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Do local tax incentives affect economic growth? What mean impacts miss in the analysis of enterprise zone policies

Daniele Bondonio[a,*], Robert T. Greenbaum[b]

[a] *Department of Public Policy and Public Choice, Università del Piemonte Orientale, Via Cavour, 84, 15100 Alessandria, Italy*
[b] *John Glenn School of Public Affairs, The Ohio State University, USA*

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Abstract

This paper exploits the exogenous variation of U.S. state enterprise zone policies to estimate the impact of geographically-targeted tax incentives on a number of dimensions of local economic growth. The econometric analysis uses establishment-level data to sort out growth outcomes into gross flows separately accounted for by new, existing, and vanishing establishments in the target areas. Results offer empirical evidence with strong external validity to support specific policy recommendations and show that the impacts of the incentives have more complex dynamics than those revealed by the null mean impact estimates obtained from analyzing net growth outcomes.

JEL classification: R5; O1; C23
Keywords: Tax incentives; Local economic growth; Enterprise zones; Program evaluation; Gross flows

1. Introduction

Since in the early 1980s, a substantial majority of US states have implemented geographically targeted economic revitalization programs, mainly referred to as enterprise zone (EZ) programs. A quarter-century later, state legislatures are revisiting the programs in order to decide whether and how to proceed with the programs. Unfortunately, findings from the existing enterprise zone literature are difficult to translate into useful policy recommendations, and conflicting results limit

* Corresponding author. Tel.: +39 011 533191; fax: +39 0131 283704.
E-mail address: daniele.bondonio@sp.unipmn.it (D. Bondonio).

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the ability to generalize them. Studies have used various methodologies ranging from descriptive case studies to careful econometric evaluations. Many of the evaluations have examined programs in only one state, and a few have evaluated only a single zone. Findings are particularly hard to generalize because of the variety of EZ approaches and local economic conditions, and their usefulness is further limited because most impact evaluations only consider the presence or absence of the program in a particular time and place rather than taking into account the policy features of the zone or the generosity of tax incentives (Peters and Fisher, 2002).

This paper contends that an effective way to evaluate state EZ programs is to use establishment-specific panel data across multiple states to comparatively estimate the impact of state-specific policy implementation features on gross flow measures of economic growth. State programs vary considerably in terms of designation criteria, geographic size of the program, and incentives offered. This variation is a precious resource for evaluation analysis, as it stems from political decisions that are likely uncorrelated with future economic trends in the target areas. These natural experiment conditions are conducive to comparative analysis to test the effectiveness of different implementation features as best practices for future business incentive interventions. EZ areas are also small enough that appropriate comparison areas can be found within the same regional economies. Thus, program impacts can be estimated from empirical models that control for factors contributing to the observed outcomes that are independent from the program intervention. State EZ programs therefore have the potential to offer valuable empirical evidence, with strong external validity, on the effectiveness of geographically targeted business incentives and to contribute evidence regarding the longstanding debate on the effect of tax differentials on firm location and investment decisions.

By making use of the policy heterogeneity across 11 different state programs and taking advantage of longitudinal plant level manufacturing data from the U.S. Census Bureau, this paper moves beyond the measurement of net employment changes due only to the presence or absence of a program. Not only are net changes in the economic outcomes of employment, capital expenditures, sales, and payroll per employee estimated, but the models developed also estimate the marginal impact of specific EZ policy features on the baseline growth rates of those four different economic measures separately sorted into outcomes accounted for by new, existing and *vanishing* establishments, business establishments that have either closed or moved. Net changes can mask the churning of establishment births and deaths that are common in both growing and shrinking economies (Davis et al., 1996; Dunne et al., 1989), and sorting growth outcomes into the gross flows is important to properly account for the impacts of different policy features. For example, testing whether particular policy features favor the attraction of new businesses versus the retention of existing production activities can help better align policy choices with their indened outcomes.

The reminder of the paper is organized as follows. Section 2 discusses recent empirical literature on zone effectiveness, and Section 3 describes the ZIP code level programmatic and outcome data. Section 4 describes the two-step econometric model that estimates impacts that are reasonably free of selection and omitted variable biases under plausible assumptions on the selection into treatment process. Section 5 presents the results, and Section 6 discusses the major findings, which highlight that while zone incentives do not have statistically significant net outcomes, separation of the various outcome measures into the distinct gross flows reveals a complex underlying dynamic. Positive zone impacts on new and existing establishments are offset by induced losses due to vanishing establishments. The analysis of the policy features indicates that restricting the geographic extent of the programs helps increase business activity due to new establishments, and requiring strategic planning and tying incentives to job creation has some positive impacts on existing establishments. Section 7 concludes the paper.

