 37 percent loss and an 11 percent gain in the number of firms in targeted areas. Difference-in-Difference results generally produce positive outcomes, but again these findings vary with the comparison group and are generally not precisely estimated. Triple difference estimation uniformly suggests a positive impact of the program on employment and the number of firms, and is generally precisely estimated, while using instrumental variables within the differencing framework produces large, positive, but statistically imprecise results. Finally, regression discontinuity designs, which are rarely used in this literature despite their recent prominence in program evaluation more generally, produce a wide range of findings that are sensitive to the use of the forcing variable, bandwidth, and control function.

VII. Conclusion

Overall these findings suggest that the choice of methodology is influential in determining the outcomes of a program evaluation of the federal EZ program, and while some methods are almost surely superior to others, we believe a robust evaluation of spatially targeted policies should include extending beyond one type of estimation strategy. We view these findings as a caution to program evaluators in using any single method or even single evaluation to do cost–benefit analysis of these types of programs.

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Table 1: Cross Section Identification Methods

	vs. EC^	vs. Qualified	vs. P-Score	vs. All Tracts	vs. Border Tracts
# of Employees					
Short Term ('94-'96)					
	-0.257	0.036	-0.179	0.308	0.038
	(0.173)	(0.185)	(0.194)	(0.215)	(0.144)
N	1,246	9,086	517	49,076	728
Long Term ('94-'00)					
	-0.312	-0.004	-0.228	0.269	0.040
	(0.199)	(0.205)	(0.214)	(0.237)	(0.165)
N	1,304	9,436	536	51,702	728
# of Firms					
Short Term ('94-'96)					
	-0.376**	-0.122	-0.286**	0.110	0.009
	(0.156)	(0.145)	(0.116)	(0.169)	(0.072)
N	1,246	9,086	517	49,076	728
Long Term ('94-'00)					
	-0.376**	-0.136	-0.313**	0.088	-0.003
	(0.156)	(0.157)	(0.129)	(0.183)	(0.085)
N	1,246	9,086	536	51,702	728

All estimates show Beta coefficient from equation (1). Number of employees is the number of employees at firms operating within the census tract at firms in any industry. Number of firms is the total number of firms operating within the census tract. All outcome data are from Dun and Bradstreet survey of firms ZIP code level data aggregated to the census tract level using ArcGIS software. All regressions control for the following census tract characteristics in 1990 levels and in changes between 1980 and 1990: unemployment rate, poverty rate, percent non-white population, percent of female headed families with children, percent of population age 25 or older with at least a college degree, average age of housing stock (and this term squared), and the homeownership rate (all of these measures come from the 1990 or 1980 decennial census). Unit of observation is the census tract. All standard errors are clustered at the county level.

^ EC comparison tracts include the Los Angeles and Cleveland Supplemental Empowerment Zones and the Washington D.C. Enterprise Community. The Los Angeles and Cleveland zones were awarded more generous allocations of block grants than the treated Empowerment zones, but were not allowed to claim the same tax advantages (notably, the wage tax credit). The Washington D.C Enterprise Community was allowed to claim the wage tax credit begining in August 1997. Results that exclude Los Angeles, Cleveland, and Washington, D.C. show negative coefficients that larger in magnitude more precisely estimated than those presented here.

Table 2: Difference-in-Difference Identification Methods

	vs. EC^	vs. Qualified	vs. P-Score	vs. All Tracts	vs. Border Tracts
# of Employees					
Short Term ('94-'96)					
	0.093	0.071	0.189*	0.027	0.081*
	(0.107)	(0.097)	(0.103)	(0.094)	(0.039)
N	2,550	18,059	1,040	98,596	1,456
Long Term ('94-'00)					
	0.014	-0.040	0.145	-0.185	0.076
	(0.137)	(0.129)	(0.119)	(0.127)	(0.059)
N	2,608	18,409	1,059	101,222	1,456
# of Firms					
Short Term ('94-'96)					
	0.060	0.093*	0.154***	0.119**	0.110***
	(0.062)	(0.049)	(0.056)	(0.051)	(0.032)
N	2,550	18,059	1,040	98,596	1,456
Long Term ('94-'00)					
	0.007	-0.029	0.148	-0.137	0.078*
	(0.010)	(0.092)	(0.107)	(0.093)	(0.036)
N	2,608	18,409	1,059	101,222	1,456

All estimates show Beta_3 coefficient from equation (2). Number of employees is the number of employees at firms operating within the census tract at firms in any industry. Number of firms is the total number of firms operating within the census tract. All outcome data are from Dun and Bradstreet survey of firms ZIP code level data aggregated to the census tract level using ArcGIS software. All regressions control for the following census tract characteristics in 1990 levels and in changes between 1980 and 1990: unemployment rate, poverty rate, percent non-white population, percent of female headed families with children, percent of population age 25 or older with at least a college degree, average age of housing stock (and this term squared), and the homeownership rate (all of these measures come from the 1990 or 1980 decennial census). Unit of observation is the census tract. All standard errors are clustered at the county level.

