Enterprise Zones and Local Employment: Evidence from the States' Programs*

June 1999

Daniele Bondonio[+]
John Engberg[++]

Heinz School of Public Policy and Management
Carnegie Mellon University
Pittsburgh, PA 15213-3890
Fax: (412) 268-7036

Abstract

Many states respond to deteriorating economic conditions in their inner cities and rural communities by establishing geographically targeted tax incentives. In this paper, we examine the impact of several of these Enterprise Zone (EZ) programs on local employment. The results show that the EZ programs do not have a significant impact on local employment. Program impact does not depend on the monetary amount of the incentives and or on specific features of program design. These conclusions are constant across two econometric approaches to controlling for the non-random placement of zones and stand up to a wide variety of sensitivity analyses.

Keywords: Enterprise zones, Economic development, Program evaluation
JEL Classification Codes: O1, R5, C23

* We would like to thank Robert Greenbaum for his contribution to this research. We are indebted to Peter Fisher and Alan Peters for providing access to their software package *TAIM2* that we used to evaluate the monetary value of economic development incentives. The first author gratefully acknowledges financial and research support from the International Center for Economic Research, ICER, Turin, and the research assistance in managing and processing the data provided by Nadia Villa (ICER) and Ada Ferrero (ICER). This paper is a revised version of a paper by the first author presented at the 20th APPAM Annual Research Conference, New York, and circulated under the title "States' Enterprise Zone Policies and Local Employment: What Lessons Can be Learned?" (Bondonio 1998).

[+]Corresponding author: db92@andrew.cmu.edu, Tel. (412) 268-8892; Fax. (412) 268-7036.
[++] je0i@andrew.cmu.edu, Tel.(412) 268-6281.

1. INTRODUCTION

In response to deteriorating conditions in many U.S. urban and rural areas, a vast majority of states have provided tax and other business incentives aimed at encouraging businesses to relocate to (or to avoid leaving) these depressed areas. These types of initiatives emerged during the early 1980's as geographically targeted policies referred to as "enterprise zone" (EZ) programs - a term borrowed from a similar economic development policy started in the U.K. in 1981. In the U.S., enterprise zone policies were initiated autonomously by a number of states, rather than by the Federal government. By the time that the Federal government implemented (at the beginning of 1994) a spatially-targeted economic development policy, referred to as the "empowerment zone/enterprise community" program, almost forty states had passed their own versions of EZ programs.

Because the states initiated the EZ policies independently, a large variety of programs emerged. States' EZ programs vary over a number of important dimensions. For example, the programs vary in the type and the monetary generosity of the incentives offered to zone businesses, the criteria for selecting the targeted areas designated as EZ, and the business eligibility rules to receive the EZ incentives. In principle, the states' experience of the recent past can be thought of as a natural experiment from which valuable lessons can be learned to refine future local economic development tools. In this paper we analyze the states' experiences to learn about the impact of different EZ policies on local employment.

We use policy and outcome data from five states: California, Kentucky, New York, Pennsylvania and Virginia. The data used in this paper are at the U.S Postal ZIP code level, whose boundaries usually do not match those of enterprise zones. Therefore, the goal of this paper is to assess which EZ policy, if any, is able to achieve an effect on employment which is noticeable at least at the level of the immediately surrounding community represented by the ZIP code areas which encompass any portion of the zone. State EZ programs are commonly designed with the stated aim to boost employment of distressed community areas. Measuring the impact of EZ policies on encompassing ZIP code areas is therefore consistent with testing these policies against one of their most commonly stated goals.

The empirical research on EZ programs is growing (a recent review of the evidence available on the impact of EZ policies on employment is offered by Wilder and Rubin 1996). Much of the evidence available, though, comes from studies looking at the impact on employment of a single state's EZ program (e.g. Alm and Hart 1997, Boarnet and Bogart 1996, Dowall 1996, Papke 1993, 1994, and U.S. General Accounting Office 1988, Rubin 1990, and Wilder and Rubin 1988).

One problem with single program evaluations is that the external validity of their results is compromised by the wide heterogeneity of states' EZ programs. Positive or negative findings from the experience of one state are difficult to generalize to other places or times, since, for example, it is not possible to disentangle whether these results were determined by some specific policy features of the program or by its relatively low or high monetary generosity.

The body of comparative evaluations of more than one state EZ program (U.S Department of Housing and Urban Development 1986 and Erickson and Friedman 1990a, 1990b, Engberg and Greenbaum 1998, Greenbaum and Engberg –forthcoming-, Greenbaum 1998) is smaller. Difficulties in gathering data on zones' location, date of designation, program features and outcome data are obstacles to comparative research. Of such studies, HUD (1986) and Erickson and Friedman (1990a, 1990b) used outcome data retrieved through interviews with state and local officials and with business and neighborhood representatives. As a consequence, the findings from these two studies are subject to the well recognized bias due to the tendency of program officials and businesses to overestimate the job growth experienced in zone areas or to attribute any job growth to the impact of EZ incentives (e.g. Bartik and Bingham 1991, Wilder and Rubin 1996). Engberg and Greenbaum (1998), Greenbaum and Engberg (forthcoming) and Greenbaum (1998) instead used Bureau of Census data, but their focus was primarily to assess the impact of EZ programs on a wide range of housing and labor market outcomes, rather than to investigate the impact of different monetary levels or of specific program features.

This paper contributes to the body of knowledge on EZ programs in many ways. First, it develops a comparative evaluation that, for the first time in the literature, assesses the impact of EZ programs on Bureau of Census employment data while controlling for the monetary value of the incentives awarded to zone businesses. Second, it controls for specific key EZ policy features which might be responsible for a marginal increase or decrease of the observed EZ impact on local employment. The sample of the five states used in the analysis provides useful variation in these key EZ policy features. Finally, this paper broadens the EZ evaluation literature by replicating the analysis with two different econometric methods to investigate the robustness of the impact estimates. Since local areas can be designated as EZs only if they show signs of economic distress, the data to evaluate EZ programs are non-experimental and non-random by nature. This poses the challenge to control for selection bias in assessing the impact estimates. The two econometric methods adopted in this paper address the selection bias issue in two different ways. The first method exploits the panel nature of the data by including in the regression model an area-specific fixed effect and an area-specific growth rate to control for the possibility that faster (or slower) growing areas were targeted by enterprise zone programs. The second method instead estimates the designation probability of each area based on pre-designation characteristics. Differences in these characteristics are then controlled for by including the predicted probability of zone designation in a regression of employment growth on indicators of zone status and program features.

The results of this paper show that the EZ programs analyzed do not have a significant impact on local employment. These results are insensitive to the monetary value of the incentives provided by the programs or to their specific policy features. Also, no significant impact is detected when the effectiveness of EZ programs is tested on single-industry employment figures instead of on total employment. The conclusion that these EZ programs do not affect the employment growth in zones and their immediately surrounding communities is robust across the two econometric methods and survives an extensive sensitivity analysis that ranges from alternative specifications to additional regression methods.

The reminder of the paper is organized as follows. Section 2 describes the EZ policy features analyzed. Section 3 illustrates the data used in the regressions. Section 4 presents the econometric methods. Section 5 presents the results of the econometric analysis. Section 6 describes the features and the results of the sensitivity analysis. Section 7 contains concluding remarks.

2. POLICY FEATURES OF ENTERPRISE ZONES

The focus of the policy debate on enterprise zones is whether these development tools are effective in arresting and reversing economic decline. The diversity of state policies provides a unique opportunity to estimate the impact of different EZ programs on local employment, controlling for some key program features that might lead to larger or smaller stimulation of employment. The specific program features investigated in this paper are summarized in Table 1.

2.1.Monetary Value of Incentives: The “Hypothetical Firm” Approach

As noted by Fisher and Peters (1996), economic development programs will only be successful if program incentives influence the investment patterns of expanding or relocating firms. To compare the efficacy of different EZ policy designs in promoting local employment, it is crucial to properly control for the monetary generosity of the incentives awarded to zone businesses by EZ programs. Negligible success of an EZ program, in fact, can be due to a limited monetary commitment to the program by the state, instead of to ineffective program features per se. For the same reason, while controlling for the overall monetary generosity of incentives, it is also crucial to test if any single policy features of EZ programs are responsible for a positive or negative marginal impact on employment.

To develop a measure of monetary generosity of EZ incentives, we adopt the "hypothetical firm" approach developed by Fisher and Peters (1998). Fisher's and Peters's "hypothetical firm" model (the Tax and Incentive Model –TAIM-) fully incorporates both tax and non-tax incentives by adapting to economic development analysis an approach first developed to study the relationship between tax burdens and economic growth (e.g. Papke 1987, 1991, and Tannenwald 1996). The TAIM algorithm works, first, by constructing the financial and tax statements, the balance sheets, and operating ratios of various hypothetical firms, each of them representing the characteristics of a typical firm in different fast-growing industries. Each firm's liabilities in terms of federal and state taxes are then calculated based on the appropriate tax laws. As a next step, the opening of a new plant in a specific location is assumed. Appropriate changes to the firms' assets, revenues, and costs are calculated, reflecting the impact of the economic development incentives offered at the new plant location (further details on the "hypothetical firm" approach and the TAIM algorithm are provided in Fisher and Peters 1996, 1998 and Peters and Fisher 1997).