2. State enterprise zones empirical literature

Despite the wide popularity of EZ programs, which, in broad terms, target tax breaks and economic development incentives to specific economically distressed areas, the available empirical evidence on their effectiveness at creating local economic growth is still inconclusive (Boarnet, 2001). As reviewed by Wilder and Rubin (1996), early evaluation studies from the mid 1980s were mostly descriptive case studies that focused on measuring economic activity in single zone areas at pre-to post-designation periods, with no attempt to empirically separate effects of zone designation from those from other exogenous economic trends. A number of more recent studies have used econometric models drawing upon empirical evidence from a single state's programs (e.g., Boarnet and Bogart, 1996; Lambert and Coomes, 2001; O'Keefe, 2004; Papke, 1993, 1994; Rogers and Tao, 2004). While these studies have the advantage that they are reasonably free from biases due to different initial conditions between the target areas and the rest of the national and regional economy, evidence from a single state does not generalize the other places or times due the extensive heterogeneity of the state EZ programs.

To provide results with larger external validity, some recent studies have begun to utilize longitudinal US Census Bureau business data to perform comparative econometric evaluation studies across states. Starting with Bondonio and Engberg (2000), the studies all point toward a negligible net mean impact of zone designation. Bondonio (2002) and Greenbaum and Engberg (2004), however, suggest that the zero mean 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 acceleration of downsizing and closure of existing firms. This consistent with earlier findings that any positive impact from zones is mainly attributable to new firms and expansions (Erickson, 1992). Peters and Fisher (2002) also analyze the composition of economic growth and decline in EZ areas with a descriptive multi-state analysis of the flows of establishment births, deaths and relocations into and out of zones and find that across 64 zones, the average zone added fewer new manufacturing establishments than it lost.

This paper builds on these studies by investigating impacts accounted for by state-specific policy features. By going beyond mean impact estimates, the paper tests which specific policy features best attract new business and which better retain establishments already operating the assisted areas. This type of evidence is what is most needed to refine future policy interventions, not only in the US, but also in other countries, such as those in the EU, where geographically-targeted business incentives still play a very important role as tools for economic development. Therefore, it is particularly important to examine the different outcomes in the context of the different policy features. The next section further describes these features and the data utilized to measure programmatic outcomes.

3. Data

Outcomes from ten states (California, Connecticut, Florida, Indiana, Kentucky, Maryland, New Jersey, New York, Pennsylvania, and Virginia) and the District of Columbia are examined for the period 1982 to 1992. These particular programs were selected based on the following criteria: location of zones in urban distressed areas and/or in industrial parks near urban areas; limited geographic extension of the program, and competition among local communities to receive the EZ status. The state-specific policy features of the EZ incentive packages offered in the 11 programs can be summarized in five variables that facilitate empirical testing of the impacts of unique policy features. These policy variables, collected from official state and federal documents (e.g., HUD,

1997) and interviews with zone officials, measure zone size, strategic planning, employment and capital investment requirements, and monetary value of the incentives.[1]

The variable EZ_SIZE measures the fraction of each state's total land area comprised of enterprise zone ZIP codes. This variable is used to test whether program size affects zone effectiveness, as there is some evidence that programs with fewer zones have been more successful (Erickson, 1992; Erickson and Friedman, 1990, 1991; Wilder and Rubin, 1996). Because zone sizes vary greatly, we prefer to measure relative land area designated rather than the number of zones. The fraction of land designated grew over time and by 1992 ranged from 2.3% in New York to 14.6% in the District of Columbia.

EC_PLAN is a dichotomous variable that measures whether the submission of a strategic economic development plan is a prerequisite for zone selection. Six programs require development of such a plan, which compels the coordination among local economic development parties. Some argue that such strategic planning can lead to better zone outcomes (Wilder and Rubin, 1996). JOB_REQ is a dichotomous variable that indicates whether creating new employment is a prerequisite for receiving tax incentives, and CAP_REQ similarly measures whether zone incentives are tied to new capital expenditures. Six of the 11 programs have the jobs requirement, and five of the programs require evidence of capital expenditures.

Finally, EZ_VAL controls for the marginal impact of the monetary value of each state's EZ incentives on economic growth. The measure is calculated using the hypothetical firm approach embedded in the TAIM software developed by Fisher and Peters (1998).[2] 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 TAIM of an 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 firms in a non-EZ area within the same state. Calculating this within-state difference is useful because the monetary value of local tax incentives is widely believed to affect firm' location decisions primarily at the margin among similar nearby locations (e.g., Bartik, 1991; Bostic, 1996; Wilder and Rubin, 1996). The value of the incentives ranged from a 0.12% difference in the IRR in California to a more generous 0.74% difference in Virginia.

The location of zones is coded in terms of U.S. Postal ZIP codes. ZIP codes are a useful geographic size for measuring localized business activity because they can capture local demand factors such as population, income and wealth (Bingham and Zhang, 2001). ZIP codes are also the smallest geographic unit for which detailed business data is available. While zone boundaries do not coincide with ZIP codes, they do not typically coincide with any other administrative boundaries, either. Therefore, a ZIP area is coded as an EZ ZIP if it includes any portion of an actual zone.[3]

Economic growth outcomes, in the form of employment, 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). The data, which include every U.S.

