JOURNAL OF REGIONAL SCIENCE

JOURNAL OF REGIONAL SCIENCE, VOL. 51, NO. 3, 2011, pp. 427–449

DO LOCATION-BASED TAX INCENTIVES ATTRACT NEW BUSINESS ESTABLISHMENTS?*

Andrew Hanson
Department of Economics, Georgia State University, P.O. Box 3992, Atlanta, GA 30302.
E-mail: ahanson@gsu.edu

Shawn Rohlin
Department of Economics, University of Akron, 290 Buchtel Common, Akron, OH 44325-1908.
E-mail: srohlin@uakron.edu

ABSTRACT. This paper examines how offering tax incentives in a local area affects the entry of new business establishments. We use the federal Empowerment Zone (EZ) program as a natural experiment to test this relationship. Using instrumental variables estimation, we find that the EZ wage tax credit is responsible for attracting about 2.2 new establishments per 1,000 existing establishments, or a total of 20 new establishments in EZ areas. New establishment growth is strongest in the retail (about 40 new establishments) and service (about five new establishments) sectors, and offset by declines or slower growth in other industries.

1. INTRODUCTION

Countless state and local governments offer a myriad of tax incentives in an attempt to lure new business establishments into locating in their jurisdiction. These incentives include a range of tax credits for investment in capital, job creation, research and development, and rehabilitation of structures.[1] Often, policy makers create incentives with the hope that they attract new establishments that become a catalyst for future economic growth.

There are two primary challenges that arise in any attempt to determine the effect policy has on the location decisions of new business establishments.[2] First, policy is typically created for a single city or state, making it difficult to find a proper comparison group to construct a counterfactual for what would have happened in the absence of policy. Second, law-makers often craft policy in an attempt to either strengthen the local economy or change historic economic fortunes; therefore, incentives are a function of the current local economic situation, and the policies are endogenous to outcome measures of interest.

These challenges often leave researchers with limited ability to identify the effects of offering tax incentives on new establishment location, as standard methods do not separate trends in the local economy from the policy effects or may give biased results due

*We would like to thank Stuart Rosenthal and Geoffrey Turnbull for comments on the original manuscript. Mark Partridge and three anonymous referees provided helpful suggestions that improved the original manuscript substantially.

Received: February 2009; revised: November 2009, February 2010, March 2010; accepted: April 2010.

[1]See Black and Hoyt (1989) for a formal model of local jurisdictions bidding for establishments.

[2]See Holmes (1998), Bartik (1989), and Bartik (1985) for a discussion and method of identification for how state-wide policies, including taxes, influence the location decision of establishments.

 DOI: 10.1111/j.1467-9787.2010.00704.x

to policy endogeneity. In this paper, we use the Empowerment Zone (EZ) program, a set of incentives offered to establishments that operate in well-defined local areas, but funded by the federal government, as a natural experiment to identify the effect of offering tax incentives on the location decision of new establishments.[3] The EZ program is the largest employer-based wage tax credit in the federal tax code with an estimated cost of $1.7 billion in the President's 2009 budget.[4]

The EZ tax program provides a unique way to identify the effect of offering tax incentives on local areas because designated areas are selected from a group of qualified applicants.[5] We use the group of areas that applied, but did not receive EZ designation to build a counterfactual for what would have happened to areas that did receive the benefits. We also use an instrumental variables (IV) estimation procedure, where we instrument for the tax incentives using the political characteristics of the applicant's federal representatives. Using these methods and the Dun and Bradstreet data on firm location, we determine how offering an employment-based tax credit in a well-defined area of a city affects the location decision of new establishments.

Our IV results show that the EZ wage tax credit is responsible for attracting about 2.2 new establishments per 1,000 existing establishments, or a total of 20 new establishments across all EZ areas. Growth is strongest in the retail (about 40 new establishments) and service (about five new establishments) sectors, but is offset by declines or slower growth in other industries. Our results imply that the cost of attracting one new business to an EZ area is about $19 million.[6] The number of new establishments attracted to EZ areas translates to approximately 130 jobs at new establishments in EZ areas, at cost per job of about $2.9 million.[7]

The remainder of the paper begins with a discussion of the theoretical and empirical reasons local governments attempt to attract new establishments. Section 3 contains an explanation of the incentives offered by the EZ program and a summary of the existing literature. We follow this with a description of our identification strategy and a statistical summary of the comparison and treatment groups, in Section 3. In Section 5, we describe the data for estimating the effect of tax incentives on new establishment location. In Section 6, we discuss our estimation results. The final section concludes.

2. WHY NEW BUSINESS ESTABLISHMENTS?

One of the primary goals of the EZ program is to expand economic opportunities, particularly in the form of jobs in designated areas for zone residents (HUD, 2001).

[3]The EZ program is not targeted to specific firms or industry sectors explicitly, it is targeted to geographic areas within cities. Many incentives offered by localities to attract new businesses are only offered to specific firms or for firms in specific industries.

[4]This is an estimate of tax revenue that the federal government would have collected in the absence of the program and includes the Empowerment Zone program and a smaller, less generous, but similar program called the Renewal Communities tax credit. To put this amount into context, consider that the estimated forgone revenue from the Earned Income Tax Credit (EITC) in the 2009 budget is $5.4 billion, and it is available regardless of geographic location.

[5]Throughout the paper, we refer to the effects of the tax incentives. While we believe our identification strategy separates the tax incentive effects from time trends, area fixed effects, and effects of the application process, ultimately we cannot separate the incentives from the actual designation effects.

[6]This estimate considers the only benefit of the program is attracting new businesses. The estimate is in 2009 dollars using annual cost of EZ program of $300 million from the 2000 tax expenditure budget. This measurement of cost ignores the lump-sum grants afforded to EZ areas at the beginning of the program.

[7]Estimate in 2009 dollars. Neither cost estimate includes new jobs created at existing establishments, or existing establishments that would have closed in the absence of the EZ program.

In principle, job opportunities for residents could arise from existing establishments in the zone area; however, EZ areas contained few existing business establishments. The average number of existing business establishments per census tract in EZ areas before the program was a meager 34.2 (standard deviation of 34.6), compared to 89.6 (standard deviation 219.7) for the rest of the country. Therefore, implicitly, a goal of the program must be to attract new business establishments to designated areas.

There are several theoretical reasons why offering location-based incentives to attract new business establishments may be worthwhile policy, as outlined by Glaeser (2001). Glaeser points out that attracting new business establishments may generate consumer or producer surplus for current residents of the targeted area. The potential gains in surplus from the direct labor market subsidy created by the EZ program may be especially attractive to areas with low employment rates. Second, Glaeser suggests that new establishments may offer agglomeration spill-overs to both existing establishments and to other new entrants. If new establishments offer agglomeration economies, then attracting an initial group of new establishments could have a self-reinforcing effect, further expanding the economic opportunities of zone residents.

In addition, Glaeser suggests that offering location-based tax incentives to new establishments may be justified to compensate them for future tax payments to the area. In the case of the EZ, the subsidy comes from the federal level, so local governments would get the benefit of future tax payments with no cost. Glaeser also points out that tax incentives could be used as a price discrimination mechanism for particular establishments on the margin of choosing a location or they may be the result of political corruption. These last two reasons for tax incentives seem to be unlikely in the case of the federal EZ, as specific new establishments are not the target of the program.[8]

Empirical evidence suggests new establishments can be quite valuable to local economies. Indeed, Greenstone et al. (2010) show that a single new manufacturing establishment (valued at $1 million or more) in a county can have substantial benefits. Paramount among the benefits is the agglomeration economies gained from the new entrant. Greenstone et al. (2010) estimate that a new manufacturing establishment can increase productivity at establishments in the surrounding area by 12 percent. In addition, Greenstone et al. estimate that a single new manufacturing establishment can increase the number of other manufacturing establishments by 12.5 percent, and raise wages of the surrounding area workforce by 2.7 percent.

There is also evidence that state-level Enterprise Zone policies have been effective at creating employment through new business establishments. Bondonio and Greenbaum (2007) find that there are substantial differences in the effect of zone-based tax incentives on employment outcomes at new, existing, and vanishing establishments. The size of the impact they estimate from new establishments is substantial, as much as a 25.2 percent increase in employment growth. Along with the positive findings for new establishments, Bondonio and Greenbaum find offsetting losses at establishments that leave the area or cease to exist.

3. INCENTIVES OFFERED BY THE EZ PROGRAM AND PREVIOUS STUDIES

The federal government began to explicitly offer tax incentives to employers located in parts of economically distressed areas with the creation of the EZ program, which passed

[8]Although EZ designation may not be subject to the same political pressure that a specific new establishment may produce, individual businesses or landowners could attempt to influence local politicians in an attempt to have their location be part of the EZ application. Such political pressure could also be applied when EZ areas are chosen at the federal level.

as part of the 1993 Omnibus Budget Reconciliation Act (OBRA 1993, P.L. 103–66). The EZ program is primarily a set of tax incentives claimed by employers that operate inside of urban areas defined by groupings of census tracts. Both urban and rural areas received EZ incentives, the Department of Housing and Urban Development (HUD) was responsible for designating EZs in urban areas; the Department of Agriculture was responsible for choosing rural EZs.

