DO TAX INCENTIVES AFFECT LOCAL ECONOMIC GROWTH? WHAT MEAN IMPACTS MISS IN THE ANALYSIS OF ENTERPRISE ZONE POLICIES

by

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Abstract

Geographically-targeted tax incentives remain popular initiatives in response to deteriorating economic conditions of urban and industrial areas. This paper exploits the exogenous variations of the U.S. state Enterprise Zone programs to estimate the impact of various incentive features on a number of dimensions of local economic growth. The econometric analysis uses plant level data to sort out growth outcomes into gross flows separately accounted for by new, existing, and vanishing businesses in the target areas. Results offer empirical evidence to support a number of specific policy recommendations and show that the impact of the incentives has more complex dynamics than those revealed by the null mean impact estimates obtained from analyzing net growth outcomes.

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1. Introduction.

In the last two decades geographically-targeted tax incentive programs have been increasingly popular initiatives aimed at attracting and/or retaining firms in economically blighted areas. Following the UK experience (McDonald 1995), in the US more than 40 states have implemented economic revitalization programs, mainly referred to as Enterprise Zone (EZ) programs, offering predominantly tax incentives to firms located in (or in the proximity of) urban pockets of severe economic and social distress (HUD 1997). Since 1994, also the Federal Government have implemented geographically -targeted economic revitalization programs, referred to as "Empowerment Zone" and "Enterprise Community" programs (HUD 1999), which offer tax incentives as the bulk of the incentive packages available to businesses in the assisted areas. In the EU, a variety of programs co-funded through the European Regional Development Fund (ERDS) target, from the beginning of the nineties, areas with severely declining industrial production or stagnant economic conditions (European Commission 1999). Stated goals, for both the US and EU programs, are broadly defined in terms of local economic growth spurred in the program target areas.

Despite the large popularity of tax incentive programs targeting disadvantaged areas, the available empirical evidence on their effectiveness on local economic growth is still somehow contradictory (e.g. Buss 2001, Boarnet 2001, Greenbaum and Engberg 2000, Boarnet and Bogart 1996, Wilder and Rubin 1996, Papke 1994, 1993). U.S. state EZ programs have the potential to offer valuable empirical evidence on the effectiveness of geographically-targeted tax incentives as local economic development tools (and to contribute to the longstanding debate around the effect of tax differentials on firm locations). State EZ programs display a great amount of heterogeneity in their policy implementation features. Such heterogeneity is a precious resource as it stems from political decisions that are very likely to be uncorrelated to the future economic trends recorded in the target areas. These natural experiment conditions favor comparative analysis to test the effectives of different implementation features as best practices for future interventions. EZ areas are also small enough so that appropriate comparison areas can be found within the same regional/local economies. EZ data, thus, could allow impact estimates to be retrieved from empirical models that effectively control for factors that may contribute to the observed outcomes independently from the program intervention.

Most of existing EZ studies, however, did not offer findings that can be easily translated in defined policy recommendations to refine future geographically-target intervention. As exhaustively reviewed by Wilder and Rubin (1996), early evaluation studies, in the mid eighties, were mostly descriptive case study researches (e.g. Jones 1985 and 1987) which focused on measuring building activity in single zone areas at pre- post-designation times, with no attempt to empirically separate effects of zone designation from those from other exogenous economic trends. Subsequent EZ evaluation efforts were a number of studies using outcome data retrieved from interviews with program officials and zone businesses (e.g. U.S. General Accounting Office 1988, HUD 1986, Erickson and Friedman 1990a, 1990b). These studies produced heterogeneous results on the effectiveness of EZ programs and were criticized for the widely recognized (e.g. Bartick and Bingham 1995, Wilder and Rubin 1996, Boarnet 2001) bias due to the tendency of program officials and businesses to overestimate the outcomes attributable to the impact of the EZ incentives. A number of more recent studies offered remarkable contributions by developing sound econometric models to estimate the impact of EZ program interventions (Boarnet and Bogart 1996, Papke 1993, 1994, Dowall 1996, Lambert and Coomes 2001). These studies offered empirical evidence from single state's programs, reasonably free of biases due to different initial conditions between the target areas and the rest of the national/regional economy, and due to spontaneous local economic trends. Such evidence, however, do not allow to easily generalize positive or negative findings to other places or times due the extensive heterogeneity of the state EZ programs.

To provide results with larger external validity, Greenbaum (1998), Bondonio and Engberg (2000) and recently Bondonio (2002) and Peters and Fischer (2002) have proposed comparative econometric evaluation studies using U.S. Census Bureau data. The results of such studies points toward a negligible impact of zone designation. Greenbaum (1998) and Bondonio (2002) studies, however, suggest that the zero-impact results may indeed derive from a positive treatment effect on attracting new firms in the target areas, and a counterbalancing loss of business activity due to the treatment-induced accelerating of downsizing and closure of existing firms. Peters and Fischer (2002) also analyze the composition of economic growth and decline in EZ areas with a descriptive analysis of the flows of establishment births, deaths and relocations into and out of zones. Building from such analyses, this paper contends that the most effective way to exploit state EZ programs is to use establishment-specific panel data to comparatively estimate the impact of state-specific policy implementation features on measures of economic growth separately accounted for by the following three types of establishments defined as whether or not, at time *t*, they:

a) operate for the first time at their present location (*new establishments*);
b) operate at the same location as at time *t-1* (*existing establishments*);
c) cease operations in the location where they were active at time *t-1* (*vanishing establishments*).

Analyzing EZ programs by operationalyzing the heterogeneity of the specific EZ policy features and by sorting growth outcomes by types of establishments is important. For example, target areas such as newly equipped industrial-parks may be aimed mainly at attracting new establishments in areas close to pockets of severe economic and/or social distress. Target areas such as inner-city industrial or business districts, may instead be mainly aimed at helping existing businesses to grow and/or survive. Empirically testing whether certain policy features favor best the attraction of new businesses rather than the retention of existing production activities would allow to design future interventions best suited either for the first or the second types of target areas.

This paper, unlike the bulk of the existing EZ literature, allows such testing to be performed by using plant level U.S. Census data to estimate the marginal impact of specific EZ policy features on the baseline growth of employment, capital expenditures, sales (value of shipments) and payroll per employee separately sorted into outcomes accounted for by *new-*, *existing-* and *vanishing-establishments*. The analysis is performed with a two-steps econometric model that yields impact estimates reasonably free of selection and omitted variable biases under plausible assumptions on the selection into treatment process. As noted by Peters and Fischer (2002), to comparatively analyze the impact of geographically-targeted tax incentives it is important to properly measure and control for the value to businesses of the incentive packages offered in the target areas. A measure of the monetary value of the EZ tax incentive packages is included in the econometric model developed for the analysis based on figures provided by the "hypothetical firm" method embedded in Fischer's and Peters's (1998) TAIM (Tax and Incentive Model) algorithm.

Results of the analysis show that, consistently with other econometric studies (Peters and Fischer 2002, Bondonio and Engberg 2000, Boarnet and Bogart 1996, Dowall 1996 and Papke 1993), state EZ programs do not have a mean positive impact neither on employment nor on capital expenditures, sales and payroll per employee, measured as net growth rates reported in the target areas. The impact of the EZ programs reveals instead more complex dynamics when the growth outcomes are separately sorted into gross flows accounted for by different types of establishments. Gross flows separately accounted for by *new-* and *existing-establishments* are indeed positively affected by the implementation of some specific EZ policy features. EZ incentives, instead, tend to increase the rate of business failures in the target areas, offsetting their positive impact in attracting new businesses in the target areas and favoring the growth of EZ businesses that remain on the market. These findings lead to some specific policy recommendations to help refining future geographically-targeted economic development initiatives.

The reminder of the paper is organized as follows. Section 2 contains a brief overview of the economic framework in which the analysis is developed. Sections 3 summarizes the data and EZ policy features. Sections 4 and 5 describe the econometric method and the sensitivity analysis. Section 6 illustrates the results. Section 7 discusses the major findings of the paper and provide policy recommendations.

2. State Enterprise Zones as Geographically-Targeted Economic Development policies

The analysis performed in this paper stems from considering state Enterprise Zones (EZ) programs as geographically-targeted policies aimed at correcting the allocation of production activities in order to boost economic growth in distressed areas. As reviewed in Ladd 1994, Gyorko 1998, there is debate as to whether help for economically blighted areas should be targeted at people or places. Certainly it could be argued that uncompetitive places should be allowed to fail in similar ways as market forces push uncompetitive businesses out of businesses. The economic justification in favor of place-based strategies, such as geographically-targeted tax incentives, rely on market failures that challenge the sustainability of central cities. Labor markets may be inefficient not adjusting promptly to changes in economic opportunities. In the European Union, labor market rigidities often prevent businesses from offering lower wages to workers in high-unemployment regions. In the U.S., it has been somehow controversially argued that inner city residents may face higher unemployment rates because of suburban job opportunities being not accessible to them (an hypothesis refereed to as “spatial mismatch”, see for example Kain 1994, Ihlanfeldt and Sjoquist 1998 and Peters and Fischer 2002 for recent reviews of theoretical arguments and empirical findings on the hypothesis).