[1] The distribution of these variables across the 11 programs can be seen in Table 1 of Bondonio and Greenbaum (2005).

[2] While Peters and Fisher (2002) have expanded and extended the model ($TAIM^{EZ}$), the TAIM algorithm available for this analysis covered only five of the states, California, Kentucky, New York, Pennsylvania, and Virginia. Because of this, the outcome analysis was replicated for both the "large" sample of 11 states and the smaller sub sample of five states.

[3] Although this decision rule is not ideal because zone boundaries that overlap small portions of neighboring and less distressed ZIP codes might lead to an underestimation of the zones' levels of distress (Greenbaum, 2004), methods to apportion the boundaries to ZIP codes also have drawbacks (Dowall, 1996). Further, since EZ programs attempt to improve local economies, it makes sense to be able to test whether zone designation has an impact on both the EZ areas and their most immediate vicinities. Regardless, as we note below, we find that that the models are robust across alternative decision rules regarding the overlap of zone boundaries and ZIPs.

Table 1
Descriptive statistics of pre-designation zip code characteristics: 11 state sample

Measure	Variable	Mean (S.D.)	
		Zone ZIPs	Non-zone ZIPs
Unemployment rate, 1980	UNEMP	0.048	0.042
		(0.018)	(0.027)
Poverty rate, 1980	POV	0.179	0.106
		(0.103)	(0.068)
Per capita income (in \$1000s), 1980	INC	5.968	7.253
		(1.554)	(2.149)
Population density (1000 people per km^2), 1980	DENS	1.856	1.015
		(2.651)	(2.957)
Percentage of population black or Hispanic, 1980	MIN	0.307	0.111
		(0.294)	(0.174)
Employment growth rate, 1977–1982	EMPGRW	−0.008	0.235
		(0.732)	(0.997)
Establishment growth rate, 1977–1982	ESTGRW	0.561	0.799
		(0.777)	(1.189)
Percentage of occupied housing units (occupied units/total units), 1980	OCCHOUS	0.877	0.893
		(0.057)	(0.107)
Average value of owner occupied houses (in \$1000), 1980	VALHOUS	48.710	60.611
		(24.367)	(32.064)

Standard deviations are in parentheses.

manufacturing plant with five or more employees in years 1977, 1982, 1987, and 1992,[4] allow pre–post intervention economic growth measures to be properly sorted into gross flows accounted for by new (establishments operating for the first time at their present location), existing (establishments operating at the same location as in the previous CM panel) and vanishing establishments (which are no longer in operation in the same location in the subsequent CM panel).

Finally, pre-designation demographic, income, poverty, unemployment and population density ZIP code area characteristics are retrieved from the 1980 Decennial Census STF3a files and allocated to ZIPs using Mable/Geocorr (Blodgett and CIESIN, 1998). Table 1 reports that ZIP codes that subsequently were designated as EZs had in 1980, on average, higher unemployment rates, poverty rates, percentage of minority residents, population density, and lower per capita income than ZIPs that were never designated. These ZIPs also had slower growth rates of employment and business establishments between 1977 and 1982, and they had fewer occupied housing units and lower average housing values in 1980.

4. Method of analysis

Because conducting randomized experiments is impossible, EZ evaluations should carefully compare outcomes in EZs to those in a control group of places that that are similar but did not receive a zone in order to be able to measure the counterfactual growth rates of zones had those places not been designated (Boarnet, 2001). When measuring the impact of substantial incentive packages to very large firms, it may be possible to use the runner-up locations as the counterfactual (Greenstone and Moretti, 2004). However, most firms that respond to EZ incentives are too small to be the subjects of extensive bidding wars, and information regarding runner-up locations is not

[4] At the time of the analysis, the 1992 data were the most recent available.

available. Instead, impact estimates of the EZ incentives are retrieved through a "two-step conditioning on a propensity score" baseline model that has been recently used in other EZ evaluation studies (e.g., Bondonio and Engberg, 2000; Engberg and Greenbaum, 1999). The model combines both a propensity score approach to evaluation (e.g., Rosembaum and Rubin, 1983, 1984) and a panel-data fixed effects approach that eliminates the time invariant unobserved ZIP-specific characteristics which may be potentially correlated with treatment assignment.

In the first step, the probability that each ZIP is designated as an EZ is estimated in a probit regression as a function of the pre-designation employment and establishment growth and the 1980 socio-economic characteristics. Designation probabilities are estimated by a separate regression for each of two clusters of states selected by the criteria for zone selection. The first cluster (California, Connecticut, the District of Columbia, Kentucky, New Jersey, and Pennsylvania) shares zone selection guidelines that primarily include income, unemployment or poverty indicators. In addition to these same indicators, the second cluster (Florida, Indiana, Maryland, New York, and Virginia) also shares designation criteria that include measures of land availability or building vacancy. For this second cluster, the probit specification also adds two 1980 housing market variables. The probit regressions for the two clusters of states are illustrated in Eqs. (1) and (2):