To determine the monetary value of a state's EZ incentives, we estimate the difference between the TAIM internal rate of return of the investment in the new plant made by a typical firm in an EZ area and the internal rate of return of the same investment made in a non-EZ area within the same state. This within-state differential estimate is motivated by the fact that development incentives are most likely to influence business location decisions at the margin, as tie-breakers between similar and spatially adjacent areas (e.g. Bostic 1996, Wilder and Rubin 1996 and Bartik 1991). This is because the magnitude of the variation in labor, tax, and other business costs, as well as of the variation in revenue potential, across different regions and states is larger than the variation in development incentives. Businesses' inter-regional and inter-state location decisions are primarily determined by these fundamental cost and revenue variations (Wilder and Rubin 1996). Although EZ incentives are very unlikely to influence businesses' location decisions across states, they might influence businesses' location decisions between similar areas within the same state.

TAIM estimates are two-digit SIC industry specific. To utilize TAIM estimates in assessing the impact of EZ programs on total employment, a non-industry specific index has to be constructed. Evidence from other studies, reviewed by Wilder and Rubin (1996), shows that existing zone businesses are more likely than others to take advantage of EZ incentives. To combine the TAIM industry specific figures into a single incentive value estimate per each EZ program, we weighted each two-digit SIC industry specific estimate by the state proportion of establishments operating in that same industry prior to the EZ program start.

2.2. Enterprise Zones Designation Process

States' EZ policies adopt a variety of requirements to select the areas to be designated as EZs. In many states, minimal thresholds (concerning unemployment, income, education levels, percentage of vacant building and population decrease) have to be met by local communities in order to be eligible for EZ designation. Eligible local communities are then required to submit a formal application for EZ designation. Finally, EZ status is typically awarded to a sub-sample of the eligible local communities that applied for EZ designation. A distinctive feature of the designation process that has been regarded as potentially important in promoting business attraction (Bostic 1996) is the requirement of a strategic development plan. California program officials, interviewed by Bostic (1996), for example, claimed that the strategic planning portion of the application process was important to organize local development resources in a more productive way. This requirement appeared to them to be beneficial by itself for local economic development, even apart from the actual designation of an area as EZ.

As pointed out by Wilder and Rubin (1996), the body of knowledge about EZ programs fails to address the planning process as a policy implementation requirement: "The failure to include planning process requirements in state legislation that authorizes enterprise zones may critically influence program outcomes; yet this issue is generally not addressed in major studies of the effects of enterprise zones" (Wilder and Rubin 1996:474). Therefore, the first policy feature examined is the presence of a strategic planning requirement in the zone designation application process. This controls for the marginal impact on employment due to a better organization and coordination of the various local resources aimed to promote local business growth.

2.3. Business Requirements for Receiving Enterprise Zone Incentives

State EZ programs often tie incentives awarded to EZ businesses to specific requirements. The two most common requirements of this type are provisions that tie EZ incentives to the number of new jobs created by zone firms and provisions that tie EZ incentives to the size of firms' capital investment in the zone. As pointed out by Papke (1993), EZ incentives also may have an impact on factor prices. Incentives that reduce the price of capital goods may increase production and employment by lowering costs, but they may also have a substitution effect by inducing businesses to substitute labor for more capital. Programs that tie incentives awarded to EZ businesses to the number of new jobs created, therefore, might be more effective in promoting local employment growth than the programs that tie incentives to capital investments. Wilder and Rubin (1996) suggested that the state programs that do not tie their major incentives directly to employment may compromise the objective of inducing employment growth in the targeted areas. Thus, job and capital requirement variables are introduced in the analysis to control for any potential substitution effect induced by EZ programs that more heavily subsidizes capital over labor.

2.4. Portion of State Land Covered by Zones

In previous EZ studies (Brintnall and Green 1988 and Erickson and Friedman 1990) it has been suggested that EZ programs might be more successful if they restrict the number of zones. A more competitive zone selection process might allow program officials to better evaluate the potential comparative advantage of the eligible areas. In this way, program officials would be able to designate the areas that have developed the strongest local support for economic growth. Thus, as also pointed out by Wilder and Rubin (1996), state programs should designate a limited number of EZs, and award EZ status as part of a careful strategy to confer comparative advantages on certain areas. A more conservative attitude in the designation process is also considered beneficial to facilitate closer monitoring and evaluation of the implementation of the program, allegedly improving its ultimate efficacy (Wilder and Rubin 1996).

To control for the degree to which EZ impact is affected by the extent of EZ coverage, we construct a policy variable that measures the percentage of a land within a state covered by EZ areas. This variable is time varying, since the number of EZ areas increase over time in every state.

3. DATA

The data we use in this paper were collected from various documents and sources provided by states EZ program and economic development offices, the Census Bureau and the Department of Housing and Urban Development.

Zone location and designation date information are obtained from interviews and questionnaires from state EZ and local development administrators. ZIP code areas were encoded as EZ ZIPs if they encompass any portion of an actual EZ area. Table 2 provides the programs' starting dates and the number of EZs and zone ZIPs tabulated by state.

Employment information, detailed at the ZIP code level, comes from special Census Bureau tabulations of the Standard Statistical Establishment List (SSEL). These tabulations provide annual counts (from 1981 to 1994) of establishments categorized by ZIP code and cross tabulated by four-digit SIC and employment class size. Employment estimates are obtained for each two-digit SIC code industry by multiplying each establishment count times the midpoint of the corresponding employment class size and then summing across the size classes.[1] Since some ZIP code areas change over time (e.g. new ZIPs created from splitting originally larger ZIPs or old ZIPs discontinued and absorbed into other existing ZIPs), we only retain the ZIP code areas that appear in the tabulations for every year in the interval 1981-1994. To further limit the data noise coming from the joins and the splits of ZIP code areas over time, observations with extreme increases or declines in yearly employment growth (falling in the 1st and 99th distribution percentiles) are also excluded from the analysis.[2]

Pre-designation demographic, income, poverty, unemployment and population density information are from the 1980 Decennial Census STF3a files. These data, recorded by Census tracts, were allocated to ZIP code areas using MABLE/GEOCORR.[3] Table 3 provides the descriptive statistics of the ZIPs' pre-designation characteristics based on 1980 Census data.

4. ECONOMETRIC METHODS

To test the robustness of the impact estimates, we implement the analysis adopting two distinct econometric approaches and a variety of different specifications. The two econometric approaches differ from each other in the way they handle the selection bias arising from the non-experimental and non-random nature of the data. If EZ status were randomly assigned to local areas, simply comparing the performance of the experimental and the control group could estimate the program impact. Of course, the actual assignment of EZ status is based on economic performance, since virtually every state EZ program defines economic eligibility criteria for zone designation (U.S. Department of Housing and Urban Development 1992). The selection bias problem that would arise through evaluating the impact of EZ programs by comparing zones' performance to non-zones' performance is widely acknowledged (Papke 1993, Engberg and Greenbaum 1998, Boarnet and Bogart 1996). Direct evidence of this problem is detectable from

[1] Distortions of the employment estimates obtained with this approach are limited by the fact that SSEL establishment counts are tabulated in narrow employment size classes up to 50 employees. Since, as pointed out by Wilder and Rubin (1996), most of the evidence available on EZ programs shows that new jobs are within firms with fewer than 50 employees, chances of obtaining noisy estimates are drastically reduced when, as in this paper, yearly employment growth is used as dependent variable of the regressions.

[2] An unreasonable sharp yearly decline in ZIP employment is a very strong indicator that a ZIP might have been split into two (or more) ZIPs. On the other hand, an unreasonable increase in ZIP employment is a strong indicator that other ZIPs have been discontinued and absorbed.

looking at the pre-designation zone and non-zone characteristics reported in Table 3: mean unemployment rate, proportion of black or Hispanic population, per capita income, poverty rate, and population density are all significantly higher in zone areas than in non-zone areas. This strongly suggests that a simple comparison between the performances of the two groups of areas could be very misleading.

In the remainder of this section, we discuss the econometric modeling issues that present challenges to this analysis. First, we describe the two econometric methods that we use to address the non-random assignment of zone status – the random growth rate approach and propensity score approach. The section concludes with a preview of the large variety of specifications that we estimate with our two methods as a further check of the robustness of our results.

4.1 Random Growth Rate Approach

The first approach is based on the random growth rate model elaborated by Heckman and Hotz (1989) and applied to an evaluation of the Indiana EZ program by Papke (1993, 1994) and to an evaluation of the New Jersey EZ program by Boarnet and Bogart (1996). This approach takes advantage of the availability of panel data in years before and after designation periods for both zone and non-zone areas. With the availability of this kind of data alone, econometric models that allow for EZ designation to be correlated with unobservables affecting job growth can be estimated.