Perhaps more convincingly than the “spatial mismatch” hypothesis, a broader externality argument can also be invoked to justify geographically targeting policies (Gyourko, 1998; Bartik, 2000, Sridhar 2001). Firms’ location decisions, if based solely on perceived private costs and revenues of possible alternative locations, would not properly account for collective costs deriving from such decisions. These costs may include the followings. Increased urban sprawl and increased vehicular traffic congestion, if firms’ decisions lead to economic activities being concentrated in newly developed areas, instead of reconverting decaying former industrial districts. Greater levels of local air and/or water pollutions and depleting of local natural resources, if firms’ decisions lead to a severely inhomogeneous geographical distribution of industrial productions. . Nurturing of criminal activities in decaying or abandoned central areas left aside from economic development. Labor market inefficiencies due to the failure of locating production activities near pockets of underused resources in high unemployment areas.

Coherently this such economic rationale, this paper investigate the EZ impacts on proximate economic growth outcomes measured directly in the EZ areas and in their most immediate vicinities. Program impacts on state-wide overall economic growth outcomes and possible geographical displacement effects induced by the programs are instead not investigated in the analysis, as, based on the same economic rationale, EZ programs would produce some socially desirable outcomes even if economic growth in the EZs would occur at the expense of the surrounding non-EZ areas.

Successful geographically-targeted programs should boost economic growth in the assisted areas by either attracting new firms or helping existing firms to expand their business. Empirical evidence of such increased economic development would be found in increased employment, sales and capital expenditures. Thus, this paper uses measures of employment, value of shipments and capital expenditures as outcome variables for the analysis. Finally, the analysis also focuses on average payroll per employee to assess whether the program intervention promoted higher- or lower-paying jobs.

3. Data

This paper analyzes data from 11 state EZ programs (California -CA, Connecticut -CT, District of Columbia D.C., Florida -FL, Indiana -IN, Kentucky -KY, Maryland -MD, New Jersey -NJ, New York -NY, Pennsylvania -PA, Virginia -VA) in place from 1982 and 1992. These EZ programs are selected based on the following criteria: location of zones in urban distressed areas and/or in abandoned urban industrial parks; limited territorial extension of the program (i.e. the sample investigated is limited to states that do not designate hundreds of zones), competition among local communities to receive the EZ status. The EZ programs from the eleven states sampled for the analysis, in general, target areas that qualify for zone consideration by virtue of per-capita income, unemployment and/or poverty rate being beyond thresholds of economic distress (while, in some cases zone eligibility is also based on land availability and/or building vacancy criteria). The bulk of the EZ incentive packages offered in the eleven sampled states relies on relieving the tax burden on businesses by tying incentives to either the number of new jobs created or the amount of investment made in the zones. Tax incentives are also complemented in some cases by services and/or utility subsidies (a detailed description of each single EZ program is available in HUD 1995).

The state-specific policy features of the 11 EZ programs are operationalized in this paper in terms of a small number of variables in order to enable the empirically testing of some policy recommendations stemming from economic theory and/or interviews with business representative and program officials. Specifically, similarly as in Bondonio and Engberg (2000), the EZ policy variables used in the analysis are those described in Table 1.

Table 1

Among the variables of Table 1, the territorial extension of the EZ program (EZ_SIZE) is operationalized to test whether or not small programs can produce better results by offering strongest marketing efforts and closest monitoring and evaluation of the implementation of the program in each zone. The provision of a strategic economic plan (EC_PLAN) is operationalized to test whether or not strategic planning is beneficial per se to local economic development by promoting a better coordination of all local development resources (Wilder and Rubin 1996). Whether EZ business incentives are tied to job or to capital investment requirements is coded (through the variables JOB_REQ and CAP_REQ) to test for possible substitution effects between capital and labor induced by EZ incentives. The monetary value of EZ incentives (EZ_VAL) is operationalized to estimate and control for the marginal impact on economic growth due to different state-specific monetary values to businesses of the EZ incentive packages. Such measure is obtained through the hypothetical firm approach embedded in the TAIM -Tax and Incentive Model developed by Fischer and Peters (1998). Following Bondonio and Engberg (2000) the monetary value of EZ incentives to businesses is defined as the difference between the average internal rate of return [IRR] (computed through the TAIM software) of the investment in a new plant made by a set of "typical" firms in an EZ area and the IRR of the same investment made by the same "typical" firms in a non-EZ area within the same state. Operationalizing the monetary value of EZ incentives through such within-state differential estimate is a choice due to the fact that local tax incentives are widely believed (e.g. wilder and Rubin 1996, Bostic 1996, Bartik 1991) to have the potential to affect firms' location decisions mainly at the margin between similar and adjacent locations rather than across different states and/or regions[1].

[1] A more detailed description of the TAIM algorithm and the procedure used to operationalize the monetary value of EZ incentives can be found in Peters and Fischer (2002) and Bondonio and Engberg (2000), respectively. At the time of analysis, the TAIM algorithm was available only for five (CA, KY, NY, PA, VA) of the eleven states included in the data sample. For this reason the analysis was forced to be replicated on two data samples: the entire group of eleven states which allows more variation in EZ policy designs to be analyzed and a smaller group of five states which allows to include in the analysis the monetary value of the EZ incentives.

EZ dates of designation, local economic growth outcomes, zone location and zone characteristics are collected from various documents and sources. EZ dates of designations are retrieved from interviews and questionnaires administered to program officials and administrators. Economic growth outcomes, in the form of value of shipments, capital expenditures and payroll per employee are obtained from the quinquennial Census of Manufactures (CM) portion of the Census Bureau's Longitudinal Research Database (LRD)[2]. The available CM data cover every U.S. manufacturing plant (establishment) with five or more employees in years 1977-82-87 and 1992, allowing establishment-specific characteristics to be tracked over time through assigning to each establishment a unique identification number. Such characteristic of the CM portion of the Census LRD allows pre-post intervention economic growth measures to be properly sorted into changes produced by *new-*, *existing-* and *vanishing-establishments*. To be used in the analysis each establishment-specific change in employment, value of shipments, capital expenditures, and payroll per employee is aggregated by type of establishment and ZIP area. The intersection among the three types of changes, sorted by types of establishments, and the four outcome measures available in the data (employment, value of shipments, capital expenditures and payroll per employee) yields twelve data bins from which the dependent variables used in most of the various specifications of the analysis are formed.

Zone location information are either provided by programs officials or retrieved from EZ maps through the Arc View GIS (vers. 3.0a) software. Location of zones is mapped in terms of U.S. Postal ZIP code areas by coding ZIP areas as EZ ZIPs if they encompass any portion of an actual zone and non-EZ ZIPs otherwise. Mapping zones in terms of ZIP code areas is a choice due to data constraints and for allowing the analysis to test whether or not zone designation had an impact on both the EZ areas and their most immediate vicinities. As ZIP code areas changed over time, the time –invariant unique plant identifier code provided by the Census LRD data was exploited to trace historic ZIP code area changes from 1977 to 1992 (the time span of the data used in the analysis). Table 2. illustrates the programs' starting dates and the number of EZs and zone-ZIPs tabulated by State.

Table 2

[2] Census Bureau's LRD was accessed while the author was a Census Bureau research associate at the Carnegie Mellon Research Data Center in Pittsburgh, PA. Results and descriptive statistics from the LRD have been screened to insure that no confidential data are revealed.

EZ pre-designation demographic, income, poverty, unemployment and population density characteristics are finally retrieved from the 1980 Decennial Census STF3a files. These data recorded at the census tract level, were allocated to the ZIP code areas using the Mable Geocorr[3] geographical correspondence engine that determines the degree of overlap between different spatial units.

4. Method of Analysis

To estimate the impact on local economic growth of different EZ policy features for each type of establishments (sorted into *new*-, *existing*- and *vanishing-establishments*) the analysis is implemented with a "two-steps conditioning on a propensity score" model that builds on the propensity score approach to evaluation developed by Rosenbaum and Rubin (1983, 1984) and that has been recently used in other EZ evaluation studies (e.g. Engberg and Greenbaum 1999, Bondonio and Engberg 2000). The model is applied to both the entire sample of the eleven states and the smaller sub-sample of the five states that allow the monetary value of the EZ incentives to be included in the analysis.