Each department considered applications for areas where at least 20 percent of the population was living in poverty and 6.3 percent were unemployed (GAO, 2004). From 78 applications, parts of six cities (Atlanta, Baltimore, Chicago, Detroit, Philadelphia/Camden, and New York City) and three rural areas (Kentucky Highlands, Mississippi Delta, and the Rio Grande Valley in Texas) were awarded EZ status. The original designation provided tax-preferred status for 10 years ending in 2005; however, Congress extended the sunset to the end of 2009 with the Community Renewal Tax Relief Act of 2000 (P.L.106–554).

The main component of the tax incentive package is a 20 percent tax credit on wages paid to employees who live and work within the zone boundary. The wage tax credit applies to the first $15,000 in wages paid to employees for a maximum value of $3,000 per employee. There is no requirement that the employee be a new hire or on the type of individual hired as long as they reside in the designated area. The EZ program also provides smaller incentives for capital investment and allows localities to issue bonds on behalf of businesses locating within the zone to finance the purchase of capital.[9]

In addition to tax incentives, EZ areas were awarded $100 million (for urban) or $40 million (for rural) in the form of Social Service Block Grant funds.[10] The tax incentives and grants are exclusively tied to the land that is designated an EZ. EZ incentives require precise location of the establishment in an EZ, but do not require that the establishment is a new entity for tax purposes. This is important, because we are interested in measuring the decision of establishments to physically locate in an area, rather than how they change their tax filing behavior. Because the EZ program does not require that the establishment is a new entity for tax purposes, it should not evoke any response other than physical relocation.

Areas that applied for an EZ, but were denied, almost all received a designation of "Enterprise Community" (EC). EC areas received a Social Service Block Grant allotment of $3 million and may claim some of the capital incentives, but are not allowed to use the wage tax credit.[11] Because the EC areas met the qualifications to be EZs, and a record of their boundaries exists, we use them as a control group to study the effect of the EZ tax incentives on new establishment location.[12]

[9]Establishments that locate within the EZ can expense (rather than deduct) a wider range of capital investment and a larger amount than the federal tax code allows for establishments not in an EZ. Establishments in EZs may also postpone the reporting of capital gains made on zone assets.

[10]Social Service Block Grants cover a variety of services including day care for children, employment services, counseling, legal services, transportation, education, and substance abuse recovery. The Department of Health and Human Services funds the block grants and they are administered by individual states.

[11]Some areas denied EZ status became designated "Supplemental Empowerment Zones" and "Enhanced Enterprise Communities" these areas received a larger allotment of grants. In future years some of the comparison areas were also designated as Empowerment Zones, but were not allowed to claim the wage tax credit until after 2001 (GAO, 2004).

[12]Our sample includes EC areas in the following cities: Akron, OH; Albany, GA; Albany, NY; Albuquerque, NM; Birmingham, AL; Boston, MA; Bridgeport, CT; Buffalo, NY; Burlington, VT; Charleston, SC; Charlotte, NC; Cleveland, OH; Columbus, OH; Dallas, TX; Denver, CO; Des Moines, IA; East St. Louis, IL; El Paso, TX; Flint, MI; Harrisburg, PA; Houston, TX; Huntington, WV; Indianapolis, IN; Ironton, OH;

As shown in Table 1, the EC and EZ areas are, on average, quite similar before zone designation took place. Table 1 reports summary statistics from the 1990 census using tract level data aggregated up to the zone and city level for all EZ areas and for the average of the EC areas. Although, on average the EC areas were in smaller cities, they still include some of the largest cities in the country, and are still on average larger than the smallest EZ city. Importantly, the areas do not differ greatly along economic dimensions as the average unemployment rate, per-capita income, and poverty rate for EC areas are all within the range of EZ areas or within a percentage point or two.

Table 2 takes the pretreatment comparison of the EZ and EC areas one step further by showing that the distribution of several relevant characteristics of the areas has substantial overlap.[13] Column 1 of Table 2 shows the un-weighted average and standard deviation for several relevant characteristics of census tracts in the EZ group, while column 2 shows this for the EC group. As column 3 points out the means are somewhat different, EZ areas are more densely populated, have a higher percentage of non-Whites, have higher unemployment rates, and have less-educated and lower income populations. Although it is true that the average characteristics of these tracts show the EZ areas were worse off, looking at the standard deviations for these characteristics shows the distributions have substantial overlap. For every characteristic in Table 2, the EC mean is within one standard deviation of the EZ mean.

Oakley and Tsao (2006), Busso and Kline (2006), unpublished data, Hanson (2009), and Krupka and Noonan (2009) all examine economic outcomes related to the federal EZ. Oakley and Tsao (2006) find that over-all the EZ had no effect on resident incomes, unemployment, or poverty rates, but that there were some exceptions in certain designated areas. Busso and Kline (2006), unpublished data, suggest that EZ designation is associated with a statistically significant 4.1 percentage point increase in zone-resident employment, and a 3.8 percentage point decrease in zone-resident poverty rates. Hanson (2009) finds that the EZ program has no statistically significant effect on zone-resident employment or zone-resident poverty rates, but is capitalized into local property values. Krupka and Noonan (2009) also find that the EZ program is responsible for a substantial increase in median property value in designated areas.

The majority of previous studies on zone-based incentives are evaluations of state-level programs, and focus on how these programs affect employment outcomes. Papke (1994) examines the State of Indiana Enterprise Zone program and finds that unemployment claims at offices within the zone declined by 19 percent, a decline of 1,500 claims per year at the mean. Boarnet and Bogart (1996) examine the effect of the New Jersey Enterprise Zone program and find that Enterprise Zone status has no effect on employment or property values at the municipal level. O'Keefe (2004) finds that the Enterprise Zone program in California increased employment growth by 3.1 percent relative to comparison areas in the first 6 years followed by a decrease in employment growth of 3.2 percent in years 7–13.

Jackson, MS; Kansas City, KS; Kansas City, MO; Las Vegas, NV; Little Rock, AR; Los Angeles, CA; Louisville, KY; Lowell, MA; Manchester, NY; Memphis, TN; Miami, FL; Milwaukee, WI; Minneapolis, MN; Muskegon, MI; Nashville, TN; New Haven, CT; Newark, NJ; Newburgh, NY; Norfolk, VA; Oakland, CA; Ogden, UT; Oklahoma City, OK; Omaha, NE; Phoenix, AZ; Pittsburgh, PA; Portland, OR; Providence, RI; Rochester, NY; San Antonio, TX; San Diego, CA; San Francisco, CA; Seattle, WA; Springfield, IL; Springfield, MA; St. Louis, MO; St. Paul, MN; Tampa, FL; Waco, TX; Washington, DC; and Wilmington, DE.

[13]The averages in Tables 1 and 2 do not match because the characteristics in Table 1 are weighted by population, whereas the characteristics in Table 2 are simple averages and standard deviations of all census tracts in each group.

TABLE 1: Pretreatment Characteristics of Sample: Zone and Surrounding City

	Atlanta, GA		Baltimore, MD		Chicago, IL		Detroit, MI		New York, NY		Philadelphia, PA*		EC Area Average	
	1990 EZ	1990 City	1990 EZ	1990 City	1990 EZ	1990 City	1990 EZ	1990 City	1990 EZ	1990 City	1990 EZ	1990 City	1990 Zone	1990 City
Land area (sq.mi.)	10	132	6	81	16	227	25	139	7	303	3	135	11	132
Population	54,514	394,017	77,173	736,014	224,737	2,783,726	112,531	1,027,974	221,178	7,322,564	44,541	1,585,577	61,835	400,592
Population density	5,561	2,985	12,829	9,087	13,972	12,263	4,547	7,395	31,927	24,167	16,167	11,745	7,200	4,537
% White	5%	31%	21%	39%	12%	45%	26%	22%	15%	52%	12%	54%	36%	65%
% Black	92%	67%	77%	59%	71%	39%	66%	76%	57%	29%	61%	40%	51%	27%
% Other	2%	2%	1%	2%	17%	15%	8%	3%	27%	19%	27%	7%	13%	8%
Unemployment Rate	18%	9%	15%	9%	24%	11%	28%	20%	17%	9%	24%	10%	16%	9%
Employment rate	28%	44%	32%	43%	25%	43%	24%	33%	31%	44%	24%	41%	34%	45%
Labor force participation	34%	49%	37%	47%	33%	49%	33%	41%	37%	49%	31%	46%	40%	49%
Income per capita (1999 dollars)	$7,057	$20,474	$10,426	$16,072	$7,527	$17,285	$9,333	$12,654	$9,938	$21,817	$7,446	$16,202	$9,819	$17,339
Income below poverty line	56%	27%	42%	22%	49%	22%	47%	32%	43%	19%	53%	20%	40%	20%
Vacant housing units	21%	15%	18%	9%	20%	10%	18%	9%	8%	6%	21%	11%	14%	9%
% Graduating from High school	41%	70%	43%	61%	42%	66%	46%	62%	45%	68%	39%	64%	52%	72%
% Graduating from college	5%	27%	8%	15%	7%	19%	8%	10%	9%	23%	5%	15%	10%	21%

Notes: Data are from 1990 census tract level and are aggregated to either the city or zone.