The random growth rate model that we use in this study,

$$\mathrm{Ln}\ Y_{it} = \alpha_i + \beta_i t + \varphi_t + \delta EZ_{it} + \lambda EZ_{it} * pol_{it} + \varepsilon_{it}, \qquad (1)$$

includes both ZIP-specific fixed effects and ZIP-specific growth rates to allow estimates to be robust against two different types of sample selection. The ZIP fixed effects, α_i, account for time-invariant differences across ZIP areas that might be correlated with EZ status (e.g. low income areas or areas with other particular characteristics of the labor force might be preferentially targeted for zone designation). The ZIP-specific growth rates, β_i, allow each ZIP area to grow at a different rate. These ZIP-specific growth rates are free to be correlated with EZ status (e.g. areas with a slow pre-designation growth rate might be targeted for zone designation). The model includes dummy variables for each year, φ_t, to capture common influences on all ZIP areas. EZ_{it} is a zone

[3] MABLE/GEOCORR, a geographical correspondence engine that determines the degree of overlap between different spatial units, is available on the World Wide Web at http://plue.sedac.ciesin.org/plue/geocorr/.

status variable that equals 1 if ZIP *i* is a zone in year *t* and zero otherwise.[4] Pol_{it} is one policy variable among those of Table 1.[5]

Equation (1) is then first-differenced to eliminate the fixed effects α_i. The resulting equation,

$$\Delta Ln\ Y_{it} = \beta_i + \Delta\varphi_t + \delta\Delta EZ_{it} + \lambda\Delta(EZ_{it}{*}pol_{it}) + u_{it}, \qquad (2)$$

still contains the ZIP-specific effect β_i. To eliminate β_i, equation (2) is estimated by the standard with-in estimator that first subtracts the ZIP-specific means from each observation. The error term in equation (2), u_{it}, is equal to the first differences of the original error term in equation (1): $u_{it} = \Delta\varepsilon_{it}$.

4.2 Propensity Score Approach

The second approach that we adopt in this study makes use of pre-designation ZIP characteristics to address the selection bias problem. With this approach, systematic differences between zone and non-zone ZIPs that might affect local employment are controlled for by first estimating the probability of zone designation as a function of a number of pre-designation ZIPs' characteristics. The predicted probability of zone designation (propensity score) is then added to the regressions of local employment growth on EZ status and on the policy variables. The propensity score approach to evaluation with non-experimental data has been developed by Rosenbaum and Rubin (1983, 1984) and recently adopted in a variety of empirical studies (e.g. Engberg and Greenbaum 1998, Dehejia and Wahba 1998 and Heckman, Ichimura and Todd 1997).

The propensity score model that we use in this study first estimates a separate probit regression for each state included in the analysis. Each probit regression (equation 3) expresses zone designation as a function of the six pre-designation economic and demographic variables of Table 3 and as a function of the employment growth in the two-year period prior to the start of the EZ program. This last variable is added to equation (3) to specifically control for the possibility that EZ programs targeted fast (or slow) growing areas:

[4] As noted by Papke (1993), including one single EZ status variable imposes the restriction that zone designation has the same impact on employment in each year after designation. A specification that replaces the single EZ status variable with a set of dummy variables that indicate the age of the EZ was also estimated. Its specific features are discussed in the sensitivity analysis section of the paper. The specification of equation (1) is described here, instead of the less parsimonious specification, because it yielded estimates of the same magnitude and significance but with smaller standard errors.

$$P(EZ_i=1) = \Phi\ [\gamma_1 Ln(Y_{it*}/Y_{it*-2}) + \ X_i\ \gamma_2]. \quad (3)$$

In equation (3), EZ_i equals 1 if ZIP *i* is ever a zone in any year from 1981 to 1994, and 0 otherwise; *t** is the starting year of the EZ program, Y_i is the employment level of ZIP *i*; and X_i is the set of variables listed in Table 3.[6]

The predicted probabilities from equation (3) are then included in a regression (equation 4) of yearly employment growth rates on a set of year dummy variables, φ_t, on an EZ status variable (EZ_{it}),[7] and on an interaction term between one policy variable (*pol*) and the EZ status variable:[8]

$$\Delta Ln\ Y_{it} = \beta_j + \phi_j\ PR^j_i + \varphi_t + \delta EZ_{it} + \lambda(EZ_{it} * pol_{it}) + u_{it}. \quad (4)$$

The predicted probabilities from equation (3) are inserted in equation (4) as a set of five propensity score variables (PR^j_i, j = CA, KY, NY, PA, VA). Each of these variables equals the predicted probability of equation (3) for all the ZIPs located within the indicated state, and 0 for all the other ZIPs. The equation also includes an intercept, β_j, that varies by state.

The fundamental difference between this specification and the random growth rate specification is the way in which ZIP-specific growth rates are represented. In this specification, ZIP-specific growth rates are represented by the propensity score function of observable ZIP characteristics. The ZIP-specific growth rate, β_i, that was included in the random growth specification (equation 2) is replaced with a linear function of the propensity score, which, in turn, is a function of observable ZIP characteristics. The difference between β_i and the function of the propensity score is an error term, ν_i, that includes unobservable ZIP characteristics:

$$\beta_i = \beta_j + \phi_j\ PR^j_i + \nu_i. \quad (5)$$

---

[5] Some of the actual specifications estimated in this paper (illustrated in section 4.3) will include two interaction terms of the form: $EZ_{it}*pol_{it}$.

[6] The unrestricted probit specification of equation (3) (i.e. one separate regression for each state) was preferred to two more restricted specifications, one with pooled data across the five states and one with pooled data but with the inclusion of state dummy variables. The choice in favor to the unrestricted model of equation (3) is supported by a likelihood ratio test.

[7] We also estimated a specification including a set of EZ age dummy variables, instead of a single EZ status variable, as we did for the random growth rate model. The specific features from this specification are discussed in the sensitivity analysis section of the paper.

[8] Some of the actual specifications that we estimate in this paper (illustrated in section 4.3) will also include a second interaction term of the form: $EZ_{it}*POL_{it}$.

If ν_i is uncorrelated with EZ designation, then the propensity score specification will yield consistent estimates of EZ impact and will be more efficient than the random growth specification. Otherwise estimates of the propensity score specification will be inconsistent.

We believe that the assumption of uncorrelated unobservables is credible for two reasons. First, the pre-designation characteristics included in the propensity score regressions cover virtually all the variables mentioned in the state program legislations as zone designation criteria. Thus, it is very likely that these variables capture all the important factors driving state administrators' designation decisions. Second, the zone designation process leaves very little room for self-selection by participants based on unobservable variables. In Dehejia and Wahba (1998), a propensity score approach worked very well in evaluating programs with much more severe selection on unobservables due to a larger role played by self-selection by participants.

Of course, the observable ZIP characteristics could be entered directly into the employment growth equation (4) rather than including the propensity score summary of these characteristics. However, as argued by Rosenbaum and Rubin (1983), the propensity score constitutes a convenient way to deal with non-linearities in the relationship between employment growth and pre-designation ZIP characteristics. Including in equation (4) the set of pre-designation variables of equation (3) in place of the propensity scores would require a correct specification of the functional form of the relationship between the pre-designation variables and the employment growth rate. This is difficult to achieve since economic theory does not provide guidance in this matter. As noted by Engberg and Greenbaum (1998), this is particularly true for EZ studies where zones are generally a small and very peculiar portion of the sample investigated. Rosenbaum and Rubin (1983) demonstrate that conditioning on the propensity scores corresponds to conditioning on the correct functional form of the pre-designation variables in a direct regression of employment growth on the pre-designation variables.[9]

The propensity score method that we implement also lends itself to modifications of the sample and estimation method that will improve the impact estimates. As shown by Dehejia and Wahba (1998) and Heckman, Ichimura and Todd (1997), for program evaluation it is crucial to limit the sample to observations with propensity scores for which there are both treatment and

[9] Heckman, Ichimura and Todd (1998) recently argued that the Rosenbaum and Rubin (1983) claim does not necessarily hold when the propensity score must be actually estimated, as in the model of equation (3) and (4). In their view, instead, the primary advantage of using the propensity score in such cases lies in simplicity of estimation. When conditioning on the propensity scores, one can estimate treatment effects in two stages, first modeling the program participation decision (as in equation 3), and then describing the outcome model (as in equation 4). The conceptual and mechanical advantages of a two-stage estimation approach is similar to the case of the conventional econometric approach using Mills ratios to correct for selection bias.

comparison observations. In this case, areas with a very low designation probability are not a valid comparison match for any zone area, since their initial demographic and economic conditions are far better than the zone areas. On the other hand, areas with a very high designation probability are almost impossible to match with any non-zone areas due to their initial overly distressed demographic and economic conditions. Dehejia and Wahba (1998) demonstrate that limiting the estimation sample based on the propensity score values yields much better impact estimates than using all the observations contained in the comparison group or limiting the estimation sample based on some particular pre-designation characteristics. To restrict the estimation sample on the basis of propensity score values, we exclude from the analysis the ZIP areas for which their propensity score falls within the 1st percentile of the zone ZIPs' distribution or within the 99th percentile of the non-zone ZIPs' distribution. Although a similar sample restriction could be implemented for the random growth model, we follow the customary practice of including all observations from the treatment and comparison samples.