In the first step of the model the probability of each ZIP area to be designated as EZ is estimated as a function of two variables capturing the pre-designation growth in employment and number of establishments, and five 1980 Decennial Census variables used to capture pre-designation poverty, unemployment, income and demographic characteristics. Designation probabilities are estimated through a separate probit regression for each of two state clusters[4]. The two samples of states used in the analysis are clustered based on the criteria mentioned in the EZ state legislations for selecting zone areas. The first cluster of states includes CA, CT, DC, KY, NJ, VA (only CA, KY, VA for the small-sample analysis that allows the TAIM estimates of the monetary value of the incentives to be included in the model) for which official zone selection guidelines include primarily income, unemployment or poverty indicators. The second cluster includes FL, IN, MD, NY, VA (only NY and VA for the small-sample analysis) for which official zone selection guidelines also include criteria based on land availability or building vacancy, in addition to unemployment, income or poverty indicators. For this second cluster of states the probit specification also include two 1980 Census housing market variables in addition to all of the variables adopted in the probit specification for the first cluster of states. The probit regressions for the two clusters of states are illustrated in equation (1) and (2) respectively, while

[3] MABLE/GEOCORR is available on the World-Wide Web at http://plue.sedac.ciesin.org/plue/geocorr/.

[4] This clustering solution is preferred to the less restrictive specification of a separate probit regression for each state included in the analysis because among the 11 states included in the analysis, several of them have too few number of EZ ZIP, so that separate estimation for each state would be unfeasible for the entire data sample.

the descriptive statistics of all the independent variables (for the large-sample of eleven states) are listed in Table3:

$$P(EZ_i=1) = \Phi(\beta_1 UNEMP_i + \beta_2 POV_i + \beta_3 INC_i + \beta_4 DENS_i + \beta_5 MIN_i + \beta_6 EMPGRW_i + \beta_7 ESTGRW_i + \phi_I), \quad (1)$$

$$P(EZ_i=1) = \Phi (\beta_1 UNEMP_i + \beta_2 POV_i + \beta_3 INC_i + \beta_4 DENS_i + \beta_5 MIN_i + \beta_6 EMPGRW_i + \beta_7 ESTGRW_i + \beta_8 OCCHOUS_i + \beta_9 VALHOUSE_i + \phi_{II}), \quad (2)$$

where:

i = ZIP areas;

$UNEMP_i$, POV_i, INC_i, $DENS_i$, MIN_i =set of 1980 Census variables capturing unemployment, poverty, per capita income, population density and percentage of African Americans and other minorities;

$EMPGRW_i$, $ESTGRW_i$ =1977-82 employment and establishment growth variables obtained from the Census CM data;

$OCCHOUS_i$, $VALHOUSE_i$ =1980 Census variables expressing the percentage of occupied houses over the total number of houses and the average value of the owner occupied houses;

ϕ_I = set of states dummies for cluster I (CA, CT, DC, KY, NJ, PA for the large-sample analysis; CA, KY, PA for the small-sample analysis);

ϕ_{II} = set of states dummies for cluster II: (FL, IN, MD, NY, VA for the large-sample analysis; NY, VA for the small-sample analysis).

Table 3

The predicted probabilities from eq. (1) e (2), referred to as propensity scores (Rosembaum and Rubin 1983, 1984, Heckman et. al. 1997, 1998), can be interpreted as a single ZIP-specific parameter that summarizes the pre-intervention economic growth and socio-economic conditions of the ZIP areas included in the data sample. In the second step of the model such predicted probabilities are inserted in the 5-year growth rate outcome regression of eq. (3).

$$Ln(Y_{it}/ Y_{it-5})= \alpha EZ_{it}*T_{it} + \Sigma_{pol}\varphi_{pol}(EZ_{it}*pol_{it}*T_{it})+ \Sigma_c\delta_c PR^c_i + \phi_j + \gamma_t + \mathbf{u}_{it} \quad (3)$$

Where:

t=1987, 1992[5]

i=ZIP area; c=cluster of states (c=I, II); j= state;

pol= policy variable (Table 1: pol=EZ_SIZE, EC_PLAN, JOB_REQ, CAP_REQ, EZ_VAL)

$\mathrm{Ln}(Y_{it}/\ Y_{it})$= outcome growth rate in the 5-year period ending in year t;

$\sum_c \delta_c PR^c{}_i = (\delta_I PR^I{}_i + \delta_{II} PR^{II}{}_i)$= predicted probabilities from eq. (1) and (2);

EZ_{it} = 1 if ZIP i contains an EZ which was active in the 5-year period ending in year t;
=0 otherwise;

$T_{it}=[(t-t^d{}_i)/5]$=portion of the 5-year period ending in year t in which ZIP i contained an active EZ ($t^d{}_i$ = year of EZ designation);

$\sum_{pol} \varphi_{pol}(EZ_{it}*T_{it}*pol_{it})$= set of interaction terms between EZ status, portion of the 5-year period in which ZIP i contained an active EZ and one of the policy variable of Table 1.

In eq. (3) the propensity scores estimated from equations (1) and (2) are operationalized as two variables: PR^I and PR^{II}. The variable PR^I is constructed as the predicted probability from equation (1) for all the ZIPs located within the states included in cluster I, and zero for the ZIPs located in any of the other states. Likewise, PR^{II} contains the predicted probability from equation (2) for all the ZIPs included in cluster II and zero for all the other ZIPs.

By inserting the propensity scores into the outcome regression of eq. (3) the model allows observed pre-intervention growth trends and socio-economic characteristics to be controlled for. EZ areas, in general, would tend to have more disadvantaged socio-economic conditions and slower growth trends, at times prior to the beginning of the program, than non-EZ areas. Such disadvantaged initial conditions could induce EZ areas to grow less than non-EZ areas even in the absence of the intervention. Without properly controlling for such differences, impact estimates of the program intervention could be biased (selection bias). The model of equation (3) sorts out impacts due to observable differences in pre-intervention growth trends and socio-economic characteristics (coefficients δ_1, δ_2) from the actual impacts due to the program intervention and the specific EZ policy features (coefficients α, φ). Area-specific characteristics, however, could be directly inserted in

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[5] As Census CM data are available only at 5-year intervals for the years 1977, 1982, 1987, 1992, the period 1977-82 cannot be used in the analysis because for 1977 it is not possible to sort out outcomes by type of establishments (which would be allowed only comparing the 1977 data with the unavailable 1972 data). For this reason, in eq. (3), the 5-year periods available for the analysis are only (1982-87) or (1987-92), and therefore t is either 1987 or 1992.

equation (2) instead of being used in the two-steps propensity score procedure of equation (1) and (2). Such a two-step procedure is performed because it constitutes a convenient way to deal with non-linearities in the relationship between the 5-year outcome growth rate Ln(Y_{it}/ Y_{it}) and pre-designation area-specific characteristics. As argued by Rosenbaum and Rubin (1983), including in the outcome equation the set of pre-designation variables of the propensity score equations 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 outcome variable of the model. This is difficult to achieve since economic theory does not offer solid guidance in this matter. 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 the outcome variable on the pre-designation variables[6].

Estimating the propensity score values also allow ZIP areas with very low or high designation probability to be excluded from the estimation sample. In a matching-estimator setting, Heckman et. al (1997) and Dehejia and Wahba (1998) show that better impact estimates can be retrieved limiting the estimation sample to observations with propensity scores for which there are both treatment and comparison observations. In this paper, the estimation sample is restricted by excluding the ZIP areas with propensity scores within the 1st percentile of the EZ-ZIPs' distribution or within the 99th percentile of the non-EZ ZIPs in order to reduce the influence of possible extreme outliers and alleviate the burden on the model to control for extreme pre-intervention characteristics.

The outcome regression of Eq. (3), together with the EZ status term ($EZ_{it}*T_{it}$), also contains a set of policy interaction term[$\boldsymbol{\varphi_{EZ\_SIZE}}(EZ_{it}*T_{it}*EZ\_SIZE_{it})$, ... , $\boldsymbol{\varphi_{EZ\_VAL}}(EZ_{it}*T_{it}*EZ\_VAL_{it})$] between the EZ status and one of the five state-specific policy variables listed in Table 1 (i.e the portion of state land covered by EZs, the monetary value of the EZ incentives and three dummies to code whether or not: an economic development plan is requires as part of the EZ application process, and EZ incentives are tied to the number of new jobs or to the amount of new capital investment,)[7]. These interaction terms are included in the model specifically to test whether state-specific EZ policy features have an impact on the dependent variable. Eq. (3) is finally completed with a set of state dummies ($\boldsymbol{\phi_j}$) to

[6] Heckman et al. (1997) have argued that what claimed by Rosembaum and Rubin (1983) does necessarily apply in practical terms when the propensity score has to be estimated instead of being known with certainty. In such cases, according to Heckman et al. the conceptual and mechanical advantages of using a two-stage propensity score approach lies mostly in simplicity of estimation, mimicking the conventional econometric approach of using Mills ratios to correct for selection bias.