*The Philadelphia EZ also includes parts of Camden, NJ that are excluded from this analysis.

TABLE 2: Comparison of Pretreatment Characteristics of EZ and EC Areas

	(1) EZ Areas	(2) EC Areas	(3) (1)–(2)	(4) Within 1 SD
Population density	6,992	3,331	3,661	Yes
	(10,089)	(14,711)		
% Non-White	0.8408	0.6855	0.1553	Yes
	(0.2188)	(0.2835)		
Unemployment rate	0.2359	0.1705	0.0653	Yes
	(0.1332)	(0.0876)		
Employment rate	0.2637	0.3230	−0.0594	Yes
	(0.1083)	(0.1015)		
Income per capita (1999 dollars)	8,513	9,429	−916	Yes
	(3,501)	(3,699)		
% Income below poverty line	0.4807	0.4200	0.0607	Yes
	(0.1607)	(0.1313)		
% Vacant housing units	0.1650	0.1446	0.0204	Yes
	(0.1159)	(0.0908)		
% Graduating from High school	0.4281	0.4859	−0.0579	Yes
	(0.1295)	(0.1276)		
% Graduating from College	0.0739	0.0888	−0.0150	Yes
	(0.0631)	(0.0615)		

Note: Average characteristics of EZ and EC areas presented above are the census tract average of all tracts that make up each area and are not weighted by population. In contrast, the averages weighted by population are shown in Table 1. Standard Deviations are shown in parentheses.

Bondonio and Engberg (2000) analyze several different state zone-based programs and find that they have no impact on employment. The null result is quite robust to changes in methodology and is not sensitive to the specific incentives of state programs (or their value). Bondonio and Greenbaum (2007) analyze a larger set of state zone-based programs and look for differential impacts by establishment tenure. They find that geographically targeted incentives have a positive effect on employment at new and existing establishments, but these gains are offset by the loss of employment from establishments that close or leave the area.

There is also a literature that more generally addresses business establishment location decisions. Kolko and Neumark (2008) use the National Establishment Time-Series database to show that business establishments are generally leaving the state of California for other U.S. states, but that employment of residents is unaffected by this trend. Using establishment level data from Maine, Gabe and Bell (2004) show that business establishments favor localities with high levels of public spending, even though these areas also have higher tax burdens. For a recent review of this literature, organized by empirical methodology, see Arauzo-Carod et al. (2010).

4. METHOD OF IDENTIFICATION

To identify the effect of the EZ tax incentives on new establishment location, we use a differencing methodology to build a counterfactual for what would have happened in the absence of the program.[14] Our strategy is to compare relative outcomes between EZ

[14]Other researchers have designed similar methods to identify the effect of zone based tax incentives on various outcomes, including Busso and Kline (2006), unpublished data, Papke (1994), Boarnet and Bogart (1996), Bondonio (2003), Bondonio and Engberg (2000) and Greenbaum and Engberg (2004). The

areas and their surrounding city with EC areas and their surrounding city to see how this difference changes before and after the program. This design isolates the effect of the EZ from city fixed effects because it makes an across-time comparison. It also isolates the effect of the EZ from time-variant, citywide effects because it makes an intra-city comparison. Our primary assumption is that the difference between these areas and their surrounding cities would have grown the same in the absence of the tax incentives.

The EC comparison group is similar to the EZ areas, but because they are located in different cities, the areas are not likely to be subject to negative spill-overs from the policy. This may be the case if we were to choose a comparison group based on an inter-city matching technique if establishments make a location choice based on a set of areas that are similar within a city. Also, because both the comparison and treatment groups applied for EZ designation and met the criteria for unemployment and poverty, there will be no unobservable effects from going through the application process or being qualified.

To avoid scaling issues between the number of new establishments in a census tract and the number of new establishments in the larger city we weight the difference between tract and city by the number of existing establishments. The dependent variable used in our regressions is of the following form, to reflect the differencing methodology and weighting

(1)
$$Y_{tract} = \left[\frac{new\ estab_{tract,post} - new\ estab_{tract,pre}}{existing\ estab_{tract,pre}}\right] - \left[\frac{new\ estab_{city,post} - new\ estab_{city,pre}}{existing\ estab_{city,pre}}\right].$$

Equation (1) states that the dependent variable is the change in new business establishments in the census tract between the pretreatment year and posttreatment year weighted by the number of existing business establishments in the tract *minus* the change in new business establishments in the city between the pretreatment year and the posttreatment year weighted by the number of existing business establishments in the city.

Taking the difference between tract and city eliminates any difference in new establishment location that happens because of city-specific shocks between our years of data. For example, if EZ cities implement a city-wide policy that attempts to induce relocation of establishments from other cities, taking this difference will separate the effect of this policy on new establishment location decisions from the effect of offering EZ, as long as the policy did not affect EZ areas differentially from the larger city.

A potential weakness of the differencing method is that the surrounding city may be subject to general equilibrium effects of the EZ incentives. Although this problem may be more serious when using comparison areas that are similar and geographically close, this may still be a concern if economic activity shifts across these areas. It could also affect our results if there are externalities (positive or negative) on comparison areas, making the effect of the program look larger or smaller than it actually is.[15] By differencing with the entire city surrounding the EZ, the potential for general equilibrium effects are muted. The estimating equation used to determine the effect of the tax incentives on new establishment location, as measured by the number of new establishments, is

(2)
$$Y_{i,n} = \alpha + \beta \mathrm{EZ}_{i,n} + X'_{i,n}\delta + u,$$

primary differences between the identification strategy presented here and these papers are the manner in which we build a counterfactual and our treatment of zone designation as an endogenous variable.

[15]If EZs improved other areas of the city because of a positive externality, then comparing the EZ area to the surrounding city would understate the true effect. If the EZs shifted resources away from other areas of the city, then comparing the EZ area to the surrounding city would overstate the true effect.

where i indexes census tracts, n indexes the industry at the two-digit Standard Industrial Code (SIC) level, X is a vector of control variables including industry dummy variables, and EZ is a dummy variable for availability of the EZ wage tax credit. The outcome variable, Y, is expressed in counts of establishments by industry in a given census tract, and is differenced and weighted by the number of existing establishments as shown in equation (1). The differencing method limits the error term to being only census tract level variables that change over the decade. If there are variables that are census tract specific that change over the decade and are correlated with designation of the EZ, they can still cause a biased estimate of the EZ program effects.

5. DATA

Our data source for the number of new establishments that enter a local area is the Dun and Bradstreet (D&B) Marketplace database.[16] The data come from the fourth quarter survey from the years 1994, 1996, and 2000. These data contain a wealth of establishment information, including employment, sales, years of service, the location at the ZIP code level, and the two-digit SIC code of the establishments.

We map the ZIP code level data to census tracts using a ZIP code to census tract correspondence to match the EZ and EC geography.[17] This correspondence determines what percent of each ZIP code lies in a given census tract and assigns that percentage of ZIP code employment or establishments to the census tract.[18] Specifically, we allocate the raw number of new establishments and employees from a given ZIP code to its overlapping census tract based on the amount of land area that overlaps between the two. For example, if a ZIP code has 100 new establishments and 20 percent of its land area is contained in a census tract in our data we allocate 20 new establishments to that census tract.

We examine 1,331 census tracts made up of 1,033 ZIP codes.[19] In our sample, 263 of the 1,033 ZIP codes reside completely in a census tract; however, census tracts in our data often contain several pieces of ZIP codes, on average only 14 percent of each ZIP code applies to a given census tract (standard deviation of 27 percent). The level of observation in our data is a census tract, so each tract contains an allocation from several ZIP codes to create uniform units of observation.

We create our dependent variable, a measure of industry agglomeration, and a count of existing business within the same broader industry (SIC one digit) from the D&B data. We classify an establishment as new if it has been in service for one year or less at the time of the survey.[20] Our measure of agglomeration is the number of establishments in the same SIC that have been in service for more than four years. For each census tract in

[16] Although the D&B does not contain all business activity in the United States, the omissions from the data is sufficiently random so the data is considered representative of the spatial distribution of the business activity in the United States.