The traditional least squares estimation method for both the propensity score and random growth specifications is sensitive to the presence of outliers. Although the sample can be modified, as we have done, to eliminate the extreme tails of the dependent variable distribution, there remains the possibility that some observations will exert tremendous influence on the impact estimates. There are alternative estimation methods such as median regression or iterative methods that re-weight the data to reduce the influence of outliers. Although these methods could be implemented for the random growth specification, the large number of parameters makes them unwieldy. The parsimony of the propensity score method lends itself to implementing these methods. We estimate the propensity score specification (equation 4) with an iterative reweighting method.[10]

A final difference between the specifications is that the propensity score equation (4) specifies that EZ designation affects the employment *growth rate* whereas the random growth equation (1) specifies that EZ designation affects the employment *level*. This difference could be easily removed by including changes in EZ status in the propensity score specification (4) or by including the EZ status dummy in the estimated form of the random growth specification (2). However, economic theory provides little guidance as to the proper form. Therefore, we retain the different approaches and add this to the catalog of ways in which the two specifications differ.

[10] Equation (4) is estimated with the StataCorp. (1997) robust regression algorithm to correct for the outlier-sensitivity deficiency in ordinary regression. This algorithm first calculates the Cook's distance (D) and eliminates the gross outliers for which D>1. Then it works iteratively by performing a regression, calculating case weights based on absolute residuals, and regressing again utilizing those weights. The iteration stops when the maximum change drops below a tolerance level.

It should be noted that there are a number of other approaches to modeling the non-random assignment of enterprise zones. We have implemented two methods from opposite ends of the spectrum. The random growth model allows each area to have its own underlying growth rate and requires no restrictive assumptions about independence of these growth rates and the assignment of enterprise zone status. The propensity score approach, at the other extreme, represents the underlying growth rate as a function of the area's observed characteristics and assumes that unobserved characteristics that affect area growth are unrelated to zone status.

In between these extremes are other options for modeling the relationship between zone status and underlying growth. Instrumental variable methods require a subset of variables that affect zone status but do not have a direct effect on the underlying growth of the area. Unfortunately, such variables are practically impossible to find since zone areas are designated based on the same pre-existing economic, social and physical conditions that are among the major factors affecting the post-treatment employment outcome of interest for the evaluation.[11] Mills ratio methods, developed by Heckman (1976) and Lee (1978), are another alternative. They rely on assumptions regarding the shape of the joint distribution of the error terms from the growth equation and the enterprise zone status equation and typically use exclusion restrictions similar to instrumental variable methods in order to obtain robust estimates (see, for example, Robinson, 1989). Without a useful set of exclusion restrictions, we are left with our two approaches. In sum, we have the random growth approach which makes a minimum amount of assumptions and the propensity score approach which gains precision by making more restrictive assumptions.

4.3 Model Specifications

Both the random growth rate and the propensity score methods are used in this study with a number of different specifications. These specifications differ along two dimensions. The first dimension concerns the type and number of interaction terms ($EZ_{it}* pol_{it}$) that are included in the estimated equations (2 and 4). A first specification (I) estimates zone impact on local employment by controlling only for the monetary value of the program incentives offered to business. Thus, specification I is constructed by substituting the generic interaction term $EZ_{it}* pol_{it}$ of equations 2 and 4 with the term $EZ_{it}* mon_{it}$.

[11] To evaluate the New Jersey EZ program, Boarnet and Bogart (1996) estimate a specification that imbeds an instrumental variable estimator within the random growth framework. They instrument EZ status with pre-designation economic and demographic area characteristics. Consistency requires such pre-existing area characteristics affect zone designation but have no direct impact on the area's employment growth rate. We prefer not to impose this assumption in our analysis.

To control for both the monetary value of incentives and other program policy variables, a set of four additional specifications is used. Each of these specification includes two interaction terms in place of the generic term EZ_{it}* pol_{it}. The first term in all four specifications is EZ_{it}* mon_{it}, while the second is the interaction between EZ status and one policy variable. These policy variables, as listed in Table 3, are: business plan –*buspl*- (in specification II); tax incentives tied to job creation –*job*- (in specification III); tax incentives tied to new capital investment –*cap*- (in specification IV); land coverage of the program –*land*- (in specification V). No more than two policy interaction terms are included in each of these specifications because of the limited variation of the policy variables across the data sample. The complete set of policy interaction terms included in specifications (I-V) is summarized in Table 4.

The second dimension that differentiates the model specifications involves an analysis of employment in specific industries. To check whether zone impact is particularly concentrated in some specific industry, we replicate the analysis of specifications (I-V) using single two-digit SIC industry employment figures instead of total employment figures. Wilder and Rubin (1996), reviewing the evidence of other EZ studies, find that new employment is heavily concentrated in manufacturing and wholesale/retail trade. To test this hypothesis we selected five manufacturing two-digit SIC industries among those with both a large number of establishments contained in the data set and an available specific TAIM estimate of the monetary value of the incentives. These five industries are: food and kindred products (SIC 20); lumber and wood products (SIC 24); printing and publishing (SIC 27); fabricated metal products (SIC 34); industrial machinery and equipment (SIC 35). For each specific SIC industry, the analysis is carried out utilizing the specifications (I-V) with both the random growth rate and the propensity score methods.

5. RESULTS

The results from the random growth rate and the propensity score models of equations (1-4) are illustrated in Tables 5-10. Before turning to the impact estimates, we will briefly examine the location of zones as illustrated by the propensity score probit.

5.1. Zone Placement

Table 5 reports the results from the probit regressions of equation (3). The coefficient estimates of Table 5 highlight the more important pre-designation characteristics of the ZIP areas that lead to zone designation. The importance of these characteristics varies considerably across the states. All states but Virginia tend to designate areas with an high proportion of minority population, while all states but New York favor densely populated areas. Kentucky and Virginia target areas with a high unemployment rate. The California and Pennsylvania programs are directed toward areas with a high poverty rate, while Virginia designates areas with lower per capita income. High school drop-out rates and employment growth prior to the beginning of the EZ program do not have independent impacts on designation in any state. This last finding, in particular, is contrary to the concerns of Boarnet and Bogart (1996) and Papke (1994) that EZ programs might target fast or slow-growing areas.

The overall picture that emerges from Table 5 is that all states target some type of economic hardship for zone designation. The predicted probabilities from the probit regressions of Table 5 confirm this conclusion. Table 6 presents the descriptive statistics of these predicted probabilities sorted by state and by EZ status. In all the five states, non-zone ZIPs have considerably lower propensity scores than zone ZIPs, reflecting more prosperous pre-designation characteristics.

5.2. Impact of Zones on Local Employment

The random growth and the propensity score impact estimates are summarized in Tables 7 and 8, respectively. The random growth impact estimates of Table 7 can be interpreted as the permanent marginal shift, in percentage points, of the employment level, while the propensity score impact estimates of Table 8 give the percentage point marginal contribution to the base employment growth rate. In both the random growth rate and the propensity score results, the EZ status coefficient estimates are not significantly different from zero, at a 0.1 level, across all of the five specifications used to implement the analysis. For each of the specifications (I-IV) reported in Table 7 and 8, the impact of EZ status on employment growth is precisely estimated to be close to zero.

Taking the results of specification I as an example, the random growth rate estimates imply a one time permanent increase in the employment level of 0.86% due to EZ designation.[12] Figure 1 illustrates this estimated increment in the context of expected employment growth in zone and non-zone areas. The left portion of the graph shows the difference in average growth rates between areas that later are designated zones and areas that do not receive zone designation. On average, areas that later became zones grew 0.6% per year between 1981 and 1984. Areas that never became zones grew 1.5% per year during this period. Extrapolating these growth rates into the zone era, a non-zone area that had 1000 employees in 1981 is expected to have 1223 employees by 1994. Extrapolating the growth rate for zone areas provides an estimate of the 1994 employment level in the absence of a zone program. A zone area that had 1000 jobs in 1981 is expected to have 1081 jobs by 1994. Our impact estimate suggests that the zone program adds 9 jobs for an expected zone employment of 1090 in 1994. These 9 jobs amount to 6.3% of the employment gap that would have existed in the absence of a zone program. Clearly, the zone program is estimated to have had very little impact on the difference between zone and non-zone employment growth.[13]

The impact estimates from the propensity score approach are even less encouraging. The point estimate indicates that zone designation *lowers* the annual employment growth rate by 0.09 percentage points.[14] Figure 2 illustrates the magnitude of this estimate. The estimated reduction in the growth rate implies that the zone program reduced zone employment by 10 jobs at the end of ten years.[15]

For both the random growth rate and the propensity score results, the coefficient estimates on the interaction term between EZ status and the monetary value of the incentives are also not statistically significant. For the random growth rate results of Table 7, the coefficient of the monetary value of the incentives has the expected positive sign. The magnitude of this estimate, however, is quite small. At most (i.e. considering the point estimate of specification V), increasing the monetary value of the incentives by fifty percent would shift the employment level only by a range of 0.18 - 1.12 percentage points. For the propensity score results of Table 8, the point estimates have the wrong sign but the magnitude of these estimates is virtually zero. A fifty percent increase in the generosity of the incentives would depress the base growth rate, at most (i.e. considering the point estimates of specifications I and IV) by a range of 0.05-0.33 percentage

---

[12] This figure is obtained when the monetary value of the incentives offered to zone businesses is set at the level of Kentucky (i.e. the median value in the data sample, Table 1): 0.86=[0.004+0.217*0.021]*100. This estimate has a standard error of 1.6.

[13] The standard error given in the previous footnote implies that the estimated impact of 9 jobs after ten years has a 95% confidence interval ranging from –9 jobs to 26 jobs.