[7] The exact composition of the set of policy interaction term varies across the single specifications estimated in the analysis, as illustrated in Table 4.

control for possible state-specific economic trends and a dummy for the 1982-87 period (γ_t) to control for possible time-specific broad economic trends.

The coefficient estimates for the model of eq. (3) are retrieved from a number of different specifications using the Statacorp. (2000) robust cluster estimator that adjust the coefficient standard deviations for the possible within-state correlation of observations (Rogers 1993, Williams 2000, Wooldridge 2002). Each estimated specification differs from the other because of a different dependent variable among the twelve formed by intersecting the four economic growth measures (employment, value of shipment, capital expenditures and payroll per employee) used in the analysis with the three different types of establishments (new, existing and vanishing) in which such measures are sorted. The set of adopted specifications is then completed with a set of four regressions on the economic growth measures unsorted by types of establishments (specifications I-IV).

To best compare the findings of this paper with evidence from other EZ studies, all sixteen specifications are first estimated without including any policy interaction term except for the zone status and, for the small-sample analysis, the monetary value of the incentives. The analysis is then replicated also including the EZ policy interaction terms indicated in Table 1 in the following way: I) each specification estimated for the large-sample analysis includes all the four policy interaction terms [$\varphi_{EZ\_SIZE}(EZ_{it}*T_{it}*EZ\_SIZE_{it})$, $\varphi_{EC\_PLAN}(EZ_{it}*T_{it}*EC\_PLAN_{it})$, $\varphi_{JOB\_REQ}(EZ_{it}*T_{it}*JOB\_REQ_{it})$, $\varphi_{CAP\_REQ}(EZ_{it}*T_{it}*CAP\_REQ_{it})$] that are not formed with the Monetary value of the incentives; II) each specification estimated for the small-sample analysis includes instead only two policy interaction terms (one of which is the monetary value of the EZ incentives) at the time, due to the limited variation of state-specific EZ policy features offered by the data. A complete description of all estimated specifications is illustrated in Table 4.

Table 4

The two-steps estimation procedure of eqs. (1-3) is well suited for evaluating geographically-targeted tax incentive programs, such as the U.S. state EZs. For such programs the final selection into treatment typically revolves around decisions made by program officials on the basis of area-specific socio-economic statistics and official thresholds also observable by the evaluator. Little room is left for pure self-selection into treatment by local communities or for selection decision based on factors that are unobservable to the external evaluator. Thus, explicitly controlling for observable characteristics should yield more efficient estimates than relying solely on differencing methods (e.g. the random growth rate models of Heckman and Hotz 1989).

Taking advantage of the panel nature of the outcome data offered by the CM portion of the Census LRD, the model of eq. (3), however, also offers some robustness against selection on unobservables. By long differencing the dependent variable $Ln(Y_{it}/ Y_{it-5})$, the model allows unobserved area-specific characteristics to be correlated with the treatment and/or policy variables, yet providing unbiased net impact estimate of the program intervention as long as such unobserved characteristics are relatively constant along the pre-post intervention period. It should be noted that the model is quite robust against biases arising from cyclical economic factors as well. Such factors do not pose a threat to the validity of the analysis as long as they affect in the same way both EZs and any adjacent non-EZ areas located within a same local economy.

In sum, the proposed two-steps estimation procedure yields unbiased net impact estimates of the program intervention under any of the following assumptions:

-unobserved cyclical economic factors homogenously affect adjacent ZIP areas located within the same local economy, regardless of their EZ status;

-EZ designation occurs based on ZIP area-characteristics and pre-intervention growth trends observable to the evaluator;

-unobserved ZIP area characteristics, those distribution is different between EZ ZIPs and non-EZ ZIPs, are relatively constant over time.

The adopted estimation procedure would instead yield biased impact estimates in the event that program officials designate EZ areas based solely on information (unknown to the evaluator) that would allow them to forecast with accuracy the areas that would spontaneously growth the least or the most. If such unobservable ZIP-specific growth trend (that could be formalized, for example, by adding to eq. 3 a simple linear term such as $\beta_i t$) were correlated with treatment assignment [i.e. $Cov(\beta_i t, EZ_{it})\neq 0$], only random growth rates models (Heckman and Hotz 1989, Papke 1993, 1994, Boarnet and Bogart 1996 and Bondonio and Engberg 2000) would yield unbiased impact estimates of zone designation and EZ policy features. Random growth rates models are not feasible in this paper due to the limited extension in time of the data (available only for years 1982, 1987 and 1992). However, the assumption of [$Cov(\beta_i t, EZ_{it})\neq 0$] should not be too worrisome for the EZ programs analyzed in this paper: even assuming that program officials would want to designate exclusively the best or worst future performers, their forecasting would have to be likely based on the same data and analytic tools available for this analysis.

It should be finally noted that Instrumental Variable (IV) and Mills Ratio (Heckman 1976, Heckman and Robb 1985 and Lee 1978) methods are also not applicable to the analysis as they both require a subset of variables that affect EZ designation but do not affect the baseline growth of an area (Mills Ratio methods need exclusion restrictions similarly as in IV models in order to obtain robust estimates, Robinson 1989). For EZ programs, unfortunately, such variables are quite impossible to find due to the fact that zone areas are designated based on the very same pre-intervention economic and social conditions that represent some of the major factors affecting the post-intervention economic growth of zones. IV estimators, however, were tried elsewhere in sound EZ evaluations (Boarnet and Bogart 1996) by instrumenting EZ status with pre-intervention economic and demographic area characteristics. For consistency such pre-intervention characteristics should not have a direct impact on the area's baseline growth rate, an assumption that is not imposed for the EZ programs analyzed in this paper.

5. Specification Tests and Sensitivity Analysis

The model of eq. (3) is restrictive in two major ways. First it imposes that zone designation has the same impact on the outcome growth of interest in each 5-year period after designation without distinguishing between zones designated 5 years prior to time t, and zones designated more than 5 years prior to time t. Second, it forces zone designation to affect the outcome growth of interest (in each of the two 5-years periods) linearly as a proportion of the 5-year period for which zones have been in existence. To test these restrictions two less parsimonious and more generalized models are also analyzed. The first of these models is illustrated in eq. (4):

$$\mathrm{Ln}(Y_{it}/Y_{it-5}) = \sum_n \theta_n EZ\_n_{it} + \sum_c \delta_c PR^c_i + \phi_j + \gamma_t + \mathbf{u_{it}}, \qquad (4)$$

$n = 1,2,3,4$;

$EZ\_n_{it} = 1$ if $[(n-1)*2.5]$years $< (t - t^d_i) \leq [n*2.5]$ years;

$= 0$ otherwise;

$t = 1987, 1992$;

t^d_i = year of EZ designation.

In eq. (4) the EZ status indicator (EZ_{it}) is replaced by a set of four EZ status variables, (EZ_1_{it}, …EZ_4_{it}), that reflect the age of the enterprise zone at time t (measured at two and three-years intervals). EZ_1_{it} equals 1 if ZIP*i* in year *t* is a zone in its first or second year of existence and zero otherwise (e.g. when t=1987, EZ_1_{it}=1 if ZIP*i* is a zone designated either in 1986 or 1985; when t=1992, EZ_1_{it}=1 if ZIP*i* is a zone designated either in 1991 or 1990). EZ_2_{it}, indicates whether ZIP *i* in year t is a zone in its third, fourth, or fifth year of existence (e.g. when t=1987, EZ_2_{it}=1 if ZIP*i* is a zone designated either in 1982, 1983 or 1984; when t=1992, EZ_2_{it}=1 if ZIP*i* is a zone designated either in 1987, 1988 or 1989). EZ_3_{it}, EZ_4_{it}, indicate whether a ZIP is a zone in its sixth-seventh and eighth-ninth-tenth year of existence, respectively. The model of eq. (4) is more generalized than that of eq. (3) because it allows zone impacts to freely vary over time (at two- and three- year intervals), and because it is able to differentiate the impact of a zone designated at the beginning of the five-year period *t-(t-5)* from the impact of a zone designated in years prior to *t-5*. The model of eq. (4) is implemented primarily to test the restrictions posed by the more parsimonious model of eq. (3). Fully developing the model of eq. (4) to include the policy features of Table 1 would require adding to the equation sixteen interaction terms (obtained by multiplying each of the four EZ status variables with each one of the four policy variables). The inclusion of these interaction terms would undermine the precision of any retrievable EZ policy feature impact estimate[8]. The specification of eq. (4) can be used instead as one way to test the two main restrictions posed by the model of eq. (3). Although the restricted model of eq. (3) is not perfectly nested into the model of eq. (4), an F-test where the null hypothesis is that $\boldsymbol{\theta_3} = \boldsymbol{\theta_4}$ (i.e. the coefficient on EZ_3 is equal to that of EZ_4) can provide evidence on whether zones have the same impact on the dependent variable regardless of their previous age, provided that they are in place for the entire duration of the five-year period. Likewise, an F-test, where the null hypothesis is that $\boldsymbol{\theta_1} = (\boldsymbol{\theta_2} * 2/5)$, can provide evidence on whether zone designation affects the dependent variable linearly as a proportion of the five-year period for which zones have been in existence.