^ EC comparison tracts include the Los Angeles and Cleveland Supplemental Empowerment Zones and the Washington D.C. Enterprise Community. The Los Angeles and Cleveland zones were awarded more generous allocations of block grants than the treated Empowerment zones, but were not allowed to claim the same tax advantages (notably, the wage tax credit). The Washington D.C Enterprise Community was allowed to claim the wage tax credit begining in August 1997. Results that exclude Los Angeles, Cleveland, and Washington, D.C. show coefficients that are nearly identical in magnitude not precisely estimated.

Table 3: Difference-in-Difference-in-Difference Identification Methods

	vs. EC^	vs. EC (exclude border)	vs. EC (qualified within City)	vs. EC (only border)
# of Employees				
Short Term ('94-'96)				
	0.272**	0.310***	0.197*	0.093*
	(0.107)	(0.119)	(0.103)	(0.053)
N	23,584	19,775	8,561	7,005
Long Term ('94-'00)				
	0.307**	0.351***	0.180	0.142***
	(0.119)	(0.132)	(0.113)	(0.051)
N	24,016	20,130	8,697	7,144
# of Firms				
Short Term ('94-'96)				
	0.144**	0.158**	0.101*	0.053
	(0.063)	(0.067)	(0.057)	(0.043)
N	23,584	19,775	8,561	7,005
Long Term ('94-'00)				
	0.191**	0.217**	0.088	0.101**
	(0.082)	(0.091)	(0.077)	(0.040)
N	24,016	20,130	8,697	7,144

All estimates represent the Beta_7 coefficient from equation (3). Number of employees is the number of employees at firms operating within the census tract at firms in any industry. Number of firms is the total number of firms operating within the census tract. All outcome data are from Dun and Bradstreet survey of firms ZIP code level data aggregated to the census tract level using ArcGIS software. All regressions control for the following census tract characteristics in 1990 levels and in changes between 1980 and 1990: unemployment rate, poverty rate, percent non-white population, percent of female headed families with children, percent of population age 25 or older with at least a college degree, average age of housing stock (and this term squared), and the homeownership rate (all of these measures come from the 1990 or 1980 decennial census). Unit of observation is the census tract. All standard errors are clustered at the county level.

^ EC comparison tracts include the Los Angeles and Cleveland Supplemental Empowerment Zones and the Washington D.C. Enterprise Community. The Los Angeles and Cleveland zones were awarded more generous allocations of block grants than the treated Empowerment zones, but were not allowed to claim the same tax advantages (notably, the wage tax credit). The Washington D.C Enterprise Community was allowed to claim the wage tax credit begining in August 1997. Results that exclude Los Angeles, Cleveland, and Washington, D.C. show coefficients that are slightly larger in magnitude precisely estimated at the one-percent level.

Table 4: Instrumental Variable Identification Methods

	D-i-D with EC, Instrumented	DDD with EC, Instrumented
# of Employees		
Short Term ('94-'96)		
	1.300	1.297
	(1.542)	(1.112)
N	1,146	10,948
Long Term ('94-'00)		
	0.849	1.067
	(1.243)	(0.895)
N	1,199	11,295
# of Firms		
Short Term ('94-'96)		
	0.107	0.420
	(0.819)	(0.766)
N	1,246	11,576
Long Term ('94-'00)		
	0.802	0.911
	(0.935)	(0.775)
N	1,302	11,950

Coefficients represent either the Beta_1 coefficent in Equation (5) in the case of D-i-D, or Beta_3 coefficient in Equation (6) for DDD. Number of employees is the number of employees at firms operating within the census tract at firms in any industry. Number of firms is the total number of firms operating within the census tract. All outcome data are from Dun and Bradstreet survey of firms ZIP code level data aggregated to the census tract level using ArcGIS software. First stage of IV regression uses local representation on the House Ways and Means committee and the number of terms the member had served at the time of EZ designation as instruments. All regressions control for the following census tract characteristics in 1990 levels and in changes between 1980 and 1990: unemployment rate, poverty rate, percent non-white population, percent of female headed families with children, percent of population age 25 or older with at least a college degree, average age of housing stock (and this term squared), and the homeownership rate (all of these measures come from the 1990 or 1980 decennial census). Unit of observation is the census tract. All standard errors are clustered at the county level.