[14] The estimated impact of zone designation is negative because of the estimated coefficient for the monetary value of incentives is negative. The negative figure of 0.09 percentage points is obtained when the monetary value of the incentives is set at the level of Kentucky [0.09=(0.001+0.217*-0.009)*100]. This estimate has a standard error of 0.8.

[15] The standard error given in the previous footnote implies that the estimated impact of –10 jobs after ten years has a 95% confidence interval ranging from –95 to 81 jobs.

points.[16]

The point estimates of the interaction terms between the EZ status and the policy variables (submission of a business plan $EZ_{it}*buspl_i$; hiring requirements $EZ_{it}*job_i$; and capital requirements $EZ_{it}*cap_i$) are also close to zero and non-significant in both the random growth rate and the propensity score results. The point estimate of the interaction between the EZ status and the program land coverage ($EZ_{it}*land_{it}$) has the expected negative sign although it, too, is not statistically significant. The point estimate indicates that a larger portion of the state covered by zones reduces the impact of each zone on local employment. The magnitude of the term ($EZ_{it}*land_{it}$) is such that increasing the percentage of land occupied by EZs from the smallest to the largest value in the data sample (from 0.6% to 12%, Table 1) would reduce the impact of zone incentives by 1.86 percentage points [= (-0.155*(0.12-0.06))*100] for the random growth results of Table 7, and by 1.37 percentage points [= (-0.121*(0.12-0.06))*100] for the propensity score results of Table 8.

Table 8 also reports the estimated coefficients on the predicted zone designation probabilities that are included to control for pre-designation area characteristics. These estimates indicate that area characteristics have a large and significant impact on subsequent growth in all states except Kentucky. These estimates all have the expected negative sign: higher initial economic and social distress, reflected in a higher propensity score, predicts lower subsequent employment growth. The magnitude of these estimates is fairly large. For example, in California the difference in average propensity scores between zone and non-zone areas implies a growth rate that is 1.79 percentage points lower in the zone areas. The zone/non-zone differences in New York, Pennsylvania and Virginia are 0.75, 1.14 and 0.78, respectively.[17]

Tables 9 and 10 report the results for the industry specific analysis. In each of these two tables, the coefficient estimates of the EZ status and the policy interaction variables are tabulated

[16] As reported in Table 1, California has the lowest monetary value of EZ incentives. It has a spread of 0.115 percentage points in the difference between the internal rate of return (IRR) of an investment in zone areas and the IRR of the same investment in non-zone areas. Virginia has the highest value of the same measure with a spread of 0.735 percentage points. A fifty percent increase on the California level would correspond to a maximum change in employment of 0.18 percentage points [=((0.115/2)*0.032)*100] in the random growth rate results and to a maximum negative change of 0.05 percentage points [=((0.115/2)*-0.009)*100] in the propensity score results. The same increase on the Virginia level would correspond to a change of 1.12 percentage points [=((0.735/2)*0.032)*100] for the random growth rate results and to a negative change of 0.33 percentage points [=((0.735/2)*-0.009)*100] for the propensity score results.

[17] From Table 6, the mean propensity score for the California zone ZIPs is 0.275, while the mean propensity score for the non-zone ZIPs is 0.069. The difference in these numbers multiplied times the coefficient on $prCA_i$ in Table 8 yields a negative difference of 1.79 percentage points [=((0.275-0.069)*-0.087)*100]. The impact of pre-designation characteristics is calculated in a similar fashion for the other states.

by the type of specifications (I-V), and by the two-digit SIC code industries of the employment figures used as the dependent variable of the regression.

The industry-specific results are very similar to the total employment results of Table 7 and 8. None of the reported coefficient estimates are statistically different from zero. The magnitude of most of the coefficient estimates are close to zero, while all the estimates with a larger magnitude in one of the econometric approaches (random growth rate or the propensity score) are not consistently replicated across the results of the two methods.

6. SENSITIVITY ANALYSIS

The robustness of the results that are reported in Tables 7-10 is examined by replicating the analysis with a variety of different regression methods, specifications and sample selection rules. The zero-impact of zone designation and the insignificance of the various policy variables prove to be extremely robust findings, withstanding the challenge of the entire range of the sensitivity analysis performed.

The alternative regression methods with which we replicate the analysis are techniques to limit the impact of outliers on the estimates. We replicate the analysis of the propensity score model by estimating equation (4) with median regression and with Tobit regression. In the Tobit regression, we censor the values of the employment growth variable at the 1^{st} and the 99^{th} percentile of the original distribution. We also re-estimate the random growth rate model with median regression and with the iterative re-weighting method which we earlier applied to the propensity score model. In both these cases, we first express the variables in equation 2 as deviations from their ZIP-specific means in order to remove the ZIP-specific growth rates.

We also estimate specifications that vary the number of EZ status and interaction terms included in equations (1, 2 and 4), and the number of independent variables included in the probit model of equation (3). These alternative specifications are illustrated, for the random growth approach, by equations (5) and (6):

$$\text{Ln } Y_{it} = \alpha_i + \beta_i t + \varphi_t + \delta EZ\_1_{it} + \delta_1 EZ\_2_{it} + \delta_2 EZ\_3_{it} + \delta_3 EZ\_4_{it} + \lambda(EZ\_1_{it}*pol_{it}) + \lambda_1(EZ\_2_{it}*pol_{it}) + \lambda_2(EZ\_3_{it}*pol_{it}) + \lambda_3(EZ\_4_{it}*pol_{it})] + \varepsilon_{it} \quad (5)$$

$$\Delta \text{Ln } Y_{it} = \beta_i + \Delta\varphi_t + [\delta\Delta EZ\_1_{it} + .. + \delta_3 \Delta EZ\_2_{it}] + +[\lambda\Delta(EZ\_3_{it}*pol_{it}) + .. + \lambda_3\Delta(EZ\_4_{it}*pol_{it})] + u_{it}. \quad (6)$$

In equations (5) and (6), zone status is represented by a set of four EZ status variables (EZ_1_{it} …EZ_4_{it}) that reflect the age of the enterprise zone. EZ_1_{it} equals 1 if ZIP *i* in year *t* is a zone in its first year of existence and zero otherwise. EZ_2_{it} and EZ_3_{it} indicate whether the zone is in the second or third year of its existence, respectively. EZ_4_{it} equals 1 if ZIP *i*, in year *t*, is a zone that has been designated for four or more years. The specification of equations (5) and (6) is less restrictive than equation (1) and (2) because it allows for zone impact on employment to change over time, while the specification of equation (1 and 2) requires zone designation to cause a permanent shift in the employment level at the time of designation. In a similar fashion, the propensity score method is re-estimated using the EZ age dummies, permitting the impact of zone designation on the employment growth rate to vary with the age of the zone.

The alternative specification for the zone designation probit is illustrated in equation (7):

$$P(EZ_i=1) = \Phi[\alpha_1(t\_est_i)+\alpha_2(s\_est_i)+\alpha_3(m\_est_i)+\alpha_4(l\_est_i)+X_i\ \beta]. \qquad (7)$$

Equation (7) adds four variables measuring the growth of establishments of various sizes to the six pre-designation economic and demographic variables previously included. These growth variables are constructed as the 1981-1983[18] growth of the total number of establishments (t_est_i), and of the number of establishment: with less than 20 employees (s_est_i); with 20-249 employees (m_est_i); and with 250-1000 employees (l_est_i). This modification allows us to examine whether program officials specifically targeted areas with lower (or higher) growth in the number of small business establishments, as opposed to areas with a decrease (or increase) in the number of medium-to-large production plants. Estimation of equation (7) yields coefficients on all four establishment growth variables that are insignificant, while the coefficients on Census variables have the same sign and similar magnitude to those reported in Table 6.

In a final set of replications, we alter the sample selection rules that govern which observations are excluded due to missing data and outliers. We estimate both models *without* excluding the ZIP code areas that have missing years of data during the observation period. We also vary the outliers-exclusion thresholds from the 1st and 99th percentiles to the 5th and 95th percentiles of the employment growth distribution.

The number of regressions produced by all of these variations is enormous. Rather than provide the results in tabular form, we briefly summarize their results. In short, our conclusion

[18] The period 1981-1983 is chosen to be immediately before the start of the first EZ program (i.e. Pennsylvania, Table 2) in the data sample.

regarding the negligible impact of these enterprise zones on employment growth is unaffected by the modeling and estimation choices represented in these sensitivity analyses. The impact of enterprise zones is small and not statistically significant in virtually all of these estimates.

7. CONCLUSIONS

This paper performs a comparative evaluation of the impact on local employment of a set of state EZ programs. We control for the monetary value of the incentives awarded to zone businesses and for other key EZ policy features. The results of the analysis show that the EZ programs analyzed (California, Kentucky, New York, Pennsylvania and Virginia) do not have a noticeable impact on the employment growth of the local neighborhoods immediately surrounding the zone areas. This conclusion is robust across two econometric methods. Furthermore, the zero-impact result withstands a wide range of sensitivity analyses. The findings of this paper are in line with some recent single-state evaluations (e.g. Dowall 1996, for California and Boarnet and Bogart 1996, for New Jersey) that reported a negligible impact of zones on local employment.