The second alternative model is illustrated in eq. (5):

$$Ln(Y_{it}/Y_{it-5}) = \sum_n \boldsymbol{\omega_n} \left\{ EZ_{it}*[t-(t^d_i+n)]/5 \right\} + \sum_c \boldsymbol{\delta_c} PR^c_i + \boldsymbol{\phi_j} + \gamma_t + \mathbf{u_{it}}; \qquad (5)$$

where:

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[8] The fully developed model of equation (4) would be:

$$Ln(Y_{it}/Y_{it-5}) = \sum_n \theta_n EZ\_n_{it} + \sum_n \sum_{pol} \gamma_{n(pol)}(EZ\_n_{it}*pol_{it}) + \sum_c \delta_c PR^c_i + \phi_j + \gamma_t + u_{it}$$

n =1,2,3,4

EZ_n_{it}= 1 if $(n-1)*2.5 < (t - t^d_i) \leq n*2.5$ years
0 otherwise

Results from such specification are available upon request to the author.

$n = 0, 3, 6$

$$(t - t^d_i + n) = 0 \quad \text{if } (t - t^d_i + n) < 0$$
$$= (t - t^d_i + n) \quad \text{if } 0 \le (t - t^d_i + n) < 5;$$
$$= 5 \quad \text{if } (t - t^d_i + n) \ge 5.$$

In eq. (5) the EZ status indicator (EZ_{it}) is replaced by a set of three EZ status variables ($EZ_{it}*[t-t^d_i]/5$, $EZ_{it}*[t-(t^d_i+3)]/5$, and $EZ_{it}*[t-(t^d_i+6)]/5$), that reflect the portion of the five-year period ending in year *t* in which a zone has been in existence measured from different times. $EZ_{it}*[t-t^d_i]/5$ expresses the portion of the five-year period ending in year *t* in which a zone *i* has been in existence, measured from the actual time t^d_i of zone designation. $EZ_{it}*[t-(t^d_i+3)]/5$ expresses the portion of the five-year period ending in year *t* measured from three years after the time of designation t^d_i. $EZ_{it}*[t-(t^d_i+6)]/5$, finally, expresses instead the portion of the five-year period ending in year *t* measured from six years after the time of designation t^d_i. These three EZ status indicators allow the restrictions posed by the model of eq. (3) to be tested directly: if the coefficients (ω_3, ω_6) on the two EZ status variables $[EZ_{it}*[t-(t^d_i+3)]/5]$ and $[EZ_{it}*[t-(t^d_i+6)]/5]$ are both zero, eq. (5) collapses into eq. (3). Therefore, an F-test, where the null hypothesis is such that the coefficients ω_3 and ω_6 are both zero, tests directly if the restrictions posed eq. (3) hold[9].

Robustness of the results is finally also tested by replicating the model of eq. (3) adopting two more stringent rules for sorting ZIP areas by EZ status: ZIP areas are coded as EZ ZIPs if at least 25% (rule I) or 50% (rule II) of their land is covered by actual zones.

6 Results

Table 5 reports the results from the probit regression of eq. (1) and (2). The coefficient estimates of Table 5 highlight the more important pre-designation characteristics affecting the probability of EZ designation for the large-sample analysis (eleven states)[10]. The importance of the pre-designation characteristics varies across the two clusters into which the states are grouped. All states tend to

[9] Due to the limited variation in EZ policy features, the chosen specification, allowing different growth rates after 3 and 6 years from designation, is preferable to a less parsimonious specification which includes 5 different growth rates (from time of designation and after 2, 4, 6 and 8 years) and twenty interaction terms to operationalize the state-specific policy features:

$$Ln(Y_{it}/Y_{it-5}) = \sum_n \omega_n\{EZ_{it}*[t-(t^d_i+n)]/5\} + \sum_n\sum_{pol} \gamma_{n(pol)}\{EZ_{it}*[(t-(t^d_i+n))/5]*pol_{it}\} + \sum_c \delta_c PR^c_i + \boldsymbol{\phi_j} + \gamma_t + \mathbf{u_{it}};$$
$$n = 0, 2, 4, 6, 8$$

Nevertheless, the analysis has been also replicated with such specification and results are available upon request to the author.

[10] Results for the small-sample analysis (five states) are very similar to those reported in Table 5 and they are omitted for the sake of brevity. Full regression results are available upon request to the author.

designate areas with high proportion of minority population and slow pre-designation employment growth. However, States in cluster I target areas with also low per-capita income and high population density, while states in cluster II tend to designate areas with also high poverty rates and low housing occupancy rates and values.

Table 5

To test the validity of the clustering solution adopted, the analysis has been replicated adopting the specification of equation (2) [which includes two housing variables] for retrieving the propensity score estimates of the cluster I states. Results from this specification, estimated on both the large and small samples of states, are in favor of the adopted clustering solution, showing that housing criteria do not affect zone designation for the cluster I states.

6.1 Impact of EZ status and monetary value of incentives

Results from the specifications of Table 4 with no policy interaction terms are summarized in Table 6. Coefficient estimates in Table 6 can be interpreted as the marginal percentage point contribution to the baseline 5-years growth rate of zone designation and the generosity of the EZ package (in terms of ΔIRR between investing in a new plant in a EZ location and in a non-EZ location within the same state).

Mean impact estimates on the net economic growth outcomes (specifications I-IV) are reported on the top portion of Table 6. Results from such specifications are in line with most of the recent econometric empirical evidence on EZ programs (Papke 1993, Dowall 1996, Boarnet and Bogart 1996, Bondonio and Engberg 2000), showing a modest (and statistically not significant) impact of zone designation and of EZ incentive generosity on all the dimensions of economic growth considered in the analysis.

Impact estimates of EZ status on employment, value of shipment and capital expenditure gross flows accounted for by new establishments show instead a positive marginal contribution of EZ designation to the 5-year baseline growth rates. These impact estimates, illustrated in the middle-top portion of Table 6, range from 25.2 (specification V, employment growth) to 19.1 (specification VII, growth in capital expenditures) percentage points for the analysis on the large sample of states, and from 29.3 (employment growth) to 10.3 (growth in capital expenditures) percentage points for the small-sample analysis. Zone designation is instead found to marginally depress the baseline growth rate of the payroll per employee accounted for by the new-establishments (the size of this negative impact is about 32.4 and 35.6 percentage points, for the large- and small-sample analysis, respectively]. Impact estimates of EZ designation on the baseline growth rates of employment, value of shipments and capital expenditures accounted for by existing establishments are also found to be positive (middle-bottom portion of Table 6), although the size of these estimates is much smaller than that on new-establishment outcomes, ranging from 7.6 (specification XI, growth in capital expenditures) to 5.7 (specification X, growth in value of shipments) percentage points for the large-sample analysis (from 6.1 to 3.2 percentage points for the small-sample analysis). No significant impact of EZ designation is instead estimated on the growth rate of payroll per employee accounted for by the existing-establishments. The bottom portion of Table 6 reports the impact of zone designation on the 5-year rate-losses accounted for by vanishing establishments. In this part of the analysis zone designation is found to significantly increase the baseline rate-loss of employment, value of shipments and capital expenditures in both the large- and small-sample analysis. EZ impact estimates for the payroll per employee specification, instead, do not reach statistical significance. For all specifications reported in Table 6, the estimated impacts of the monetary value of the EZ incentives, in the small-sample analysis, are with the expected sign but shows large standard errors and fail to reach statistical significance.