[17] We obtained a list of EZ and EC census tracts through personal correspondence with the Department of Housing and Urban Development; this list is also partially available through the department's webpage.

[18] To use this correspondence, we assume that the spatial distribution of business activity in a ZIP code is sufficiently random so that when we allocate to the tract level the EZ receives roughly the correct proportion of activity from the ZIP code. This is a standard assumption in the literature, for example see Holmes (1998) and Rosenthal and Strange (2003).

[19] The average area of a tract used in this study is around a half of a mile compared to the average area of a ZIP code that has some of its area within the census tract is roughly three miles. Therefore, the average ratio of the area of a tract and the area of the ZIP code used to impute the values for the tract is 0.5/3 or 0.18.

[20] This includes establishments that are new movers into EZ areas.

our data we have several observations as we use the SIC two-digit industry as our unit of analysis. Census tracts typically have between 60 and 80 different two-digit industries.

6. RESULTS

New Establishments Across Industry Types

Table 3 shows estimation results for equation (2). Table 3 presents both short-term results (the 1994–1996 difference) and long-term results (1994–2000 difference) of estimating equation (2) using OLS.[21] All regressions include SIC two-digit industry fixed effects. We estimate equation (2) using only the industry controls as well as using a set of controls that includes the total number of establishments in 1994, the share of establishments classified as manufacturing in 1994, and the share of establishments classified as retail in 1994. We cluster standard errors in all regressions at the city level, because we expect that the number of new establishments in a census tract is correlated within a city and therefore the error term is likely to be correlated within a city.

The short-term (top panel) regression results in column 1 of Table 3 show that the location based tax incentives offered by the EZ program had a positive effect on the number of new establishments that choose to locate in designated areas. The coefficient on the tax incentive (EZ) variable shows that areas designated with tax incentive status had about 0.17 more new establishments per 1,000 existing establishments, which translates into about 1.5 total new establishments. The standard errors on the EZ coefficient are larger than the point estimate, making it difficult to infer what the true effect of the incentives across all industries is.

One reason for the large standard errors on the EZ coefficient for all industries could be that the tax incentives have a heterogeneous effect on new establishments across industry types.[22] The results in columns 3 through 12 of the top panel in Table 3 show that the sign on the point estimates differ across industries. Columns 3 and 4 in the top panel of Table 3 show that the EZ tax incentives have a negative effect on the number of new manufacturing establishments in the targeted areas. The coefficients in columns 3 and 4 suggest that EZ areas had a decline in new manufacturing establishments that was between 22 and 26 percent larger than the decline in the comparison areas (comparing the EZ coefficient to the constant term).

A potential problem with measuring the effect of the tax incentives using a short time window is that establishments may not have knowledge of the incentives, and thus my not react accordingly. To test whether the long-term effect of the tax incentives is different, we use the same econometric specification in equation (2), but with year 2000 data as the treatment period.[23] The bottom of Table 3 shows results using year 2000 as the treatment period. The results for all industries estimated without controls suggest that the long-term effect of the tax incentives is also positive, with a magnitude similar to the short-term effect, but not statistically different than zero. The heterogeneous effects across industry type in the short-term estimates also hold for the long-term estimates.

[21]The short-term differences include only data from 1994 and 1996 and the long-term differences include only data from 1994 to 2000, no intervening years are included.

[22]Hanson and Rohlin (2010) show, in a simple theory model as well as empirically, that a wage tax credit has a differential effect across industry sectors.

[23]These regressions measure the number of new establishments in 2000, not the cumulative number of establishments between 1994 and 2000.

TABLE 3: Effect of EZ Tax Incentives on New Establishment Location, Standard Errors Clustered at the City Level Shown in Parentheses

	All		Manufacturing		Wholesale		Retail		FIRE		Service	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Short-term (1994–1996)												
EZ	0.173	0.0659	−0.059**	−0.09***	5	2.85	2.07*	1.56	−0.0896	−0.152	0.427	0.341
	(0.235)	(0.173)	(0.0276)	(0.0272)	(3.57)	(2.49)	(1.16)	(0.945)	(0.153)	(0.156)	(0.352)	(0.314)
% Retail (1994)		2.37		0.957**		53.6*		11.3		1.62		1.27
		(1.85)		(0.473)		(29.7)		(7.89)		(1.94)		(1.81)
% Manufacturing		0.358		−0.0205		9.22		2.85		0.334		0.142
(1994)		(0.670)		(0.263)		(11.5)		(3.65)		(1.59)		(0.820)
Establishments		−0.0005***		<0.0001		−0.0055*		−0.002**		>−0.0001		−0.0008***
(1994)		(0.0002)		(0.0001)		(0.0028)		(0.0008)		(0.0002)		(0.0003)
Constant	−0.0983*	−0.264*	−0.277***	−0.346***	0.272	−4.05*	0.716**	−0.225	−0.218	−0.361	−0.149	−0.185
	(0.0526)	(0.155)	(0.0966)	(0.0995)	(0.581)	(2.35)	(0.336)	(0.415)	(0.161)	(0.269)	(0.0971)	(0.215)
Observations	106,480	106,480	26,620	26,620	2,662	2,662	10,648	10,648	9,317	9,317	18,634	18,634
R^2	0.024	0.025	0.006	0.008	0.017	0.038	0.040	0.050	0.020	0.021	0.017	0.022
Long-term (1994–2000)												
EZ	0.169	−0.0097	−0.0319	−0.0598**	0.0351	−0.834	2.09	1.41	−0.127	−0.177	0.316	0.226
	(0.222)	(0.102)	(0.0272)	(0.0284)	(0.690)	(0.526)	(1.65)	(1.05)	(0.138)	(0.134)	(0.201)	(0.145)
% Retail (1994)		5.02*		0.682**		21.8**		18.1		1.31		1.92
		(2.96)		(0.280)		(9.21)		(15.7)		(1.14)		(1.66)
% Manufacturing		−0.0786		0.284**		4.83		−0.498		0.593		−0.126
(1994)		(0.364)		(0.135)		(6.07)		(2.05)		(0.952)		(0.551)
Establishments		−0.0001		>−0.0001		−0.002**		−0.0012		>−0.0001		−0.0005**
(1994)		(0.0002)		(0.0000)		(−0.0008)		(0.0008)		(0.0001)		(0.0002)
Constant	−0.0428	−0.378	−0.136**	−0.209***	0.997**	−0.897	0.305	−0.814	−0.406***	−0.559***	−0.249***	−0.330**
	(0.0652)	(0.269)	(0.067)	(0.0761)	(0.439)	(1.26)	(0.279)	(0.813)	(0.111)	(0.195)	(0.0631)	(0.159)
Observations	106,480	106,480	26,620	26,620	2,662	2,662	10,648	10,648	9,317	9,317	18,634	18,634
R^2	0.017	0.020	0.004	0.005	0.004	0.020	0.020	0.034	0.017	0.017	0.008	0.011

Notes: (a) The pretreatment year is always 1994, the treatment year for the short-term results is 1996, and the treatment year for the long-term results is 2000.

(b) Unit of observation is the two-digit SIC industry at the census tract level of geography.

(c) Data on number of establishments is from the Dun & Bradstreet survey and is differenced as shown in equation (1) to reflect our identification strategy.

(d) The EZ variable represents designation of the federal EZ wage tax credit at the census tract level, the coefficient on this variable reflects the effect of this tax incentive on the number of new establishments per 1,000 existing establishments that locate in an area relative to the surrounding city compared to areas that applied for the EZ designation but were denied relative to their respective surrounding city.

***indicates statistically significant at 1% level, **at 5% level, *at 10% level.

New Establishments Across Establishment Size

There has been recent interest by state and local policy makers to attract or encourage the birth and growth of *small* establishments known as "economic gardening."[24] Since location-based tax incentives are one tool for meeting this goal, we are interested in testing if the EZ tax incentives have a differential effect on new establishments across different sizes, where we measure size by the number of employees. The Dun and Bradstreet data allow for this flexibility, as they include information on the number of establishments sorted by number of employees.

To test the effect of the EZ tax incentives on new establishment location across different size establishments, we break the dependent variable down by counts of establishments based on the number of employees. We run separate regressions using equation (2) for establishments that are between one and four employees, five to nine employees, 10–49 employees, 50–99 employees, and 100 or more employees. Table 4 shows the regression results for equation (2) using establishments of different size as the dependent variable.

The results for estimating the effect of the EZ program by establishment size are broken down into the short-term (top panel of Table 4) and long-term effect (bottom panel of Table 4). Columns 1 and 2 in the top panel of Table 4 show that the EZ tax incentives reduced the number of new establishments that have between one and four employees by between 0.08 and 0.09 per 1,000 existing establishments. Considering that the constant is positive (between 0.15 and 0.25), this is a large decline, and both estimates with and without controls are statistically significant at the 5 percent level. Across the establishment-size distribution (moving from column 1 through 10 of Table 4), the negative effect of the EZ program becomes small and then vanishes[25] for establishments with more than 100 employees.[26]

Employment at Existing Establishments

At first glance, our results may appear counter-intuitive; we show that a policy designed to strengthen local economies causes a decline in the number of new small establishments and has a statistically unimportant effect on all new establishments. Although our IV estimates suggest the effect of the program is positive, the null OLS finding seems to fit with the existing evidence in Krupka and Noonan (2009) and Hanson (2009) that the EZ tax incentives are capitalized into local property values.