There are a variety of explanations for the lack of impact. For example, EZ programs might have a direct impact on the employment growth within the strict boundaries of the zone areas, but this might be offset by employment loss in same ZIP codes immediately surrounding the zone areas. However, one of the commonly stated goals of EZ programs is to promote economic growth of distressed local communities. Thus, one might argue that the programs only succeed if they increase employment in an area that includes a small buffer around the zone. Another possible explanation for the lack of impact is that EZ programs might promote new business start-ups that drive away existing businesses already established in the zones, as suggested by the results of Greenbaum (1998). Thus, EZ programs might actually promote higher business turnover that does not result in an overall growth in the local level of employment. In this case, possible benefits of the EZ programs could be limited, for example, to the eventual gain in efficiency brought on by the higher business turnover.

The results of this paper also show that the level of the monetary value of the incentives awarded to zone businesses does not noticeably contribute toward enhancing the impact on local employment of the EZ programs analyzed. Other program features also appear to be irrelevant. The insignificance of the monetary value of the incentives contrasts with a recent stream of literature, reviewed by Bartik (1991), that finds geographic differences in tax levels to affect local economic growth. However, it appears that current EZ programs are not large enough to have a measurable impact on the within-state locations of economic activity.

REFERENCES

Alm James and Julie Ann Hart (1997), Enterprise zones and economic development in Colorado, Manuscript, University of Colorado at Boulder.

Bartik Timothy J. (1991), Who benefits from state and local economic development policies?, Kalamazoo, MI: W.E. Upjohn Institute for Employment Research.

Bartik Timothy J. and Richard Bingham (1995), Can economic development programs be evaluated, W.E. Upjohn Institute for Employment Research, Kalamazoo, MI: Staff Working Paper 95-29.

Boarnet Marlon G. and William T. Bogart (1996), Enterprise zones and employment: evidence from New Jersey, Journal of Urban Economics 40 (2): 198-215.

Bondonio Daniele (1998), States' Enterprise Zone Policies and Local Employment: What Lessons Can be Learned?, ICER, Torino, Working Paper Series, 28, 1998.

Bostic Raphael W. (1996), Enterprise zones and attraction of business and investment: the importance of implementation strategies and program incentives, Division of research and statistics, board of governors, Washington D.C: working paper.

Brintnall Michael and Roy E. Green (1988), Comparing state enterprise zone progrmas: variations in structure and coverage, Economic Development Quarterly 2 (1): 50-68.

Dehejia Rajeev H. and Sadek Wahba (1998), Causal effects in non-experimental studies: Re-evaluating the evaluation of training program, National Bureau of Economic Research Working Paper 6586.

Dowall David E. (1996), An evaluation of California's enterprise zone programs, Economic Development Quarterly, 10 (4): 352-368.

Engberg John and Robert Greenbaum (1998), The impact of state enterprise zones on urban housing markets, unpublished manuscript, Carnegie Mellon University, PA..

Erickson Rodney A. and Susan W. Friedman (1990a), Enterprise zones: 1. Investment and job creation of state government programs in the United States of America, Environment and Planning C: Government and Policy 8: 251-267.

Erickson Rodney A. and Susan W. Friedman (1990b), Enterprise zones: 2. A comparative analysis of zone performance and state government policies, Environment and Planning C: Government and Policy 8: 363-378.

Fisher Peter S. and Alan H. Peters (1996), Tax and spending incentives and Enterprise Zones, prepared for the Symposium: The effects of state and local public policies on economic development, Federal Reserve Bank of Boston MA.

Fisher Peter S. and Alan H. Peters (1998), Industrial incentives. Competition among American States and Cities, Kalamazoo, MI: W.E. Upjohn Institute for Employment Research.

Greenbaum Robert (1998), The impact of state urban enterprise zones on business outcomes, Paper presented at the 20th APPAM Annual Research Conference, New York.

Greenbaum Robert and John Engberg (Forthcoming), An evaluation of state enterprise zone policies: Measuring the impact on urban housing market outcomes, Policy Studies Journal.

Heckman James (1976), The common structure of statistical models of truncation, sample selection, and limited dependent variables and a simple estimator for such models, Annals of Economic and Social Measurement 5: 475-492.

Heckman J. and V. Hotz (1989). Choosing among alternative nonexperimental methods for estimating the impact of social programs: The case of manpower training, Journal of the American Statistical Association 84: 862-875.

Heckman J., H. Ichimura, and P. Todd (1997), Matching as a econometric evaluation estimator: Evidence from evaluating a job training Programme, Review of Economic Studies 64: 605-654.

Lee Lung-Fei (1978), Unionism and wage rates: a simultanous equation model with qualitative dependent variables, International Economic Review, 19: 415-433.

Papke Leslie E. (1993), What do we know about enterprise zones? In James M. Poterba, ed., Tax Policy and the economy, 7. Cambridge MA, MIT Press: 37-72.

Papke Leslie E. (1994), Tax policy and urban development. Evidence from the Indiana enterprise zone program, Journal of Public Economics 54: 37-49

Peters Alan .H. and Peter S. Fisher (1997), Methodological issues in the measurement of tax and incentive competition, paper presented at the Annual Meeting of the Regional Science Association, Buffalo, NY.

Robinson, Chris (1989), The joint determination of union status and union wage effects: some tests of alternative models, Journal of Political Economy, 97, 3: 639-667.

Rosenbaum P. and D. Rubin (1983), The central role of the propensity score in observational studies for causal effects, Biometrika 70: 41-55.

Rosenbaum P. and D. Rubin (1984), Reducing the bias in observational studies using subclassification on the propensity score, Journal of the American Statistical Association 79: 516-524.

Rubin Marilyn (1990), Urban Enterprise Zones: Do they work? Evidence from New Jersey, Public Budgeting and Finance 10: 3-17.

StataCorp. (1997), Stata Statistical Software: release 5.0, College Station, TX, Stata Corporation.

U.S. Department of Housing and Urban Development (1986), State-designated enterprise zones: ten case studies, Washington D.C.

U.S. General Accounting Office (1988), Enterprise Zones: lessons from the Maryland experience, Washington D.C.

Wilder Margaret G. and Barry M. Rubin (1988), Targeted redevelopment through urban enterprise zones, Journal of Urban Affairs 10 (1):1-17.

Wilder Margaret G. and Barry M. Rubin (1996). Rhetoric versus Reality: a review of Studies on state enterprise zone programs, Journal of the American Planning Association, 62: 473-491.

Table 1: Programs Features

Policy feature	*Variable name*	*Measure*	*CA* [(a)]	*KY*	*NY*	*PA*	*VA*
Monetary value of the incentive package offered to EZ businesses	Monetary value of the incentives (mon)	ΔIRR(%points) btw. EZ and non-EZ loc. within the same state[(b)]	0.115	0.217	0.183	0.229	0.735
Zone eligibility is conditional to the submission of an application complete with a business development plan for the local community	Business Plan (buspl)	1=yes 0=no	1	0	1	1	1
Tax incentives to EZ businesses are proportional to the number of new jobs created	Tax incentives tied to job creation (job)	1=yes 0=no	1	1	0	0	0
Tax incentives to EZ businesses are proportional to the amount of new capital investment	Tax incentives tied to the amount of new capital investment (cap)	1=yes 0=no	0	0	1	0	0
Total surface occupied by EZ areas as a percentage of the total State land size	Portion of State land covered by zones (land)	% land of the State occupied by EZs[(c)]	1.3 3.5	0.9 3.3	1.0 3.4	0.6 7.4	1.5 12.0

Notes:

(a) California had two EZ programs that respectively established the Enterprise zones and the Employment and economic Incentive Areas. Since the two programs did not differ from each other in the policy dimensions considered in this paper they are considered as a single program.

(b) ΔIRR values vary across industries. The figure reported is the state average obtained by weighting each two-digit SIC specific estimate by the proportion of establishments in the state operating in that industry prior to the start of the EZ program.

(c) The land coverage variable (land) is time-varying. The lower range value of the measure reflect the % of state land occupied by EZs at the beginning of the program. The upper range value reflects the same % in 1994.