Table 6

The results reported in Table 6, in general, prove to be quite robust, as they withstand the challenge of replicating the analysis with the alternative models of equation (4) and (5). Results from the model of eq. (4) are entirely consistent with the findings illustrated in Table 6. The two specification tests (Ho: $\theta_3 = \theta_3$; and Ho: $\theta_1 = \theta_2 * 2/5$) developed from eq. (4) to check whether the restriction posed by the model of eq. (3) hold or not, are in favor of accepting the restrictions of eq. (3). Results from the model of eq. (5) show that estimating three different changes in the baseline growth rate (measured at different times from zone designation: $\omega_0 \{EZ_{it} * [t-(t^d_i+0)]/5\}$, $\omega_3 \{EZ_{it} * [t-(t^d_i+3)]/5\}$, $\omega_6 \{EZ_{it} * [t-(t^d_i+6)]/5\}$) do not provide any additional reliable information on the marginal impact of zone designation due to the loss in precision in retrieving the three slope estimates. The overall significance test on the three slope coefficients of eq. (5) and their signs corroborate the findings retrieved from the model of equation (3). The specification test retrieved from the model of eq. (5) (i.e. Ho: $\omega_3 = \omega_6 = 0$)

aimed at checking the functional restrictions posed by eq. (3) is also in favor of accepting such restrictions[11].

6.2 Marginal Contribution of EZ Policy Features

Table 7 summarizes the results for the four specifications (V-VIII) of equation (3), estimated on the large sample of states, that have new-establishment outcome data as dependent variable and that fully include the EZ policy features interaction terms $\Sigma_{pol}\varphi_{pol}(EZ_{it}*pol_{it}*T_{it})$[12]. Zone designation and the portion of state land covered by zones are estimated to significantly affect the baseline outcome growth rate of all four specifications (V-VIII) [except for specification VIII –payroll per employee- in which only zone land coverage reaches statistical significance levels].

Similarly to the results of Table 6, zone designation is found to positively affect employment, value of shipment and capital expenditure outcomes accounted for by new-establishments (specifications V, VI and VII). Crucial for the effectiveness of EZ programs, however, is the overall size of the designated areas, with a significant marginal decrease of the 5-year baseline growth rate for each one-standard deviation increase in the land coverage of zones (-20.5; -17.5 and -14.6 percentage points for employment, value of shipments and capital expenditures, respectively). Designating a larger portion of land covered by zones is also estimated to lower the growth rate of payroll per employee (-43 percentage points on a 5-year base).

Table 7

Table 8 illustrates the impact estimates of the EZ status and the state-specific EZ policy features on existing-establishment outcomes for the large sample of states (specifications IX-XII)[13]. Estimates for the employment specification (IX) highlight that providing EZ incentives tied to the number of new jobs created is the only policy variable that marginally affect the employment baseline growth rate (with a positive marginal impact estimate of 22.2 percentage points on a 5-year growth rate). Tying incentives to hiring requirements is also found to be the only significant policy variable in the payroll

[11] Result tables for coefficient estimates and specification tests from all specifications of eq. (4) and (5) are omitted for the sake of brevity but are available upon request to the author.
[12] Results from the small-sample analysis, omitted from Table 7 for the sake of brevity, are in all very similar to those from the large sample of states and they are available upon request to the author.
[13] As in Table 7, results from the small-sample analysis are omitted for the sake of brevity. Results from the small sample of states are very similar to those illustrated in Table 7 and are available upon request to the author.

per employee specification (XII), showing, however, a negative marginal impact on the 5-year payroll-per-employee baseline growth rate (-31.2 percentage points). Mandating the provision of a strategic economic plan is the only policy feature significantly affecting the baseline growth rate of both value of shipments (specification X) and capital expenditures (specification XI), with positive marginal impact estimates of 25.8 and 17.5 percentage points, respectively.

Table 8

Results for the vanishing-establishment specifications (XIII-XVI), not reported for the sake of brevity[14], highlight that, consistently with the results of Table 6, EZ designation is found to marginally accelerate the rate-loss of employment, value of shipments and capital expenditures due to establishments that either cease all operations or move elsewhere from EZ areas. None of the EZ policy interaction terms added to the model, however, are found instead to be significant in any of the four estimated specifications.

All results reported in this section are robust in three ways. First they are corroborated by the results from the specifications estimated on the small sample of states that allows the TAIM estimates of the monetary value of the EZ incentives to be included in the analysis. Second, they endure replicating the analysis by coding ZIP areas as EZ ZIPs only if they have at least either 25%, 50% or 75% of their land covered by actual zones. Third, the restrictions posed by the chosen functional form (eq. 3) are accepted by the specification tests yielded from replicating the analysis with the models of eq. (4) and (5).

7. Conclusions and Policy Recommendations

This paper looked deep into the states' Enterprise Zone (EZ) experience of the mid eighties-early nineties to test whether or not different impacts of EZ programs may be detected by looking at employment, sales (measured as value of shipments), capital expenditures and payroll per employee growth outcomes sorted into different gross flows separately accounted for by the following three types of business units: new-establishments (those that operate for the first time at their present location); existing-establishments (those that are still active and were operating at their present location at a previous time), vanishing-establishments (those that ceased operations at the location where they were previously active). Testing whether EZ incentives affect in different ways different gross flows of local economic growth is important as some EZ may focus on the attraction of new establishments, while other EZs may aim predominantly at favoring the expansion or the retention of existing establishments. Knowing whether or not certain specific policy features are found to favor the attraction of new businesses instead of the retention of existing production activities (or vice versa) may be an important piece of information to refine future geographically-targeted tax incentive initiatives.

Results of the analysis show that when local employment, sales, capital expenditures and payroll per employee growth outcomes are sorted into gross flows separately accounted for by new-, existing- and vanishing-establishments, EZ incentives are found to have different significant impact on different types of flows. EZ policies are found to increase the growth of jobs, sales and capital expenditure brought by new establishments. This positive impact, however, is crucially affected by the size of the overall territorial extension of designated zones: states with a small territorial extension of zones attract more employment and economic activity from new establishments than states with a large EZ territorial extension. Changes in payroll per employee accounted for by new-establishments are instead negatively affected by the introduction of EZ incentives. Employment, sales and capital expenditures accounted for by existing establishment tend also to be positively affected by EZ policies, although by a smaller extent. For existing-establishment outcomes, however, tying zone incentives to job creation requirements is found to be crucial in order to promote employment growth, while requiring the submission of an economic development strategic plan as part of the EZ application process is crucial to promote growth in sales and capital expenditures. EZ policies, finally, are found to significantly accelerate the rate-loss of employment, sales, and capital expenditures accounted for by vanishing establishments.

In agreement with the empirical evidence of other recent econometric studies (Papke 1993, Boarnet and Bogart 1996, Dowall 1996, Bondonio and Engberg 2000), mean impact estimates of the EZ incentives are instead found not to be statistically significant on any measure of net local economic growth. There are different possible explanations for these findings. New businesses could simple increase the rate at which previously existing businesses leave the area (Greenbaum 1998). New jobs and new economic activity are much more headline grabbing than retention or savage of existing jobs and economic activity. New economic activity is quickly used to emphasize the merit of EZ programs, while business closures are often unlikely to be linked to zone designation. As a result, zone incentives may tend to be marketed more toward attracting new establishments than toward helping struggling existing ones.

[14] Results for such specifications are available upon request to author.

The greatest relevance of these findings, however, is perhaps related to the opportunity offered by the analysis to empirically test a number of predictions on recommended incentive features for geographically targeted economic development initiatives that stem from economic theory and/or interviews with business representatives and EZ program officials. Such predictions can be summarized as follows.

I) 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 capital for labor (Papke 1993). Programs that tie tax incentives awarded to zone businesses to the number of new jobs created, for example, can be viewed as more effective in promoting local employment growth than programs that tie incentives to capital investments (Wilder and Rubin 1996). Moreover, tax incentives are expected to appeal more to established businesses than to start-ups, since new businesses do not typically expect to make profits in the first years of operation (Sheldon and Elling 1989). Thus, programs that ties tax incentives to the creation of new jobs can be expected to benefit zone employment when the target of zone designation are mostly existing establishments. The result of the analysis presented in the paper support this prediction showing that tying incentives to new jobs created is the only EZ feature that marginally increase the baseline employment growth of existing EZ firms. Tying incentives to new jobs is instead not found to have a significant impact on employment neither for new firms, nor for outcomes other than employment. As EZ incentives are also not found to boost payroll per employee, the results of the analysis suggest that the new jobs created tend to be not more qualified than existing jobs.

II) The provision of a strategic local economic development plan among the program application requirements may be crucial for effectively promoting zone employment and economic growth. California program officials, interviewed by Bostic (1996), for example, revealed that the strategic planning portion of the application process was important to organize local development resources in a more productive way. In particular, mandating the submission of a strategic plan is viewed to be a key feature of EZ policies in order to effectively support existing firms. The reason being that the development of a EZ strategic plan should often gather together local businesses with different administrative and community branches, making existing local businesses best aware of the opportunities offered by the EZ incentive package.