^ EC comparison tracts include the Los Angeles and Cleveland Supplemental Empowerment Zones and the Washington D.C. Enterprise Community. The Los Angeles and Cleveland zones were awarded more generous allocations of block grants than the treated Empowerment zones, but were not allowed to claim the same tax advantages (notably, the wage tax credit). The Washington D.C Enterprise Community was allowed to claim the wage tax credit begining in August 1997. Results that exclude Los Angeles, Cleveland, and Washington, D.C. show coefficients that are slightly larger in magnitude precisely estimated at the one-percent level.

Table 5: Unemployment Regression Discontinuity Identification Methods

	Large Bandwidth (0.043 ≤ b ≤ 0.083)	Medium Bandwidth (0.053 ≤ b ≤ 0.073)	Small Bandwidth (0.058 ≤ b ≤ 0.068)
# of Employees			
Short Term ('94-'96)			
linear	1.562***	2.414***	2.362***
	(0.425)	(0.227)	(0.0308)
quadratic	1.703***	2.225***	2.371***
	(0.595)	(0.0318)	(0.0325)
N	22,686	11,328	5,694
Long Term ('94-'00)			
linear	1.469***	2.096***	2.056***
	(0.391)	(0.252)	(0.0317)
quadratic	1.412**	1.901***	2.067***
	(0.560)	(0.0326)	(0.0324)
N	24,022	11,998	6,004
# of Firms			
Short Term ('94-'96)			
linear	0.928**	1.048**	2.093***
	(0.368)	(0.526)	(0.0296)
quadratic	1.166**	2.199***	2.098***
	(0.516)	(0.227)	(0.0312)
N	23,424	11,692	5,878
Long Term ('94-'00)			
linear	0.551*	0.703	1.626***
	(0.318)	(0.463)	(0.0310)
quadratic	0.832*	1.671***	1.632***
	(0.466)	(0.343)	(0.0315)
N	24,570	12,274	6,159

All estimates represent Beta_1 coefficient in equations (7). Number of employees is the number of employees at firms operating within the census tract at firms in any industry. Number of firms is the total number of firms operating within the census tract. All outcome data are from Dun and Bradstreet survey of firms ZIP code level data aggregated to the census tract level using ArcGIS software. All regressions control for the following census tract characteristics in 1990 levels and in changes between 1980 and 1990: unemployment rate, poverty rate, percent non-white population, percent of female headed families with children, percent of population age 25 or older with at least a college degree, average age of housing stock (and this term squared), and the homeownership rate (all of these measures come from the 1990 or 1980 decennial census). Unit of observation is the census tract. All standard errors are clustered at the county level.

Table 6: Poverty Regression Discontinuity Identification Methods

	Large Bandwidth (0.043 ≤ b ≤ 0.083)	Medium Bandwidth (0.053 ≤ b ≤ 0.073)	Small Bandwidth (0.058 ≤ b ≤ 0.068)
# of Employees			
Short Term ('94-'96)			
linear	0.396	0.149	0.084
	(0.401)	(0.510)	(0.567)
quadratic	0.598	-0.265	-1.213***
	(0.632)	(0.577)	(0.046)
N	12,018	7,764	3,788
Long Term ('94-'00)			
linear	0.0537	-0.229***	-0.214***
	(0.524)	(0.033)	(0.035)
quadratic	0.528**	-0.190***	-0.210***
	(0.249)	(0.036)	(0.039)
N	12,763	8,258	3,996
# of Firms			
Short Term ('94-'96)			
linear	0.821***	0.669***	0.536**
	(0.148)	(0.213)	(0.212)
quadratic	0.991***	0.239**	0.054
	(0.185)	(0.117)	(0.045)
N	12,438	8,038	3,914
Long Term ('94-'00)			
linear	-0.035	-0.213	-0.324
	(0.169)	(0.220)	(0.224)
quadratic	0.164	-0.596***	-0.835***
	(0.197)	(0.149)	(0.044)
N	13,087	8,452	4,085

All coefficients represent the EZ variable from equations (X)-(Z). Number of employees is the number of employees at firms operating within the census tract at firms in any industry. Number of firms is the total number of firms operating within the census tract. All outcome data are from Dun and Bradstreet survey of firms ZIP code level data aggregated to the census tract level using ArcGIS software. All regressions control for the following census tract characteristics in 1990 levels and in changes between 1980 and 1990: unemployment rate, poverty rate, percent non-white population, percent of female headed families with children, percent of population age 25 or older with at least a college degree, average age of housing stock (and this term squared), and the homeownership rate (all of these measures come from the 1990 or 1980 decennial census). Unit of observation is the census tract. All standard errors are clustered at the county level.