If property values reflect immediate capitalization of the tax incentives by the marginal land purchase, new establishments considering locating in the targeted area may not be able to afford the increased rents. Existing establishments may be insulated from an immediate increase in rents if they have a pre-existing lease, so they may be able

[24]Littleton, Colorado pioneered the economic gardening approach to growth in the late 1980s. A complete description of the approach is available on the cities' webpage at: www.littletongov.org/bia/economicgardening/.

[25]One reason the estimates on the EZ coefficient differ by new establishment size is because establishments at the upper end of the distribution are increasingly rare. The mean change in the number of new establishments of 100 or more employees (our dependent variable) is 0.0003 per 1,000 existing establishments.

[26]The results presented in Table 4 are not heterogeneous across industries. We have run regressions at the one digit industry level (as in Table 3) by establishment size and consistently find a negative effect of the tax incentives on new establishment location. This effect is largest for establishments of between one and four employees regardless of the industry, and remains so across the spectrum of establishment sizes as shown in Table 4. These results are available from the authors upon request.

TABLE 4: Effect of Tax Incentives on New Establishment Location by Establishment Size, Standard Errors Clustered at the City Level Shown in Parentheses

	1–4		5–9		10–49		50–99		100 or More	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Short-term (1994–1996)										
EZ	−0.0823**	−0.0934**	−0.017***	−0.0207***	−0.0084	−0.0115*	−0.0037**	−0.0038*	−0.002	−0.0029
	(0.0368)	(0.0361)	(0.0059)	(0.0062)	(0.006)	(0.0059)	(0.0014)	(0.0022)	(0.0013)	(0.002)
% Retail (1994)		0.797***		0.191***		0.145***		0.0468		0.067**
		(0.27)		(0.0682)		(0.0504)		(0.0306)		(0.0283)
% Manufacturing		0.164		0.0592		0.107***		−0.00639		0.0137
(1994)		(0.232)		(0.0364)		(0.0363)		(0.0137)		(0.0139)
Establishments		0.00033*		<0.0001**		<0.0001		<0.0001		>0.0001*
(1994)		(0.0002)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
Constant	0.252**	0.15	−0.0218	−0.0468**	0.0068	−0.0189	−0.0044	−0.0093	−0.000597	−0.0091***
	(0.121)	(0.126)	(0.0218)	(0.0221)	(0.0116)	(0.0123)	(0.0058)	(0.0064)	(0.001)	(0.0032)
Observations	105,149	105,149	105,149	105,149	105,149	105,149	105,149	105,149	105,149	105,149
R^2	0.064	0.073	0.004	0.007	0.004	0.006	0.001	0.002	0.001	0.003
Long-term (1994–2000)										
EZ	−0.012	−0.0181	−0.0081	−0.0083	−0.0072	−0.0101*	−0.0028*	−0.0055**	−0.0028	−0.0017
	(0.0163)	(0.0166)	(0.0082)	(0.0085)	(0.0051)	(0.006)	(0.0015)	(0.0021)	(0.0022)	(0.0023)
% Retail (1994)		0.418***		0.0734		0.180***		0.0988***		0.0063
		(0.132)		(0.046)		(0.0608)		(0.0309)		(0.0232)
% Manufacturing		0.0215		0.0175		0.0149		0.0317**		0.0134
(1994)		(0.0982)		(0.0262)		(0.032)		(0.0125)		(0.0125)
Establishments		0.0002**		<0.0001***		<0.0001**		<0.0001		<0.0001***
(1994)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
Constant	0.0455	<0.0001	−0.0144	−0.0253	−0.0091	−0.0288***	−0.0024	−0.0142*	−0.0003	−0.0045
	(0.0346)	(0.0384)	(0.0152)	(0.0167)	(0.0075)	(0.0103)	(0.0067)	(0.0074)	(0.0010)	(0.0032)
Observations	105,149	105,149	105,149	105,149	105,149	105,149	105,149	105,149	105,149	105,149
R^2	0.040	0.047	0.003	0.004	0.003	0.007	0.001	0.002	0.001	0.002

Notes: (a) The pretreatment year is always 1994, the treatment year for the short-term results is 1996, and the treatment year for the long-term results is 2000.

(b) Unit of observation is the two-digit SIC industry at the census tract level of geography.

(c) Data on number of establishments is from the Dun & Bradstreet survey and is differenced as shown in equation (1) to reflect our identification strategy.

(d) The EZ variable represents designation of the federal EZ wage tax credit at the census tract level, the coefficient on this variable reflects the effect of this tax incentive on the number of new establishments per 1,000 existing establishments that locate in an area relative to the surrounding city compared to areas that applied for the EZ designation but were denied relative to their respective surrounding city.

***indicates statistically significant at 1% level, **at 5% level, *at 10% level.

to take advantage of the credit by hiring more workers without the cost of rising rents in the short term. Rent increases may disproportionally discourage new establishments if they are uncertain about how much benefit they would receive from the program due to uncertainty about profitability (and therefore tax liability).

Knowing property values increase as a result of the tax credits, we would expect there to be some effect on existing establishments in the targeted area. The D&B data provide counts of establishments and employees by age of the establishment, allowing us to test the effect on establishments that were in the area prior to the start of the tax incentives. We use the same econometric specification presented in equation (2), but change our dependent variable to be the number of employees at existing establishments.[27] This allows us to see if existing establishments react to the tax incentives by expanding employment.[28]

Our results, shown in Table 5, provide evidence that existing establishments in certain industries do expand employment as a result of the tax incentives. Regression results at the industry level show that the retail and service sectors expand employment. The effect in the retail sector (columns 7 and 8) is positive, and suggests existing establishments expand employment by between 14.5 and 19.2 employees per 1,000 existing establishments in the short term and between 11.5 and 14.7 employees in the long term (statistically significant in all specifications). In addition, the effect in the service sector (columns 11 and 12) shows that existing establishments expanded employment by between 8.1 and 9.2 employees per 1,000 existing establishments in the short term and by between 6.5 and 7.2 employees per 1,000 existing establishments in the long term.

Instrumental Variables

Ultimately EZ areas are selected over the EC areas for a reason.[29] If EZ areas are selected based on expectations of future economic fortune (either negative or positive) then our OLS results are biased. For example, if EZ areas are selected over other applicants because they are less likely to be able to attract new establishments than other applicants, then our OLS results would be biased toward finding a negative effect of the tax incentives.[30] In addition, the D&B data contain a limited set of control variables, which invites the possibility of omitted variable bias.

To test the possibility that our primary findings are driven by selection of EZ areas or omitted variables, we use an instrumental variables estimation procedure and re-estimate equation (2). A plausible instrument for EZ designation is one that reflects the political influence of the Representative associated with the census tract. Our measures of political influence are whether the area had a representative on the House of Representatives Ways and Means Committee and the number of years the representative was in office at the time of EZ designation. There is existing evidence on the relationship between political favoritism and EZ designation. Both Wallace (2004) and Hanson (2009) find that a location

[27]Existing establishments are establishments that have been in service in the location for four years or more.

[28]This regression uses employment at establishments within the EZ boundary as the dependant variable; this is not necessarily the same as employment of residents. Hanson (2009) finds employment changes of residents to be zero, although research using alternative specifications by Busso and Kline (2006), unpublished data, finds positive effects.

[29]See Hanson (2009) for a discussion of EZ selection process and a discussion of the potential bias in estimation caused by this process.

[30]Bias in the OLS estimation could also occur if establishments see the designation of EZ status as a signal that they should avoid the area. If establishments gain negative knowledge about the targeted area through designation, then the OLS findings are likely bias toward finding no effect or a negative effect of the program.