Robust empirical support is offered to this prediction by the results of the analysis showing that the requirement of a strategic plan as part of the zone application process is beneficial to induce growth in sales and capital investment of existing establishments. The strategic plan requirement, instead, does not significantly affect the attraction of new economic activity brought by new businesses.

III) According to program officials, the territorial extension of designated zones is the most relevant factor in boosting zone employment and economic activity brought by the attraction of new firms (Bostic 1996). In particular, EZ programs are viewed to be more successful in attracting new business if they adopt a competitive zone selection process that yields few designated EZs of limited size. EZ programs with limited zone territorial extension are advocated for three reasons. First, they allow to devote more intense marketing efforts to each single zone (Wilder and Rubin 1996). Second, they enable program officials to better evaluate the potential comparative advantage of the different eligible areas, yielding to the designation of areas that have developed the strongest local support for economic growth. Third, they facilitates a close program monitoring and evaluation.

The results of the analysis support this prediction by showing that the baseline employment and economic growths accounted for by new establishments are positively affected by the reduction of the overall zone territorial extension. Reducing the territorial extension of the program is also found to increase the attraction of firms that offer higher levels of payroll per employee, suggesting that efforts to attract new businesses with qualified job opportunities are more successful if the size of the program is limited.

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Table 1. Policy Features of EZ Programs

Policy feature	*Description*	*Measure*	*CA* [(a)]	*CT*	*DC*	*FL*	*IN*	*KY*	*MD*	*NJ*	*NY*	*PA*	*VA*
Size of EZ program (EZ_SIZE) [(b)]	Size of the program in terms of percentage of the total state land occupied by EZs	% of state land occupied by Ezs	1.26 3.45	0.8 2.75	0 14.55	0 9.33	1.67 3.62	2.51 3.22	4.79 10.03	6.59 6.59	0 2.31	0.21 6.46	5.37 7.91
Strategic plan required (EC_PLAN)	EZ designation follows a competitive application process requiring the submission of a strategic economic development plan by prospective EZ communities	1=yes 0=no	0	0	0	0	1	0	1	1	1	1	1
Tax incentives proportional to new jobs (JOB_REQ)	Program tax incentives based on the number of new jobs created by EZ firms	1=yes 0=no	1	1	0	0	1	1	1	1	0	0	0
Tax Incentives propotional to new capital investment (CAP_REQ)	Program tax incentives based on the size of the new capital expenditures promoted by EZ firms	1=yes 0=no	0	1	0	1	1	1	0	0	1	0	0
Monetary value to businesses of the EZ incentive package (EZ_VAL)	%Points difference in the Internal Rate of Return (IRR) between typical firms' investing in a new plant in EZ and non-EZ locations within the same state (c)	ΔIRR %point difference (1=1%)	0,115	-	-	-	-	0,217	-	-	0,183	0,229	0,735

Notes:

(a) California has two EZ programs that, respectively, established the Enterprise Zones and the Employment and Economic Incentive Areas. Since the two programs do not differ from each other in the policy dimensions considered in this paper, they are considered as a single program.

(b) As the geographic size of the EZ programs typically expands over time, un upper and lower boundaries are offered to measure the % of the state land occupied by EZs in 1992 and in 1987.

(c) ΔIRR values varies across two-digit SIC industrial sectors. The Values reported in Table 1 are state-specific average figures obtained by weighting each two-digit SIC specific estimate by the proportion of establisments operating in the state in that industrial sector prior to the start of the EZ program. Missing values for CT, DC, FL, IN, MD and NJ are due to the fact that at the time of the analysis the available version of the TAIM software did not include those states.

Table 2 Program Starting Dates and Zones by State

	CA [b]	*CT*	*DC*	*FL*	*IN*	*KY*	*MD*	*NJ*	*NY*	*PA*	*VA*
Program starting date	1986	1982	1987	1987	1984	1983	1983	1985	1987	1983	1984
EZ ZIPs [a]	125	17	2	91	37	46	16	20	41	121	60

Notes:

(a) Number of ZIP code areas encompassing any portion of EZ areas in existence at 12/31/1992

(b) For California, the number of zones and zone ZIPs combines the counts for both the Enterpise Zones and the Employment and Economic Incentive Areas

Table 3. Descriptive Statistics of Pre-Designation ZIPs' Characteristics (Sample of 11 states)

Variable		Mean (S.D.) Zone ZIPs (EZ=1)	Non-zone ZIPs (EZ=0)
Unemplyment rate	UNEMP	0.048	0.042
		(0.018)	*(0.027)*
Per capita income (in $1,000s)	INC	5.968	7.253
		(1.554)	*(2.149)*
Poverty rate	POV	0.179	0.106
		(0.103)	*(0.068)*
Population density (1,000 people per km2)	DENS	1.856	1.015
		(2.651)	*2.957*
Percentage of population black or hispanic	MIN	0.307	0.111
		(0.294)	*(0.174)*
5-year employment growth rate [ln(Yt/Yt-5)]	EMPGRW	-0.008	0.235
		(0.732)	*(0.997)*
5-year growth for number of establishment [ln(Yt/Yt-5)]	ESTGRW	0.561	0.799
		(0.777)	*(1.189)*
Percentage of occupied units over total number of housing units	OCCHOUS	0.877	0.893
		(0.057)	*(0.107)*
Average value of owner occupied houses (in $1,000s)	VALHOUS	48.710	60.611
		(24.367)	*(32.064)*

S.D. are in parentheses

Table 4. Model specifications

Dependent variable [ln(Y_t/Y_{t-5})]		*Specification*	EZ treatment variables included: *Sample of 11 states*	EZ treatment variables included: *Sample of 5 states*
A. Equations with no EZ policy interaction terms				
Net growth outcomes				
Employment	*EMP*	*I*		
Value of shipments	*SHIP*	II		
Capital expenditures	*CAP*	*III*		
Payroll per employee	*PAYR*	*IV*		
Outcomes due to new establishments				
Employment	*EMP_NEW*	*V*		
Value of Shipments	*SHIP_NEW*	VI		
Capital expenditures	*CAP_NEW*	*VII*		
Payroll per employee	*PAYR_NEW*	*VIII*		
Outcomes due to existing establishments			EZ*T	EZ*T
Employment	*EMP_EXT*	*IX*		and
Value of Shipments	*SHIP_EXT*	X		EZ*T*EZ_VAL
Capital expenditures	*CAP_EXT*	*XI*		
Payroll per employee	*PAYR_EXT*	*XII*		
Outcomes due to vanishing establishments				
Employment	*EMP_VAN*	*XIII*		
Value of Shipments	*SHIP_VAN*	XIV		
Capital expenditures	*CAP_VAN*	*XV*		
Payroll per employee	*PAYR_VAN*	*XVI*		
B. Equations with EZ policy interaction terms				
Net growth outcomes				
Employment	*EMP*	*I*		(EZ*T)
Value of shipments	*SHIP*	II	(EZ*T)	and
Capital expenditures	*CAP*	*III*		(EZ*T*EZ_VAL)
Payroll per employee	*PAYR*	*IV*		
Outcomes due to new establishments				
Employment	*EMP_NEW*	*V*		Specs. replicated
Value of Shipments	*SHIP_NEW*	VI		separately including one of
Capital expenditures	*CAP_NEW*	*VII*	(EZ*T)	the following groups of terms
Payroll per employee	*PAYR_NEW*	*VIII*	and	a) (EZ*T), (EZ*T*EZ_VAL),
Outcomes due to existing establishments			(EZ*T*EZ_SIZE)	(EZ*T*EZ_SIZE)
Employment	*EMP_EXT*	*IX*	and	
Value of Shipments	*SHIP_EXT*	X	(EZ*T*EC_PLAN)	b) (EZ*T), (EZ*T*EZ_VAL),
Capital expenditures	*CAP_EXT*	*XI*	and	(EZ*T*EC_PLAN)
Payroll per employee	*PAYR_EXT*	*XII*	(EZ*T*JOB_REQ)	
Outcomes due to vanishing establishments			and	c) (EZ*T), (EZ*T*EZ_VAL),
Employment	*EMP_VAN*	*XIII*	(EZ*T*CAP_REQ)	(EZ*T*JOB_REQ)
Value of Shipments	*SHIP_VAN*	XIV		
Capital expenditures	*CAP_VAN*	*XV*		d) (EZ*T), (EZ*T*EZ_VAL),
Payroll per employee	*PAYR_VAN*	*XVI*		(EZ*T*CAP_REQ)