Table 2: Programs Starting Dates and Zones by State

State	*Program starting date*	*Zones* [(a)]	*Zone ZIPs* [(b)]
California[(c)]	1986	29	129
Kentucky	1983	10	50
New York	1987	19	44
Pennsylvania	1983	52	134
Virginia	1984	24	90

Notes:
(a) Number of zones in existence at 12/31/1993
(b) Number of ZIP code areas encompassing any portion of EZ areas.
(c) For California, the number of zones and zone ZIPs combines the counts
for both the Enterpise Zone and the Economic Incentive Area programs

Table 3 : Descriptive Statistics of Pre-Designation ZIPs' Characteristics (1980 Census data)

Variable		*Mean*		*Std. Dev.*	
		Zone ZIPs (EZ=1)	*Non-zone ZIPs (EZ=0)*	*EZ=1*	*EZ=0*
Proportion of ZIP population black or hispanic	(blhis)	0.2745	0.0995	0.2932	0.1642
Proportion of persons 25 or older with only elementary education	(educt)	0.2455	0.2227	0.1080	0.1398
Unemployment rate	(unemp)	0.0497	0.0446	0.0183	0.0196
Per capita income (in $1,000s)	(incap)	6.0350	6.6300	1.5514	2.1298
Poverty rate	(povrt)	0.1674	0.1300	0.0982	0.0816
Population densisty	(popds)	1.87	0.80	2.88	2.90

Table 4: Model Specifications

Specification	*Controlling for:*	*Policy interactions to substitute for $EZ_{it}*pol_{it}$ in eq. (1,2 and 4)*				
(I)	Monetary value of incentives (mon)	$EZ_{it}*mon_i$				
(II)	(mon) & business plan (buspl)	$EZ_{it}*mon_i$	$EZ_{it}*buspl_i$			
(III)	(mon) & incentives tied to job creation (job)	$EZ_{it}*mon_i$		$EZ_{it}*job_i$		
(IV)	(mon) & incentives tied to new capital invest. (cap)	$EZ_{it}*mon_i$			$EZ_{it}*cap_i$	
(V)	(mon) & % land occupied by EZs (land)	$EZ_{it}*mon_i$				$EZ_{it}*land_{it}$

Table 5: Probability of Zone Designation -Probit Estimates from Equation (3)

Variable		*California*	*Kentucky*	*New York*	*Pennsylvania*	*Virginia*
Proportion of ZIP population black or hispanic	(blhis)	1.345***	8.098***	1.562***	1.777***	-0.057
		(0.328)	*(2.118)*	*(0.440)*	*(0.536)*	*(0.395)*
Prop. of persons 25 or older with only element. educ.	(educt)	-0.709	0.315	1.214	-1.509*	-0.742
		(0.662)	*(2.022)*	*(1.403)*	*(0.897)*	*(0.803)*
Unemployment rate	(unemp)	3.711	20.350**	7.881*	-5.379	23.793***
		(2.707)	*(9.657)*	*(4.141)*	*(3.916)*	*(5.924)*
Per capita income	(incap)	-0.035	0.236*	-0.034	-0.033	-0.218***
		(0.041)	*(0.136)*	*(0.073)*	*(0.051)*	*(0.062)*
Poverty rate	(povrt)	3.890***	-4.790	-0.643	3.435***	-1.943
		(1.194)	*(3.322)*	*(1.610)*	*(1.328)*	*(1.659)*
Population densisty	(popds)	0.099***	1.655***	-0.027	0.116***	0.619***
		(0.027)	*(0.263)*	*(0.019)*	*(0.040)*	*(0.135)*
Employment growth prior to program starting date	$Ln(Y_{it*}/Y_{it*-2})$	-0.033	-0.056	-0.332	-0.020	-0.047
		(0.124)	*(0.213)*	*(0.220)*	*(0.125)*	*(0.114)*
Constant		-2.241***	-4.133***	-2.415***	-1.116**	-0.316
		(0.438)	*(1.415)*	*(0.705)*	*(0.536)*	*(0.574)*
Number of observations		1377	673	1561	1412	781
Pseudo R2		0.233	0.601	0.107	0.142	0.110
Log Likelihood		-314.1	-63.8	-166.0	-380.3	-244.7

* p-value<0.1 ** p-value<0.05 *** p-value<0.01
Standard errors are in parentheses

Table 6: Estimated Designation Probabilities -Propensity Scores

State	Predicted probability: Zone ZIPs (EZ_i=1)	Predicted probability: Non-zone ZIPs (EZ_i=0)
California	0.275 (0.248)	0.069 (0.099)
Kentucky	0.594 (0.363)	0.026 (0.081)
New York	0.065 (0.072)	0.024 (0.034)
Pennsylvania	0.211 (0.229)	0.079 (0.079)
Virginia	0.194 (0.160)	0.101 (0.086)

Mean
(standard deviation)

Table 7: Random Growth Rates Estimates of Zone Status and Policy Features Impact on Local Employment

Independent variable [(a)]	*Specification*				
	(I)	*(II)*	*(III)*	*(IV)*	*(V)*
Constant	0.019***	0.019***	0.019***	0.019***	0.019***
	(0.001)	*(0.001)*	*(0.001)*	*(0.001)*	*(0.001)*
EZ_{it}	0.004	0.000	0.013	0.001	0.005
	(0.026)	*(0.051)*	*(0.037)*	*(0.027)*	*(0.025)*
$EZ_{it}*mon_i$	0.021	0.021	0.007	0.025	0.032
	(0.069)	*(0.069)*	*(0.081)*	*(0.070)*	*(0.078)*
$EZ_{it}*buspl_i$		0.004			
		(0.052)			
$EZ_{it}*job_i$			-0.013		
			(0.037)		
$EZ_{it}*cap_i$				0.019	
				(0.053)	
$EZ_{it}*land_{it}$					-0.155
					(0.482)

Number of observations: 93386
Overall R2: 0.026
Standard errors are in parentheses
* p-value<0.1 ** p-value<0.05 *** p-value<0.01
(a) For clarity of exposition the coefficient estimates of the year dummies are not reported.
The complete list of regression results is available upon request.

Table 8 : Estimates of Zone Status and Policy Features Impact on Local Employment Growth. Propensity Score Approach -Robust Regression Estimates

Independent variable [(a)]	*Specification*				
	(I)	*(II)*	*(III)*	*(IV)*	*(V)*
Constant	0.002 *(0.003)*	0.002 *(0.003)*	0.002 *(0.003)*	0.002 *(0.003)*	0.002 *(0.003)*
$prCA_i$	-0.087*** *(0.013)*	-0.087*** *(0.013)*	-0.087*** *(0.014)*	-0.087*** *(0.014)*	-0.087*** *(0.014)*
$prKY_i$	0.051* *(0.030)*	0.041 *(0.033)*	0.050 *(0.031)*	0.051* *(0.030)*	0.049 *(0.030)*
$prNY_i$	-0.185*** *(0.050)*	-0.185*** *(0.050)*	-0.185*** *(0.050)*	-0.185*** *(0.050)*	-0.186*** *(0.050)*
$prPA_i$	-0.088*** *(0.024)*	-0.087*** *(0.024)*	-0.087*** *(0.024)*	-0.088*** *(0.024)*	-0.086*** *(0.024)*
$prVA_i$	-0.084*** *(0.020)*	-0.084*** *(0.020)*	-0.085*** *(0.020)*	-0.085*** *(0.020)*	-0.085*** *(0.020)*
EZ_{it}	0.001 *(0.005)*	0.009 *(0.014)*	0.000 *(0.007)*	0.001 *(0.006)*	0.004 *(0.006)*
$EZ_{it}*mon_i$	-0.009 *(0.014)*	-0.008 *(0.014)*	-0.007 *(0.016)*	-0.009 *(0.014)*	0.000 *(0.019)*
$EZ_{it}*buspl_i$		-0.008 *(0.014)*			
$EZ_{it}*job_i$			0.001 (0.008)		
$EZ_{it}*cap_i$				-0.000 *(0.010)*	
$EZ_{it}*land_{it}$					-0.121 *(0.168)*

Number of observations: 60531
* p-value<0.1 ** p-value<0.05 *** p-value<0.01
Standard errors are in parentheses
(a) For clarity of exposition the coefficient estimates of the year and state dummies are not reported.
The complete list of regression results is available upon request.

Table 9: Random Growth Rates Estimates of Zone Status and Policy Features Impact on Two-Digit SIC Industry Employment

		Independent variable (a)					
Specification		EZ_{it}	$EZ_{it}*mon_i$	$EZ_{it}*buspl_i$	$EZ_{it}*job_i$	$EZ_{it}*cap_i$	$EZ_{it}*land_{it}$
(I)	SIC 20	0.070 *(0.056)*	-0.251 *(0.287)*				
	SIC 24	-0.002 *(0.066)*	-0.003 *(0.052)*				
	SIC 27	0.028 *(0.044)*	-0.051 *(0.082)*				
	SIC34	-0.001 *(0.072)*	0.279 *(0.211)*				
	SIC 35	0.122 *(0.104)*	-0.648 *(0.524)*				
(II)	SIC 20	-0.090 *(0.170)*	-0.183 *(0.294)*	0.170 *(0.169)*			
	SIC 24	0.009 *(0.159)*	-0.002 *(0.053)*	-0.014 *(0.169)*			
	SIC 27	0.124 *(0.122)*	-0.055 *(0.082)*	-0.105 *(0.126)*			
	SIC34	-0.107 *(0.149)*	0.250 *(0.214)*	0.126 *(0.155)*			
	SIC 35	0.164 *(0.158)*	-0.607 *(0.536)*	-0.055 *(0.154)*			
(III)	SIC 20	0.090 *(0.070)*	-0.259 *(0.287)*		-0.045 *(0.097)*		
	SIC 24	0.106 *(0.125)*	-0.056 *(0.073)*		-0.143 *(0.139)*		
	SIC 27	0.009 *(0.059)*	-0.040 *(0.084)*		0.039 *(0.081)*		
	SIC34	0.089 *(0.091)*	0.183 *(0.219)*		-0.157 *(0.098)*		
	SIC 35	0.145 *(0.109)*	-0.624 *(0.525)*		-0.0661 *(0.091)*		
(IV)	SIC 20	0.096 *(0.060)*	-0.314 *(0.291)*			-0.203 *(0.165)*	
	SIC 24	0.007 *(0.067)*	0.003 *(0.052)*			-0.182 *(0.174)*	
	SIC 27	0.011 *(0.047)*	-0.043 *(0.082)*			0.137 *(0.125)*	
	SIC34	-0.002 *(0.082)*	0.281 *(0.227)*			0.005 *(0.164)*	
	SIC 35	0.061 *(0.127)*	-0.393 *(0.607)*			0.142 *(0.171)*	
(V)	SIC 20	0.070 *(0.058)*	-0.242 *(0.301)*				-0.153 *(1.549)*
	SIC 24	-0.021 *(0.067)*	-0.024 *(0.058)*				-1.299 *(1.539)*
	SIC 27	0.010 *(0.012)*	-0.065 *(0.090)*				-0.445 *(1.217)*
	SIC34	-0.015 *(0.012)*	0.186 *(0.234)*				-1.419 *(1.554)*
	SIC 35	0.121 *(0.139)*	-0.571 *(0.566)*				0.496 *(1.386)*