TABLE 5: Effect of Tax Incentives on Employment at Existing Establishments, Standard Errors Clustered at the City Level Shown in Parentheses

	All		Manufacturing		Wholesale		Retail		FIRE		Service	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Short-term (1994–1996)												
EZ	1.52	−0.061	−0.189	−0.14	−7.27	−6.39	19.2***	14.5***	−2.86	−2.66	9.22***	8.10**
	(1.74)	(1.63)	(0.194)	(0.179)	(5.27)	(5.01)	(4.63)	(5.10)	(3.14)	(2.95)	(3.19)	(3.50)
% Retail (1994)		57.5***		−0.34		−38.7		109**		−2.70		16.7
		(10.8)		(2.37)		(31.3)		(44.8)		(20.6)		(21.3)
% Manufacturing		11		−3.27**		−9.58		43.9		−27**		3.86
(1994)		(7.90)		(1.36)		(35.9)		(33)		(12.8)		(16.2)
Establishments		0.0085*		0.0004		−0.0095*		−0.0141***		−0.0003		−0.0102***
(1994)		(0.0046)		(0.0004)		(0.0048)		(0.005)		(0.0033)		(0.0031)
Constant	6.96**	0.977	−0.279	0.0619	2.32	6.9	8.32**	−2.91	0.959	4.09	−2.36**	−3.09
	(3.08)	(3.50)	(0.360)	(0.556)	(2.41)	(6.36)	(3.18)	(3.33)	(0.937)	(2.58)	(1.06)	(2.48)
Observations	106,480	106,480	26,620	26,620	2,662	2,662	10,648	10,648	9,317	9,317	18,634	18,634
R^2	0.040	0.048	0.012	0.014	0.010	0.015	0.088	0.102	0.093	0.097	0.084	0.087
Long-term (1994–2000)												
EZ	2.46	0.872	0.544***	0.493**	−2.99	−2.79	14.7***	11.5**	6	5.72	7.27**	6.59**
	(1.94)	(1.81)	(0.169)	(0.194)	(3.91)	(3.81)	(4.64)	(4.92)	(5.91)	(5.58)	(2.91)	(3.21)
% Retail (1994)		65.9***		2.62		−18.6		72.8**		10.9		5.44
		(11.9)		(1.76)		(41.1)		(29.5)		(18.7)		(16.9)
% Manufacturing		19.8**		−2.99***		49.5		40.3		−21.6		5.39
(1994)		(8.23)		(1.10)		(32)		(28.5)		(19.9)		(12.4)
Establishments		0.0148**		0.0005		−0.0043		−0.0109***		>−0.0001		−0.0092***
(1994)		(0.0064)		(0.0004)		(0.0038)		(0.0036)		(0.0031)		(0.0030)
Constant	6.97*	−1.09	−1.05**	−0.952*	2.22	−1.45	3.71	−4.87	−2.82*	−1.26	−2.41***	−2.6
	(3.94)	(4.21)	(0.445)	(0.494)	(2.89)	(7.06)	(2.77)	(3.31)	(1.63)	(2.52)	(0.890)	(2.03)
Observations	106,480	106,480	26,620	26,620	2,662	2,662	10,648	10,648	9,317	9,317	18,634	18,634
R^2	0.026	0.037	0.009	0.012	0.001	0.010	0.062	0.072	0.104	0.106	0.085	0.089

Notes: (a) The pretreatment year is always 1994, the treatment year for the short-term results is 1996, and the treatment year for the long-term results is 2000.

(b) Unit of observation is the two-digit SIC industry at the census tract level of geography.

(c) Data on number of establishments is from the Dunn & Bradstreet survey and is differenced as shown in equation (1) to reflect our identification strategy.

(d) The EZ variable represents designation of the federal EZ wage tax credit at the census tract level, the coefficient on this variable reflects the effect of this tax incentive on the number of new establishments per 1,000 existing establishments that locate in an area relative to the surrounding city compared to areas that applied for the EZ designation but were denied relative to their respective surrounding city.

***indicates statistically significant at 1% level, **at 5% level, *at 10% level.

TABLE 6: 1st stage IV: Dependent variable is EZ Designation, Standard Errors Shown in Parentheses

	(1)	(2)	(3)	(4)
Ways and means member	0.1416***		–0.2589***	–0.2589***
	(0.0033)		(0.0061)	(0.0061)
Number of terms on Committee		0.0224***	0.0445***	0.0445***
		(0.0004)	(0.0006)	(0.0006)
Constant	0.1826	0.1727	0.1826	0.1826
	(0.0013)	(0.0013)	(0.0013)	(0.0109)
Industry fixed effects	No	No	No	Yes
N	105,149	105,149	105,149	105,149
R^2	0.0169	0.0443	0.0573	0.0573
Instrument F-test (1,105147)	1,804.36	4,870.23		
P-value	0	0		
Instrument F-test (2,105146)/(2,105068)			3,115.81	3,113.5
P-value			0	0

Notes: (a) Information about congressional committee assignment and years of service comes from http://clerk.house.gov/. We match this to census tract geography using the Mable/Geocorr database online at http://mcdc2.missouri.edu/websas/geocorr90.shtml.

(b) Unit of observation is the two-digit SIC industry at the census tract level of geography.

(c) We also run the first stage by clustering standard errors at the SIC two-digit level; this decreases our standard errors on the instrument parameters and does not change the fact that they pass the instrument F-test.

***indicates statistically significant at 1% level, **at 5% level, *at 10% level.

represented by a member serving on the House Ways and Means committee is correlated with being designated an EZ.

Table 6 shows the first stage results using both Ways and Means committee membership and the number of years that the congressman was on that committee. As shown by the instrument F-test, the instruments show a strong correlation with EZ designation. They are both individually significant as shown by the P-values, as well as jointly significant as shown by the instrument F-test and corresponding P-value.[31] These results are essentially the same whether we use the two-digit SIC effects in the model or not.

The second stage regression results from the IV estimation using both instruments, shown in Table 7, suggest a much different effect of the EZ program on new establishment location than the OLS findings. The IV results show a large, positive, and statistically significant relationship between the EZ tax incentives and new establishment location estimating all sectors jointly. This positive result is driven by the particularly strong finding for the retail and service sectors.[32] Columns 1 and 2 of the top panel in Table 7 show that the EZ program was responsible for attracting between 2.1 and 2.4 new establishments per 1,000 existing establishments in designated areas in the short term (1994–1996). These results are statistically significant at the 1 percent level.

The large, statistically significant results for new establishments in all industries continue into the longer term as shown by the estimates in columns 1 and 2 in the bottom

[31]The partial F-statistic from first stage results using both instruments suggests that the instruments are jointly significant. We also test the over-identification restriction using the Sargon–Hansen J-statistic (through the ivreg2 command in Stata). This test rejects the null hypothesis that instruments are valid, with a P-value of 0.0207.

[32]The positive relationship between the EZ tax incentives and new establishment location is not sensitive to using only one instrument. The magnitude of this relationship remains about the same using only the number of years on committee variable, but is somewhat smaller when using only the membership instrument.

TABLE 7: 2nd stage IV for Effect of EZ Tax Incentives on New Establishment Location, Standard Errors Clustered at the City Level Shown in Parentheses

	All		Manufacturing		Wholesale		Retail		FIRE		Service	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Short-term (1994–1996)												
EZ	2.12***	2.41***	−0.087	−.204*	34.0***	36.8***	12.3***	14.1***	−0.277	−0.466	2.68***	3.14***
	(0.568)	(0.861)	(0.0959)	(0.119)	(8.75)	(12.8)	(3.51)	(4.99)	(0.712)	(0.932)	(0.811)	(1.17)
% Retail (1994)		−5.5		1.34**		−60.6		−30.8		2.68		−8.16
		(5.41)		(0.619)		(79.0)		(26.4)		(4.06)		(6.11)
% Manufacturing		−2.73		0.131		−35.6		−13.7		0.75		−3.56
(1994)		(3.12)		(0.316)		(46.7)		(16.5)		(2.13)		(3.46)
Establishments		0.0003		<0.0001		0.0054		0.0020		−0.0002		<0.0001
(1994)		(0.0006)		(<0.0001)		(0.0078)		(0.0029)		(0.0004)		(0.0006)
Constant	−0.499**	0.148	−0.272***	−0.366***	−5.7	1.93	−1.39	1.98	−0.18	−0.417	−0.613*	0.309
	(0.249)	(0.454)	(0.100)	(0.103)	(3.88)	(6.27)	(1.36)	(2.00)	(0.238)	(0.327)	(0.311)	(0.510)
Observations	106,480	106,480	26,620	26,620	2,662	2,662	10,648	10,648	9,317	9,317	18,634	18,634
Long-term (1994–2000)												
EZ	2.21***	2.19**	−0.0844	−0.143	−4.27	−7.39*	16.0***	17.7***	−0.404	−0.557	1.94***	2.13***
	(0.609)	(0.905)	(0.0737)	(0.0866)	(2.82)	(3.91)	(4.32)	(6.43)	(0.573)	(0.770)	(0.523)	(0.765)
% Retail (1994)		−2.38		0.960**		43.8**		−36.6		2.59		−4.48
		(6.19)		(0.377)		(20.6)		(39.5)		(3.02)		(4.37)
% Manufacturing		−2.98		0.393**		13.5		−22		1.09		−2.64
(1994)		(3.06)		(0.196)		(10.5)		(21.5)		(1.39)		(2.55)
Establishments		0.0006		>−0.0001**		−0.0041*		0.004		−0.0002		0.0001
(1994)		(0.0006)		(<0.0001)		(0.0023)		(0.004)		(0.0003)		(0.0004)
Constant	−0.464*	0.0099	−0.125*	−0.224***	1.88**	−2.05	−2.56	2.05	−0.349**	−0.626**	−0.582***	0.0052
	(0.258)	(0.534)	(0.0688)	(0.0762)	(0.918)	(1.71)	(1.86)	(2.94)	(0.173)	(0.24)	(0.214)	(0.374)
Observations	106,480	106,480	26,620	26,620	2,662	2,662	10,648	10,648	9,317	9,317	18,634	18,634

Notes: (a) The pretreatment year is always 1994, the treatment year for the short-term results is 1996, and the treatment year for the long-term results is 2000.