Table 5. Probability of zone designation
-Probit estimates from Equation (1) and (2)
(Results form the 11-state sample)

Variables[(a)]		Cluster I (CA, CT, DC, KY, NJ, PA)	Cluster II (FL, IN, MD, NY, VA)
Unemployment rate	UNEMP	-1.888	-0.286
		(2.177)	*(1.202)*
Poverty rate	POV	1.104	**2.129*** **
		(0.721)	***(0.814)***
Per capita income (in $1,000s)	INC	**-0.084*** **	0.042
		(0.031)	*(0.044)*
Population density (1,000 people per km2)	DENS	**0.091*** **	-0.013
		(0.019)	*(0.014)*
Proportion of population black or hispanic	MIN	**1.579*** **	**1.387*** **
		(0.225)	***(0.229)***
Employment growth	EMPGRW	**-0.127** **	**-0.128** **
		(0.063)	***(0.058)***
Establishment growth	ESTGRW	0.017	0.049
		(0.110)	*(0.101)*
Proportion of number of occupied units over total number of housing units	OCCHOUS	-	**-1.437*** **
			(0.387)
Average value of owner occupied housing units (in 1,000$)	VALHOUSE	-	**-0.008** **
			(0.003)
Number of observations		3068	2828
Pseudo R2		0.1785	0.1736
Log likelihood		-830.42	-671.04

* 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 on the state dummies are not reported. The complete list of regression results is available upon request

Table 6. Impact Estimates of Zone Designation on Five-Year Growth Rates. Results from Equation (3) [without policy interaction terms]

Dependent variable (5-year growth rates) [(a) (b)]	Model spec.	11-state sample [(c)] Impact of EZ designation	5-state sample [(d)] Impact of EZ designation	5-state sample [(d)] Mon. Val. of EZ incent.
Unsorted Outcomes				
Employment (EMP)	(I)	0.019 (0.033)	0.031 (0.067)	0.047 (0.057)
Value of shipments (SHIP)	(II)	0.012 (0.042)	0.014 (0.019)	0.053 (0.067)
Capital expenditures (CAP)	(III)	0.035 (0.052)	0.024 (0.019)	0.036 (0.031)
Payroll per employee (PAYR)	(IV)	-0.034 (0.037)	-0.074 (0.077)	0.222 (0.219)
New-establishment Outcomes				
Employment (EMP_NEW)	(V)	**0.252*** (0.080)**	**0.293* (0.167)**	0.260(0.491)
Value of shipments (SHIP_NEW)	(VI)	**0.199** (0.101)**	**0.191* (0.102)**	0.101 (0.216)
Capital expenditures (CAP_NEW)	(VII)	**0.191* (0.099)**	**0.103** (0.036)**	0.233 (0.205)
Payroll per employee (PAYR_NEW)	(VIII)	**-0.324*** (0.067)**	**-0,356*** (0,138)**	-0,006 (0,408)
Existing-establishment Outcomes				
Employment (EMP_EXT)	(IX)	**0.067*** (0.028)**	**0.032*** (0.010)**	0.146 (0.103)
Value of shipments (SHIP_EXT)	(X)	**0.057*** (0.028)**	**0.061** (0.029)**	0.102 (0.101)
Capital expenditures (CAP_EXT)	(XI)	**0.076** (0.046)**	**0.041*** (0.016)**	0.156 (0.106)
Payroll per employee (PAYR_EXT)	(XII)	-0.031 (0.040)	-0.099 (0.083)	0.290 (0.236)
Vanishing-est. Outcomes (marginal contribution to rate-losses)				
Employment (EMP_VAN)	(XIII)	**0.192** (0.087)**	**0.205** (0.101)**	-0.134 (0.106)
Value of shipments (SHIP_VAN)	(XIV)	**0.148* (0.108)**	**0.174* (0.103)**	-0.110 (0.091)
Capital expenditures (CAP_VAN)	(XV)	**0.160* (0.103)**	**0.183 * (0.104)**	-0.098 (0.082)
Payroll per employee (PAYR_VAN)	(XVI)	-0.102 (0.070)	-0.093 (0.145)	0.115 (0.248)

* p-value<0.1 ** p-value<0.05 *** p-value<0.01

(a) For clarity of exposition the coefficient estimates on the state dummies, the propensity scores, and the (1982-87) five-year period dummy are not reported. The complete regression results are available upon request

(b) Prob>F = 0.000 (Ho: All coefficients =0) for all specifications.

(c) N=11,766 for specifications (I-IV); N=8,284 (V-VIII); N=11,046 (IX-XII); N=7,622 (XIII-XVI).

(d) N=7,852 for specifications (I-IV); N=5,368 (V-VIII); N=7,352 (IX-XII); N=5,086 (XIII-XVI).

Table 7. Marginal Impact of EZ policy Features on New-Establishment Outcomes
Results from Specifications (V-VIII), Eq. (3)

Independent Variables[a]		Specification[+] [Dep. Variable: 5-year growth rate]			
		(V) Dep.var. EMP_NEW	(VI) Dep.var. SHIP_NEW	(VII) Dep.var. CAP_NEW	(VIII) Dep.var. PAYR_NEW
ZONE DESIGNATION	(EZ*T)	**0.692****	**0.756***	**0.687***	0.299
Portion of 5-years with active EZ		**(0.322)**	**(0.406)**	**(0.397)**	(0.271)
ZONE POLICIES					
Strategic economic plan	EC_PLAN	0.134	0.025	-0.087	-0.478
		(0.192)	(0.242)	(0.236)	(0.361)
Tax incent. tied to job creation	JOB_REQ	-0.308	-0.352	-0.426	-0.042
		(0.192)	(0.241)	(0.236)	(0.161)
Tax incent. tied to capital expend.	CAP_REQ	0.025	-0.164	0.053	-0.198
		(0.177)	(0.222)	(0.217)	(0.148)
Portion of state land covered by EZs	EZ_SIZE	**-0.730****	**-0.641****	**-0.535***	**-0.493****
[1=10% increase]		**(0.315)**	**(0.296)**	**(0.307)**	**(0.234)**

* p-value<0.1 ** p-value<0.05 *** p-value<0.01. Standard errors are in parentheses
N=8,282. Adj. R-sq. : 0.3306 for specification (V); 0.3051 (VI); 0.3037 (VII); 0.2042 (VIII).
Prob>F = 0.000 (Ho: All coefficients =0) for all specifications.
(a) For clarity of exposition the coefficient estimates on the state dummies, the two propensity score variables and the 1982-87 dummy are not reported. The complete list of regression results is available upon request to the author.
(+) F-tests of joint significance has been performed for all of the variables with coefficient estimates having p-values>0.10. For all four of the specifications F-test results lead to accept the null hypothesis of non-significance

Table 8. Marginal Impact of EZ policy Features on Existing-Establishment Outcomes Results from Specifications (IX-XII), Eq. (3)

Independent variables[a]		Specification[+] [Dep. Variable: 5-year growth rate]			
		(IX) Dep.var. EMP_NEW	(X) Dep.var. SHIP_NEW	(XI) Dep.var. CAP_NEW	(XII) Dep.var. PAYR_NEW
ZONE DESIGNATION	(EZ*T)	0.071	-0.083	0.235	-0.076
Portion of 5-year period with active EZ		(0.156)	(0.195)	(0.270)	(0.165)
ZONE POLICIES					
Strategic economic plan	EC_PLAN	-0.060	**0.258****	**0.175***	0.165
		(0.092)	**(0.116)**	**(0.060)**	(0.097)
Tax incentives tied to job creation	JOB_REQ	**0.222****	0.017	-0.110	**-0.312*****
		(0.092)	(0.115)	(0.159)	**(0.097)**
Tax incentives tied to capital expenditures	CAP_REQ	-0.043	0.054	0.105	0.058
		(0.085)	(0.107)	(0.148)	(0.090)
Portion of state land covered by zones	EZ_SIZE	0.057	0.145	-0.111	0.121
[1=10% increase]		(0.150)	(0.188)	(0.261)	(0.159)

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

N=11,046. Adj. R-sq. : 0.3229 (IX); 0.3293 (X); 0.3206 (XI); 0.2104 (XII).

Prob>F = 0.000 (Ho: All coefficients =0) for all specifications.

(a) For clarity of exposition the coefficient estimates on the state dummies, the two propensity score variables and the 1982-87 dummy are not reported. The complete list of regression results is available upon request to the author.

(+) F-tests of joint significance has been performed for all of the variables with coefficient estimates having p-values>0.10. For all four of the specifications F-test results lead to accept the null hypothesis of non-significance