$$P(\mathrm{EZ}_i = 1) = \Phi(\Sigma_c \beta_c \mathrm{CEN}_i^c + \Sigma_g \eta_g \mathrm{GRW}_i^g + \varphi_I), \tag{1}$$

$$P(EZ_i = 1) = \Phi(\Sigma_c \beta_c \mathrm{CEN}_i^c + \Sigma_g \eta_g \mathrm{GRW}_i^g + \Sigma_h \lambda_h \mathrm{HOUSE}_i^h + \varphi_{II}), \tag{2}$$

where:

i = ZIP
$\Sigma_c\mathrm{CEN}_i^c$ = set of 1980 Census variables capturing unemployment, poverty, per capita income, population density and percentage of African Americans and Hispanics
$\Sigma_g\mathrm{GRW}_i^g$ = set of 1977–1982 CM measures of employment and establishment growth
$\Sigma_h\mathrm{HOUSE}_i^h$ = set of 1980 Census variables expressing the percentage of occupied houses and the average value of owner occupied houses
ϕ_{I} = set of state 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 state dummies for cluster II: (FL, IN, MD, NY, VA for the large-sample analysis; NY and VA for the small-sample analysis)

The predicted probabilities in Eqs. (1) and (2) are referred to as propensity scores and can be interpreted as single ZIP-specific parameters that summarize all of the pre-intervention economic growth and socio-economic measures in a single index.

In the second step of the model, the predicted probabilities are inserted into the 5-year growth rate outcome regression of Eq. (3):

$$\mathrm{Ln}(Y_{it}/Y_{it-5}) = \alpha \mathrm{EZ}_{it} * T_{it} + \Sigma_{\mathrm{pol}} \varphi_{\mathrm{pol}} (\mathrm{EZ}_{it} * \mathrm{pol}_{it} * T_{it}) + \Sigma_c \delta_c \mathrm{PR}_i^c + \varphi_j + \gamma_t + u_{it} \tag{3}$$

Where:

t = 1987, 1992[5]
i = ZIP

[5] The CM data are available at 5-year intervals beginning in 1977. Because data for 1972 are unavailable, it is impossible to categorize establishments in 1977. For this reason, the 5-year periods available for the analysis are only 1982–1987 and 1987–1992. Therefore, t is either 1987 or 1992.

c = cluster of states I or II
j = state
pol = set of the following policy variables: EZ_SIZE (fraction of each state's total land area that comprised ZIP codes containing enterprise zones); EC_PLAN (=1 if the submission of a strategic economic development plan is a prerequisite for zone selection; =0 otherwise); JOB_REQ (=1 if tax incentives are tied to creating new employment; =0 otherwise); CAP_REQ (=1 if zone incentives are tied to new capital expenditures; =0 otherwise); EZ_VAL (monetary value of EZ incentives to businesses)
$\mathrm{Ln}(Y_{it}/Y_{it})$ = outcome growth rate in the 5-year period ending in year t
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} = $[(t-t_i^d)/5]$ = portion of the 5-year period ending in year t in which ZIP i contained an active EZ where t_i^d = year of EZ designation in ZIP i
$\Sigma_{\mathrm{pol}}(\mathrm{EZ}_{it} * T_i * \mathrm{pol}_{it})$ = 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 variables
$\Sigma_c\ \mathrm{PR}_i^c$ = predicted probabilities from Eqs. (1) and (2)
ϕ_j = set of state dummies
γ_t = 5-year period dummy [=1 if the $(t-(t-5))$ period is 1982–87; =0 otherwise]
u_{it} = normally distributed error term

In Eq. (3), the program intervention is operationalized with a treatment status variable weighted by the portion of the 5-year period in which the EZ was actually active ($\mathrm{EZ}_{it} * T_{it}$) and the set of policy terms interacted with the EZ status with each of the five state-specific policy variables and the portion active ($\Sigma_{pol}(\mathrm{EZ}_{it} * \mathrm{pol}_{it} * T_{it})$). These interaction terms are included in the model specifically to test whether state-specific EZ policy features have an impact on the dependent variable. The propensity scores PR^{I} and $\mathrm{PR}^{\mathrm{II}}$ are the estimated predicted probabilities from Eqs. (1) and (2) for all the ZIPs located within the states in cluster I or cluster II.

The two propensity scores control for all of the observed ZIP-specific pre-intervention growth trends and socio-economic characteristics. As shown in Table 1, EZ areas tend to have more disadvantaged socio-economic conditions and slower growth trends prior to the beginning of the programs than non-EZ areas. Such disadvantaged initial conditions could induce EZ areas to grow more slowly than non-EZ areas absent the program intervention. Without properly controlling for such differences, impact estimates of the program intervention could be wrong due to this selection bias. The model of Eq. (3) sorts out impacts due to observable differences in pre-intervention growth trends and socio-economic characteristics (coefficients $\delta_{\mathrm{I},}\ \delta_{\mathrm{II}}$) from the actual impacts due to the program intervention and the specific EZ policy features (coefficients α, φ).