(b) Unit of observation is the two-digit SIC industry at the census tract level of geography.

(c) Data on number of establishments is from the Dun & Bradstreet survey and is differenced as shown in equation (1) to reflect our identification strategy.

(d) The EZ variable represents designation of the federal EZ wage tax credit at the census tract level, the coefficient on this variable reflects the effect of this tax incentive on the number of new establishments per 1,000 existing establishments that locate in an area relative to the surrounding city compared to areas that applied for the EZ designation but were denied relative to their respective surrounding city.

***indicates statistically significant at 1% level, **at 5% level, *at 10% level.

panel of Table 7. These results suggest that the EZ program was responsible for about 2.2 new establishments per 1,000 existing establishments in the long term, again statistically significant at conventional levels. This result translates into approximately 20 total new establishments in EZ areas, and approximately 130 jobs at new establishments.

As with the OLS results, the IV results show a differential effect on new establishment location across industry sectors. The most consistent IV findings in terms of sign, magnitude, and significance are in the retail and service sectors, shown in columns 7, 8, 11, and 12 of Table 7. In the short term, the EZ program is responsible for adding between 12.3 and 14.1 new retail establishments per 1,000 existing establishments, statistically significant at the one percent level. The positive effect on retail establishments is stronger in the long term as the EZ program attracts between 16 and 17.7 new retail establishments per 1,000 existing establishments in the designated area, statistically significant at the one percent level.

In the service industry, the short-term effect of the EZ program is also large, between 2.6 and 3.1 new establishments per 1,000 existing establishments, and statistically significant. The positive effect in the service industry is somewhat diminished in the long term as the estimates shrink to between 1.9 and 2.1 new establishments per 1,000 existing establishments; however, the estimates are still statistically significant. These estimates translate into about 40 new firms in the retail industry and 5 in the service industry in EZ areas in the long term. Given the average number of employees at these types of new firms, the new establishments create about 138 retail jobs and 30 service industry jobs in EZ areas.

The estimates provided here are gross, not net effects of the number of new establishments and employees at those establishments, and they do not include any employment effects at existing establishments. Given the geographic targeting of the EZ policy, it is possible there are substantial displacement effects of the program on other areas. This may be especially true in industries that can relocate easily such as the retail and service sectors. If displacement from other areas does occur as a result of the EZ tax incentives, it would be appropriate to measure the benefit of the program in terms of net new establishment creation.

A potential criticism of the IV approach is that political representation is not uncorrelated with the error term in our original regression, or that the instruments are not exogenous. This concern is valid, for example if representatives on the Ways and Means Committee had a larger *change* in earmarked spending (other than the EZ) for their districts than other representatives in our sample did over the period. Hanson (2009) shows that although the *level* of spending was higher for Ways and Means members, the *change* in spending that occurs over our sample period was not substantially larger for these member's districts. In addition, there is substantial turnover of Representatives on the committee before and after the 103rd congress (when zones were chosen). Of the 39 members, the Ways and Means committee featured 12 new members for the 104th Congress, which began by the time the EZ tax incentives started. By the year 2000, the 106th Congress (the end of the data for this paper) only 21 of the 39 Ways and Means members from the 103rd Congress remained.

Industry Agglomeration

Another possible criticism of the results presented in Table 3 (and Table 7) is that the tax incentives could have a heterogeneous effect across areas with different amounts of existing industry presence or agglomeration. If this is true, then the large standard errors in Table 3 could be the result of large positive effects of the tax incentives in areas with agglomeration being offset by large negative (or zero) effects in areas without

agglomeration. An existing industry presence in the targeted area can increase productivity of entering establishments through several mechanisms. The most common channels are that near-by establishments share a pool of skilled workers, gain insight into the production process through knowledge sharing, and can cut costs by sharing inputs to production.

A large literature establishes a link between industry agglomeration and productivity, for recent examples see Greenstone et al. (2010), Rosenthal and Strange (2004), and Ellison and Glaeser (1999). It would be natural for new establishments to react to a policy differently in areas with some degree of existing agglomeration. Indeed, previous work by Devereux et al. (2007) finds that government grants are less effective at inducing establishments to locate in an area that has fewer establishments in the same industry.

To capture the differential effect that agglomeration economies may induce on new establishment location decisions, we use a simple model that allows us to test the significance of the interaction between existing industry presence and the tax incentives. Our model for testing this effect is

$$Y_{i,n} = \alpha + \beta_1 EZ_{i,n} + \beta_2 AG_{i,n} * EZ_{i,n} + \beta_3 AG_{i,n} + X'_{i,n}\delta + u. \tag{3}$$

The parameter of interest in this regression is β_2, which tells us if the effect of the EZ tax incentives was different in areas with a existing agglomeration. We measure agglomeration as the number of employees at existing establishments in a given industry prior to the start of the policy (1994), often referred to as a localization effect.[33] Table 8 displays the regression results for estimating equation (3), which reveals that the tax incentives increased the number of new establishments more in areas that had existing agglomeration in the same industry. The parameter of interest, β_2, shows that in the short-term areas with existing agglomeration the EZ tax incentives were responsible for a small increase in the number of new establishments (between 0.0149 and 0.0151 new establishments per 1,000 existing establishments). The effect in areas with existing agglomeration is small, but statistically significant at the 10 percent level.

The β_2 coefficient is not statistically different from zero for any of the long-term regressions or the results across industry types. The sign of β_2 in both the retail and service industries is positive, suggesting that the large positive effects measured with the IV specification are even larger in areas with existing agglomeration. It is difficult to draw a definitive conclusion from the results across industries because of the large standard errors.

Differential Effects Across EZ Recipients

Although the tax incentives and grants offered by the federal government were identical across EZ's, there is a local component to planning and the use of funds that we cannot quantify. As explained in Oakley and Tsao (2006), different EZ recipients highlighted different goals in their strategic plan.[34] The different goals of policy-makers across EZ areas opens up the possibility that, despite receiving the same federal tax incentives

[33] We use the number of employees (as is common in the agglomeration literature) instead of the number of establishments to avoid bias in our coefficients because the number of establishments is in the denominator of our dependent variable.

[34] For example, Baltimore Zone plan consisted of eight different components: community mobilization; community development; public safety; housing; health and family development; education, training, and literacy; youth support programs; and economic development. The strategic plan for the Chicago Zone consisted of two components: alleviating poverty through strategies enabling residents to achieve self-sufficiency and build sustainable communities; and changing the fundamental way the federal, state,

TABLE 8: Effect of Tax Incentives on New Establishment Location with Agglomeration, Standard Errors Clustered at the City Level in Parentheses