Number of observations: 26971 (SIC 20); 31952 (SIC 24); 35527 (SIC 27); 26949 (SIC 34); 32665 (SIC 35)
Overall R2: 0.0007 (SIC 20); 0.075 (SIC 24); 0.0034(SIC 27); 0.0031 (SIC 34); 0.0024 (SIC35)
Standard errors are in parentheses
* p-value<0.1 ** p-value<0.05 *** p-value<0.01
(a) For clarity of exposition the coefficient estimates of the constant and of the year dummies are not reported.
The complete list of regression results is available upon request.

Table 10: Estimates of Zone Status and Policy Features Impact on Two-Digit SIC Industry Employment Growth. Propensity Score Approach -Robust Regression Estimates -

		Independent variable [(a)]					
Specification		EZ_{it}	$EZ_{it}*mon_i$	$EZ_{it}*buspl_i$	$EZ_{it}*job_i$	$EZ_{it}*cap_i$	$EZ_{it}*land_{it}$
(I)	SIC 20	0.000 *(0.001)*	0.002 *(0.008)*				
	SIC 24	-0.007 *(0.007)*	0.004 *(0.005)*				
	SIC 27	0.002 *(0.004)*	-0.004 *(0.007)*				
	SIC34	0.004 *(0.007)*	-0.008 *(0.021)*				
	SIC 35	-0.001 *(0.009)*	0.008 *(0.049)*				
(II)	SIC 20	-0.007 *(0.007)*	0.003 *(0.009)*	0.007 *(0.007)*			
	SIC 24	-0.019 *(0.023)*	0.004 *(0.005)*	0.13 *(0.024)*			
	SIC 27	-0.009 *(0.014)*	-0.03 *(0.007)*	0.012 *(0.015)*			
	SIC34	0.012 *(0.020)*	-0.007 *(0.022)*	-0.009 *(0.021)*			
	SIC 35	0.063 *(0.022)*	0.024 *(0.050)*	-0.070 *(0.022)*			
(III)	SIC 20	0.000 *(0.002)*	0.002 *(0.008)*		0.001 *(0.002)*		
	SIC 24	0.005 *(0.012)*	-0.001 *(0.007)*		-0.019 *(0.014)*		
	SIC 27	-0.002 *(0.005)*	-0.001 *(0.007)*		0.011 *(0.007)*		
	SIC34	0.010 *(0.008)*	-0.015 *(0.022)*		-0.013 *(0.010)*		
	SIC 35	-0.002 *(0.010)*	0.007 *(0.050)*		0.003 *(0.009)*		
(IV)	SIC 20	0.001 *(0.001)*	-0.001 *(0.009)*			-0.008* *(0.004)*	
	SIC 24	-0.006 *(0.007)*	0.004 *(0.005)*			-0.006 *(0.017)*	
	SIC 27	0.003 *(0.004)*	-0.004 *(0.007)*			-0.008 *(0.010)*	
	SIC34	0.004 *(0.008)*	-0.008 *(0.024)*			0.000 *(0.015)*	
	SIC 35	-0.010 *(0.012)*	0.042 *(0.059)*			0.018 *(0.015)*	
(V)	SIC 20	-0.005 *(0.002)*	-0.007 *(0.009)*				-0.143 *(0.062)*
	SIC 24	0.000 *(0.010)*	0.009 *(0.007)*				-0.263 *(0.250)*
	SIC 27	0.005 *(0.007)*	-0.001 *(0.008)*				-0.093 *(0.158)*
	SIC34	0.002 *(0.009)*	-0.013 *(0.026)*				0.067 *(0.232)*
	SIC 35	0.000 *(0.010)*	0.021 *(0.055)*				-0.090 *(0.189)*

Number of observations: 20424 (SIC 20); 26077 (SIC 24); 25283 (SIC 27); 20348 (SIC 34); 24449 (SIC 35)
Standard errors are in parentheses
* p-value<0.1 ** p-value<0.05 *** p-value<0.01
(a) For clarity of exposition the coefficient estimates of the constant, the year and state dummies
and the propensity scores prCA_i - prVA_i are not reported. The complete list of regression results is available upon request.

**Figure 1: Impact of Zone Designation and Spontaneous Employment Growth Trend.
Random Growth Rates Estimate of Specification I –Median values for ZIP areas with 1000 Employees in 1981-**

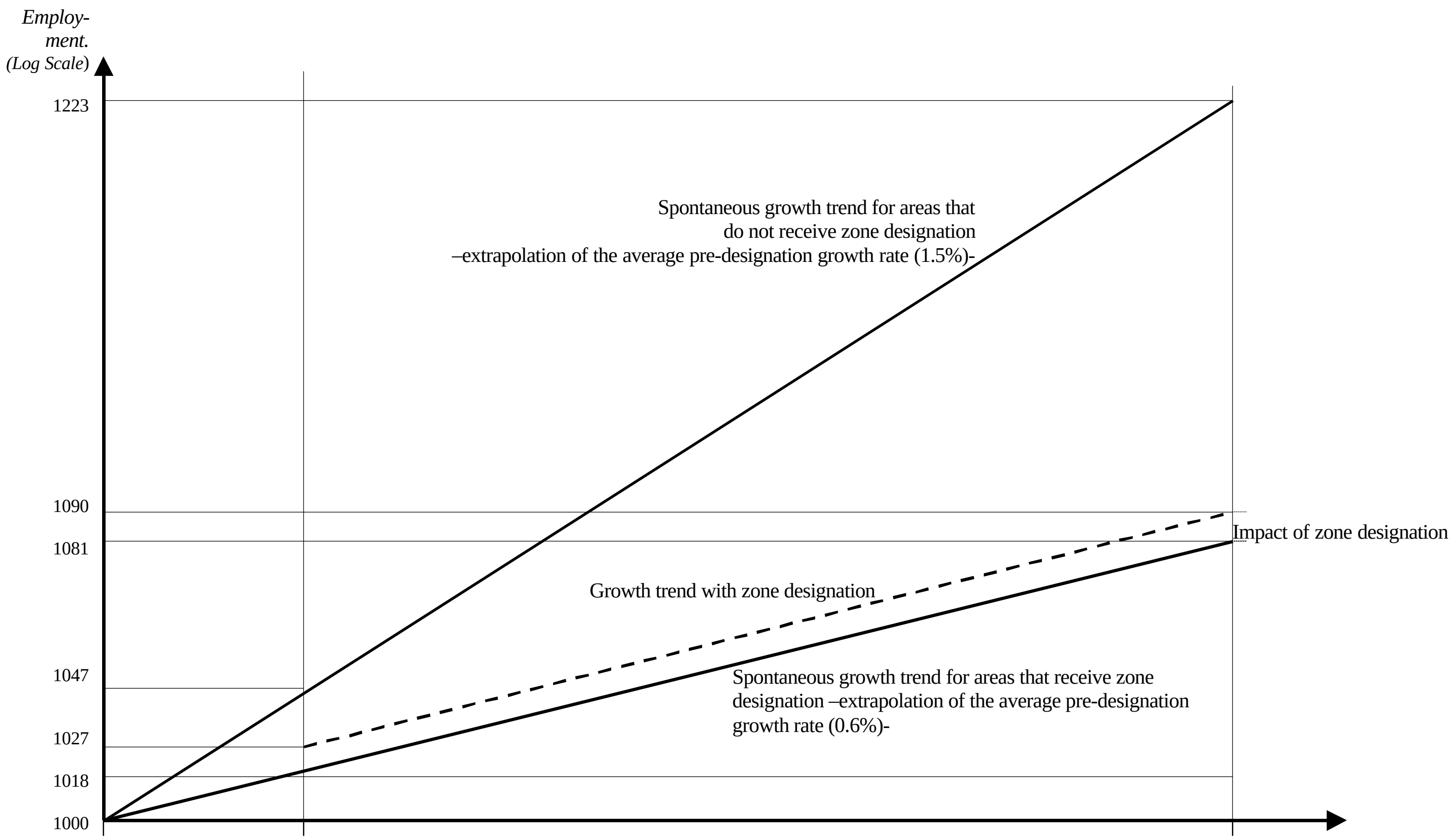

Figure 2: Impact of Zone Designation and Spontaneous Employment Growth Trend. Propensity Score Estimate of Specification I –Median values for ZIP areas with 1000 Employees in 1981-