Moreover, Eq. (3) results from long differencing an equation in which the independent variables are interacted with a baseline time-trend and regressed on levels of Y. Thus, all time-unvarying unobserved ZIP-specific pre-intervention characteristics (i.e., the panel data fixed effects, α_i) that may be correlated with treatment assignment do not pose any threats to the validity of the analysis, as they are purged by the long differencing that yields Eq. (3).

Compared to a standard panel data fixed effect estimation in which time-unvarying observed pre-intervention characteristics would have no role, the two-step model of Eqs. (1)–(3) yields unbiased impact estimates of the treatment under weaker assumptions than a panel data fixed effects estimator. Standard fixed effect estimation is unbiased only assuming that all of the time-unvarying pre-intervention ZIP specific characteristics, which may be correlated with treatment assignment, have the same impact on the future levels of the dependent variable. Our model, instead, allows unbiased impact estimates to be retrieved even if the observed ZIP-specific pre-intervention characteristics were correlated with treatment assignment and affect the dependent variable not just by impacting its future levels in the same way but also by impacting its future growth trend.

In a matching-estimator setting, Heckman et al. (1997) and Dehejia and Wahba (1999) show that better impact estimates can be retrieved by limiting the estimation sample to observations with propensity scores for which there are both treatment and comparison observations. We restrict the sample by excluding the ZIPs 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.[6]

4.1. Model specifications

The coefficient estimates for the model of Eq. (3) are retrieved for the four different economic growth measures (employment, shipments, capital expenditures, and payroll) across three different gross flows and overall net changes and account for heterogeneity by using robust cluster estimators that adjust the coefficient standard errors for the possible within-state correlation of observations (Rogers, 1993; Williams, 2000; Wooldridge, 2002). To compare the findings of this paper with those from other EZ studies, all 16 specifications are first estimated without including any of the policy interaction terms other than the monetary value of the incentives, which is included only in the small-sample analysis. The analysis is estimated differently for the large sample of 11 states and for the smaller sample of five states that have information on the monetary value of the incentives. For the larger sample, all four policy interaction terms that are not formed with the monetary value of the incentives are included in each specification. For the small-sample analysis, only two policy interaction terms are included at a time, one of which is always the monetary value of the EZ incentives. This is a choice made due to the limited variation in state-specific EZ policy features.

4.2. Specification tests and sensitivity analysis

Eq. (3) is restrictive in two major ways. First, it imposes that zone designation has the same impact on the growth outcomes in each 5-year period after designation without distinguishing between zones designated five years prior to time *t* and zones designated more than 5 years prior to time *t*. Second, it forces zone designation to affect the growth outcomes in each of the two 5-year 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. In the first model, the EZ status indicator (EZ_{it}) of Eq. (3) is replaced by a set of four EZ status variables that reflect the age of the enterprise zone at time *t* measured at two and three-year intervals. This model is more generalized than that of Eq. (3) because it allows zone impacts to

[6] The upper tail of high designation probability ZIPs are likely all EZs and have no suitable matches. The lowest probability ZIPs, likewise, are unlikely to ever be designated and also do not make for suitable comparisons. Only 116 out of a total sample of 5883 ZIPs were excluded from the analysis. Results of the sensitivity analysis show that impact estimates remain fairly stable when adopting alternative outlier exclusion thresholds or no exclusions at all.

vary freely 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 $t-5$.

In the second model, the EZ status indicator EZ_{it} is replaced by a set of three EZ status variables that reflect the portion of the 5-year period ending in year t in which a zone has been in existence measured from three different times, the actual year of zone designation and the portion of the five-year period measured from three and six years after the designation year. These three EZ status indicators allow the restrictions posed by the model of Eq. (3) to be tested directly. If the coefficients on the three and six year status variables are both zero, then the restrictions posed Eq. (3) hold.

Finally, robustness of the results is also tested by replicating the model of Eq. (3) adopting more stringent rules for sorting ZIP areas by EZ status. ZIP areas are coded as EZ ZIPs if at least 25, 50 or 75% of its land is covered by zones.

5. Results

Mean impact estimates of EZ status on economic growth outcomes sorted by gross flows accounted for by new, existing and vanishing establishments are reported in Table 2. The coefficient estimates can be interpreted as the mean marginal percentage point contribution to the baseline 5-year growth rate of zone designation and the generosity (monetary value) of the EZ packages. Consistent with much of the recent literature, mean impact estimates on the net economic growth outcomes (top portion of Table 2) indicate that the EZs have a modest and statistically insignificant net impact on net outcomes.

These results are misleading, however, because impacts vary across the gross flows. Among new establishments, employment, shipments and capital expenditure gross flow outcomes show a positive marginal contribution of EZ designation to the 5-year baseline growth rates. These impact estimates range from a 19.1 percentage point growth in capital expenditures to a 25.2 increase in employment growth for the analysis on the large sample of states and from 10.3 to 29.3 percentage points for the small sample analysis. Zone designation, by contrast, depresses the marginal baseline growth rate of the payroll per employee accounted for by the new establishments. The size of this negative impact is 32.4 and 35.6 percentage points for the large- and small-sample analysis, respectively. Impact estimates of zone 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, although these coefficient estimates are much smaller than those on new establishment outcomes, ranging from 5.7 for the growth in value of shipments to 7.6 percentage points for the growth in capital expenditures in the large-sample analysis and from 3.2 to 6.1 percentage points for the small sample analysis. No significant impact of EZ designation is estimated on the growth rate of payroll per employee accounted for by the existing establishments.

The final four specifications reported contain 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 analyses. EZ impact estimates for the payroll per employee specification do not reach statistical significance at conventional levels. As reported in the last column of Table 2, the estimated impacts of the monetary value of the EZ incentives in the small sample analyses have the expected signs but show large standard errors and are not significant.

Table 2
Impact estimates of zone designation on five-year growth rates: estimates from Eq. (3) without the policy interaction terms

Dependent variable (5-year growth rates)[a, b]		11-State sample[c]	5-State sample[d]	
		Impact of EZ designation	Impact of EZ designation	Mon. val of EZ incentive
Net growth outcomes				
Employment	EMP	0.019	0.031	0.047
		(0.033)	(0.067)	(0.057)
Value of shipments	SHIP	0.012	0.014	0.053
		(0.042)	(0.019)	(0.067)
Capital expenditures	CAP	0.035	0.024	0.036
		(0.052)	(0.019)	(0.031)
Payroll per employee	PAYR	−0.034	−0.074	0.222
		(0.037)	(0.077)	(0.219)
Outcomes due to new establishments				
Employment	EMP_NEW	0.252***	0.293*	0.260
		(0.080)	(0.167)	(0.491)
Value of shipments	SHIP_NEW	0.199**	0.191*	0.101
		(0.101)	(0.102)	(0.216)
Capital expenditures	CAP_NEW	0.191*	0.103**	0.233
		(0.099)	(0.036)	(0.205)
Payroll per employee	PAYR_NEW	−0.324***	−0.356***	−0.006
		(0.067)	(0.138)	(0.408)
Outcomes due to existing establishments				
Employment	EMP_EXT	0.067***	0.032***	0.146
		(0.028)	(0.010)	(0.103)
Value of Shipments	SHIP_EXT	0.057***	0.061**	0.102
		(0.028)	(0.029)	(0.101)
Capital expenditures	CAP_EXT	0.076**	0.041***	0.156
		(0.046)	(0.016)	(0.106)
Payroll per employee	PAYR_EXT	−0.031	−0.099	0.290
		(0.040)	(0.083)	(0.236)
Outcomes due to vanishing establishments				
Employment	EMP_VAN	0.192**	0.205**	−0.134
		(0.087)	(0.101)	(0.106)
Value of shipments	SHIP_VAN	0.148*	0.174*	−0.110
		(0.108)	(0.103)	(0.091)
Capital expenditures	CAP_VAN	0.160*	0.183*	−0.098
		(0.103)	(0.104)	(0.082)
Payroll per employee	PAYR_VAN	−0.102	−0.093	0.115
		(0.070)	(0.145)	(0.248)

* 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, the propensity scores, and the (1982–1987) five-year period dummy are not reported and are available upon request.
[b] Prob>F=0.000 (H_0: All coefficients=0) for all specifications.
[c] N=11,766 (net establishment regressions); =8284 (new); =11,046 (existing); =7622 (vanishing).
[d] N=7852 (net establishment regressions); =5368 (new); =7352 (existing); N=5086 (vanishing).

In general, these results prove to be quite robust, as they withstand the challenge of replicating the analysis with the less restrictive models described in Section 4.2. All of the specification tests described in Section 4.2 favor acceptance of the restrictions imposed by the model of Eq. (3).[7]

[7] These results are omitted for the sake of brevity and are available from the authors.

Table 3
Marginal impact of EZ policy features on new establishment outcomes: results from Eq. (3)

Independent variables[a]		Specification/dependent variable[b]			
		EMP_NEW	SHIP_NEW	CAP_NEW	PAYR_NEW
Zone designation					
Portion of 5 years with active EZ	EZ*T	0.692**	0.756*	0.687*	0.299
		(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)
Incentives tied to job creation	JOB_REQ	−0.308	−0.352	−0.426	−0.042
		(0.192)	(0.241)	(0.236)	(0.161)
Incentives tied to capital expenditures	CAP_REQ	0.025	−0.164	0.053	−0.198
		(0.177)	(0.222)	(0.217)	(0.148)
Portion of state land covered by zones [1=10% increase]	EZ_SIZE	−0.730**	−0.641**	−0.535	−0.493**
		(0.315)	(0.296)	(0.387)	(0.234)
Adjusted R^2		0.3306	0.3051	0.3037	0.2042

* p-value<0.1, ** p-value<0.05, *** p-value<0.01.
Standard errors are in parentheses.
N=8282.
Prob>F=0.000 (H_0: All coefficients=0) for all specifications.
[a] For clarity of exposition, the coefficient estimates on the state fixed effects, propensity score variables and time fixed effects are not reported. These are available from the authors.
[b] F-tests of joint significance have been performed for all of the variables with the coefficient estimates having p-values>0.10. For all four of the specifications, F-test results lead to failure to reject the null hypothesis of non-significance.

5.1. Marginal contribution of EZ policy features

Table 3 summarizes the results for the four specifications of Eq. (3) estimated on the large sample of states that have new establishment gross flow dependent variables and that fully include the EZ policy features interaction terms.[8] Similar to the results in Table 2, zone designation is found to positively affect employment, value of shipments and capital expenditure gross flows accounted for by new establishments. Although zone designation appears to have a positive impact on new establishments, expansion of the program to more areas appears to counteract that effect since increases in the portion of the state designated as EZs significantly reduces growth rates across all four specifications. The coefficient estimates shown in Table 3 highlight the marginal impact on the baseline growth rate of a 10% change in the portion of state land covered by zones. Such estimates imply that, for each one-standard deviation increase in the land coverage of zones, there is a statistically significant marginal decrease in the 5-year baseline growth rates of −20.5, −17.5 and −14.6 percentage points for employment, value of shipments and capital expenditures, respectively.

Designating a larger portion (i.e., one standard deviation increase) 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.

[8] Results from the small-sample analysis are omitted for the sake of brevity. They are all very similar to those from the large sample of states and are available upon request to the authors.

Table 4
Marginal impact of EZ policy features on existing establishment outcomes: results from Eq. (3)

Independent variables[a]		Specification/dependent variable[b]			
		EMP_EXT	SHIP_EXT	CAP_EXT	PAYR_EXT
Zone designation					
Portion of 5 years with active EZ	EZ*T	0.071	−0.083	0.235	−0.076
		(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)
Incentives tied to job creation	JOB_REQ	0.222**	0.017	−0.110	−0.312***
		(0.092)	(0.115)	(0.159)	(0.097)
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 [1=10% increase]	EZ_SIZE	0.057	0.145	−0.111	0.121
		(0.150)	(0.188)	(0.261)	(0.159)
Adjusted R^2		0.3229	0.3293	0.3206	0.2104

* p-value<0.1, **, p-value<0.05, *** p-value<0.01.
Standard errors are in parentheses.
N=11,046.
Prob>F=0.000 (H_0: All coefficients=0) for all specifications.
[a] For clarity of exposition, the coefficient estimates on the state fixed effects, propensity score variables and time fixed effects are not reported. These are available from the authors.
[b] F-tests of joint significance have been performed for all of the variables with the coefficient estimates having p-values>0.10. For all four of the specifications, F-test results lead to failure to reject the null hypothesis of non-significance.

Table 4 illustrates the impact estimates of the EZ status and the state-specific EZ policy features on existing establishment gross flows for the large sample of states.[9] Estimates for the employment specification highlight that providing EZ incentives tied to the number of new jobs created is the only policy variable that marginally affects 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 per employee specification, showing, however, a negative marginal impact of 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 and capital expenditures, with positive marginal impact estimates of 25.8 and 17.5 percentage points, respectively.

Results for the vanishing establishment specifications highlight that, consistent with the results of Table 2, EZ status is found to marginally accelerate the rate-loss of employment, value of shipments and capital expenditures.[10] None of the EZ policy interaction terms added to the model is found to significantly affect the baseline rate loss 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 allow 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 25%,

[9] Again, results from the small sample analysis are omitted for the sake of brevity. These results are very similar to those illustrated in Table 3 and are available from the authors.
[10] Results are not reported for the sake of brevity and are available upon request from the authors.

50% or 75% of their land covered by actual zones. Third, the restrictions posed by the chosen functional form of Eq. (3) are accepted by the specification tests included in the sensitivity analysis.

6. Discussion

The results indicate that positive zone-induced increases in employment, sales, and capital expenditures in new and existing establishments are offset by zone-induced losses among firms that close or leave the zone areas. The estimated null overall mean impact of EZ designation on local economic growth measures across the different gross flows outcomes accounted for by the new, existing and vanishing establishments is consistent with the findings from other recent econometric studies (e.g., Boarnet and Bogart, 1996; Bondonio and Engberg, 2000; Greenbaum and Engberg, 2004; Peters and Fisher, 2002).

There are various possible political and economic explanations for these findings. Announcements of new jobs and new economic activity is more visible than retention of existing jobs and economic activity, and thus political pressure may lead to greater emphasis on attracting new economic activity rather than on retention. If it is the case that incentives are targeted towards new establishments, the existing establishments, which must compete in the same factor markets without subsidy, may be placed at a competitive disadvantage. There may be some benefit to encouraging the economic transformation of these distressed local economies by encouraging new employment in new firms, even at the expense of hastening the loss of established firms and no net additional employment. If this induced accelerated churning of jobs were a positive for the targeted areas, we would expect this benefit to be capitalized into local property markets, which would be an indication that the zone areas are a more desirable place to live and to locate a business. However, Engberg and Greenbaum (1999) found that EZ programs in small cities across 22 states did not improve housing values, thus providing some evidence these changes may not be positive.

Further, the EZ incentives are consistently found to reduce payroll per employee. This indicates that whatever jobs are induced by the incentives are likely low-paying and low-skill. This may be due to requirements in some states that new hires are zone residents, and it may also be an indication that zone areas are not attractive to higher-skilled workers.

The mean impact estimates of EZ designation, however, are not very informative for helping to refine future geographically-targeted economic development policies. The empirical evidence that is most needed for policy makers has to be capable of disentangling which types of program features are best suited for assisting diverse target areas. The marginal impact estimates produced for each relevant policy feature in which state EZ programs offer exogenous variation enables empirically testing of a number of predictions that yield recommended incentive features for refining future geographically-targeted business incentive initiatives. Such predictions that stem from economic theory and/or interviews with business representatives and EZ program officials can be summarized as follows.

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). On the other hand, incentives tied to the number of new jobs created reduces the relative price of labor and may be more effective in promoting local employment growth than programs that tie incentives to capital investments (Wilder and Rubin, 1996). Moreover, many tax incentives appeal more to established businesses than to start-ups because new businesses do not typically earn profits in the first years of operation. Thus, programs that tie tax incentives to the creation of new jobs can benefit zone employment when the targets of zone designation are primarily existing establishments. The results support this prediction by showing that tying incentives to new jobs created is the only EZ feature that marginally increases the baseline employment growth of existing EZ firms. Tying incentives to new jobs is, however, not found to have a significant impact on employment for either new firms or for outcomes other than employment.

The requirement of a strategic local economic development plan in the zone application may be important for effectively promoting zone employment and economic growth. For example, California program officials interviewed by Bostic (1996) 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 to effectively support existing firms. The development of an EZ strategic plan often brings together local business leaders with various administrative and community officials, thus facilitating communication regarding business needs and at the same time making business owners more aware of the opportunities offered by the EZ incentive packages. Indeed, results show that the strategic plan requirement is beneficial for inducing growth in sales and capital investment of existing establishments. However, the strategic plan requirement does not significantly affect the attraction of new economic activity induced by new businesses. This may suggest that other efforts are necessary to inform potential investors about the availability of zone incentives. Alternatively, for new establishments, it may just be that the mere existence of the program is what matters most.

According to program officials, limiting the geographic expansion 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. Limiting the geographic expansion of zones is advocated for four reasons. First, it allows for more intense marketing efforts for each single zone (Wilder and Rubin, 1996). Second, it enables program officials to better evaluate the potential comparative advantage of different eligible areas, allowing designation of areas that have developed the strongest local support for economic growth. Third, it facilitates close program monitoring and evaluation. Finally, keeping the geographic scope more focused prevents the dilution of the zone incentives (Greenbaum and Bondonio, 2004). The results support these predictions by showing that the baseline employment and sales growth among new establishments increases when the geographic extent of the zones decreases. Reducing the geographic expansion 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 quality job opportunities are more successful if the size of the program is limited.

7. Conclusion

This paper examines whether different impacts of EZ programs may be detected by looking at employment, sales (shipments), capital expenditures and payroll per employee growth outcomes. These outcomes are evaluated separately for new, existing, and vanishing establishments. Testing whether specific policy incentives differentially affect the various gross flows of local economic growth is important to help refine future geographically-targeted tax incentive initiatives.

The paper finds that the EZ incentives do have some significant impacts that would be missed by only examining net outcomes. In particular, EZ policies are found to positively affect the gross flows of employment, sales, and capital expenditures accounted for by new establishments. This positive impact, however, is negated by the overall geographic expansion of the designated zones: Zones in states with a smaller relative area designated tend to attract more employment and economic activity from new establishments. Among the new establishments, the EZ incentives are found to reduce payroll per employee, indicating that the new jobs are low-paying. Employment, sales and capital expenditures accounted for by existing establishments tend also to be positively affected by EZ policies, although by a smaller extent. For the existing establishments, tying zone incentives to job creation requirements is found to promote employment growth, and mandating economic development strategic plans as part of the EZ application process promotes growth in sales and capital expenditures. EZ policies, finally, are found to significantly accelerate the loss of employment, sales, and capital expenditures accounted for by vanishing establishments, thus offsetting the gains to the other establishments and resulting in the finding of no net zone impact common in other studies.

As policymakers continue to reexamine their geographically-targeted business incentives, this paper offers some guidance based upon the experience of earlier programs. Within zone areas, greater attention should be paid to existing businesses, as the incentive programs studied in this paper benefited new establishments at the expense of established businesses that subsequently closed or moved. In particular, we find two policy features that have greater positive impacts on existing businesses than on new establishments, the requirement that incentives be tied to job creation and the requirement of a strategic local economic development plan.

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