	All		Manufacturing		Wholesale		Retail		FIRE		Service	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Short-term (1994–1996)												
EZ	−0.0461	−0.1190	−0.0977***	−0.132***	6.8100	4.5900	0.2610	−0.0835	−0.0856	−0.1480	0.3100	0.2900
	(0.151)	(0.12)	(0.0325)	(0.043)	(4.95)	(3.69)	(0.438)	(0.575)	(0.225)	(0.231)	(0.324)	(0.29)
EZ*AG	0.0149*	0.0151*	0.0042	0.0042	−0.0377	−0.0259	0.1210	0.1220	0.0004	0.0006	0.0045	0.0045
	(0.0089)	(0.0090)	(0.0039)	(0.0038)	(0.0374)	(0.0327)	(0.0976)	(0.0979)	(0.0031)	(0.0031)	(0.0033)	(0.0033)
AG	−0.0033	−0.0036	−0.0006	−0.0008	0.0195	0.0143	−0.0114	−0.0124	−0.0014	−0.0017	−0.0020	−0.0021
	(0.0032)	(0.0033)	(0.0014)	(0.0013)	−0.0165	(0.016)	(0.0096)	(0.0096)	(0.0021)	(0.0022)	(0.0027)	(0.0027)
Constant	−0.0413	−0.2050	−0.272***	−0.342***	−0.3750	−4.19*	0.7790	−0.0779	−0.2070	−0.3650	−0.1250	−0.1460
	(0.0445)	(0.144)	(0.0946)	(0.099)	(0.791)	(2.25)	(0.556)	(0.713)	(0.165)	(0.269)	(0.0867)	(0.212)
Observations	106,480	106,480	26,620	26,620	2,662	2,662	10,648	10,648	9,317	9,317	18,634	18,634
R^2	0.025	0.026	0.006	0.008	0.018	0.035	0.052	0.057	0.020	0.021	0.018	0.019
Long-term (1994–2000)												
EZ	0.0406	−0.1360	−0.0478	−0.0699*	1.0000	0.0868	−0.1270	−0.7290	−0.0524	−0.1020	0.2560	0.2050
	(0.166)	(0.0968)	(0.0291)	(0.0314)	(1.44)	(1.22)	(0.628)	(1.13)	(0.158)	(0.157)	(0.202)	(0.148)
EZ*AG	0.0087	0.0092	0.0017	0.0017	−0.0207	−0.0156	0.1510	0.1520	−0.0022	−0.0020	0.0024	0.0025
	(0.0057)	(0.0058)	(0.0016)	(0.0016)	(0.0192)	(0.0178)	(0.148)	(0.147)	(0.0029)	(0.0029)	(0.0015)	(0.0014)
AG	−0.0027	−0.0033	−0.0003	−0.0005	0.0128	0.0105	−0.0065	−0.0080	−0.0001	−0.0005	0.0006	0.0004
	(0.0023)	(0.0025)	(0.0013)	(0.0012)	(0.0151)	(0.0145)	(0.0057)	(0.0066)	(0.0020)	(0.0019)	(0.0015)	(0.0015)
Constant	−0.0048	−0.3420	−0.134*	−0.205**	0.5690	−1.0700	0.1090	−0.9200	−0.415***	−0.575***	−0.237***	−0.306*
	(0.0533)	(0.248)	(0.073)	(0.0783)	(0.768)	(1.35)	(0.351)	(1.13)	(0.115)	(0.195)	(0.0636)	−0.1600
Observations	106,480	106,480	26,620	26,620	2,662	2,662	10,648	10,648	9,317	9,317	18,634	18,634
R^2	0.018	0.021	0.004	0.005	0.006	0.018	0.030	0.043	0.017	0.017	0.008	0.009

Notes: (a) The pretreatment year is always 1994, the treatment year for the short-term results is 1996, and the treatment year for the long-term results is 2000.

(b) Unit of observation is the two-digit SIC industry at the census tract level of geography.

(c) Data on number of establishments is from the Dunn & Bradstreet survey and is differenced as shown in equation (1) to reflect our identification strategy.

(d) The EZ*AG variable represents the interaction of our agglomeration measure and an area designated with the federal EZ wage tax credit at the census tract level. The coefficient on this variable reflects how existing agglomeration in an area changes the effect of this tax incentive on the number of new establishments per 1,000 existing establishments that locate in an area relative to the surrounding city compared to areas that applied for the EZ designation but were denied relative to their respective surrounding city.

(e) Regressions in even numbered columns control for the share of existing establishments that are retail in 1994, and that are manufacturing in 1994, regressions in odd numbered columns are estimated without these controls.

***indicates statistically significant at 1% level, **at 5% level, *at 10% level.

TABLE 9: Effect of EZ Tax Incentives on New Establishment Location Across Zone Areas, Standard Errors Clustered at City Level

	Short-term				Long-term			
	OLS	OLS (controls)	IV	IV (controls)	OLS	OLS (controls)	IV	IV (controls)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Atlanta	−0.00297	0.00206	−0.101	0.0191	−0.109***	−0.132***	0.29	0.0852
	(0.0616)	(0.0691)	(0.511)	(0.536)	(0.0366)	(0.0392)	(0.741)	(0.612)
Baltimore	0.0435	0.033	0.525	0.954	0.0831**	0.0158	2.07	0.858
	(0.0616)	(0.0647)	(3.24)	(4.95)	(0.0366)	(0.0411)	(4.46)	(4.67)
Chicago	−0.214***	−0.251***	0.00426	−0.269	−0.178***	−0.211***	−0.74	−0.579
	(0.0615)	(0.0557)	(0.854)	(0.809)	(0.0366)	(0.042)	(1.14)	(0.935)
Detroit	−0.0041	−0.0319	0.414	0.058	−0.0579	−0.0942**	−1.13	−0.682
	(0.0615)	(0.058)	(1.68)	(1.45)	(0.0366)	(0.0376)	(2.2)	(1.65)
Philadelphia	0.453	0.407	1.54	0.86	0.453	0.401	−2.34	−0.631
	(0.481)	(0.464)	(4.43)	(3.94)	(0.411)	(0.338)	(5.41)	(4.15)
New York	0.895***	0.765***	1.52***	1.47***	0.882***	0.54**	1.59***	1.25***
	(0.0615)	(0.142)	(0.165)	(0.184)	(0.0366)	(0.204)	(0.133)	(0.204)

Notes: (a) The pretreatment year is always 1994, the treatment year for the short-term results is 1996, and the treatment year for the long-term results is 2000.

(b) Unit of observation is the two-digit SIC industry at the census tract level of geography.

(c) Data on number of establishments is from the Dunn & Bradstreet survey and is differenced as shown in equation (1) to reflect our identification strategy.

(d) The coefficients reflect the effect of the EZ tax incentives on the number of new establishments per 1,000 existing establishments that locate in an area relative to the surrounding city compared to areas that applied for the EZ designation but were denied relative to their respective surrounding city. All regressions omit EZ areas besides the zone specified.

***indicates statistically significant at 1% level, **at 5% level, *at 10% level.

and grants, the policy has a differential effect across areas. To test for this possibility, we re-estimate equation (2) separately for each EZ. Table 9 shows the EZ coefficient estimates by separate area using a variety of different specifications. The results show that there are differential effects of the program across zones.

The New York EZ shows a sizeable positive and statistically significant increase in the number of new establishments entering the zone area. The magnitude of the New York coefficient in the short-term suggests the EZ increased the number of new establishments by between 0.765 and 1.52 per 1,000 existing establishments. The long-term results for New York suggest the EZ increased the number of new establishments by between 0.54 and 1.59 per 1,000 existing establishments. The New York results are robust to using control variables and remain strong and positive across estimation techniques (OLS or IV). We also find some evidence (not using control variables) of a small positive (statistically significant) effect in the long term for the Baltimore zone.

The other statistically significant results estimating the effect of the EZ separately across zones show a negative effect on the number of new establishments. Both the short-term and long-term OLS results show the program had a negative effect in the Chicago zone, reducing the number of new establishments entering the Chicago EZ. We also find small, negative (statistically significant) effects in the long term using the OLS specifications for Atlanta.

county, and city governments interact with ordinary citizens, especially those with low incomes (Oakley and Tsao, 2006).

7. CONCLUSION

A wide range of state and local governments use tax incentives as an economic (re)development tool. Part of the hope of policy makers is to attract new establishments to the local economy. Our IV results suggest a positive and statistically significant effect of the EZ tax incentive program on attracting new establishments, a result that is particularly strong in the retail and service industries. OLS results suggest a positive effect in the retail industry, although not as large as the IV specification. Our estimates of the effect of the EZ program and the cost involved in attracting a new establishment ignore any displacement and existing establishment effects that may occur as a result of the EZ. If the new establishments entering EZs relocated from other areas of the city, a true measure of success at the city-level should net out any displacement effects. Quantifying displacement and existing establishment effects of the EZ program is an area ripe for future research.

REFERENCES

Arauzo-Carod, Josep-Maria, Daniel Liviano-Solis and Miguel Manjon-Antolin. 2010. "Empirical Studies in Industrial Location: An Assessment of their Methods and Results," *Journal of Regional Science*, 50(3), 685–711.

Bartik, Timothy J. 1985. "Business Location Decisions in the United States: Estimates of the Effects of Unionization, Taxes, and Other Characteristics of States," *Journal of Business and Economic Statistics* 3, 14–22.

Bartik, Timothy J. 1989. "Small Business Start-Ups in the United States: Estimates of the Effect of Characteristics of States," *Southern Economic Journal*, 55, 1004–1018.

Black, Dan A. and William Hoyt. 1989. "Bidding for establishments," *American Economic Review*, 79, 1249–1256.

Boarnet, Marlon G. and William T. Bogart. 1996. "Enterprise Zones and Employment: Evidence from New Jersey," *Journal of Urban Economics*, 40, 198–215.

Bondonio, Daniele. 2003. "Do Tax Incentives Effect Local Economic Growth? What Mean Impacts Miss in the Analysis of Enterprise Zone Policies," U.S Census Bureau Center for Economic Studies Discussion Paper CES 03–17.

Bondonio, Daniele and John Engberg. 2000. ""States" Enterprise Zone Policies and Local Employment: What Lessons Can Be Learned?" *Regional Science and Urban Economics*, 30, 519–549.

Bondonio, Daniele and Robert T. Greenbaum. 2007. "Do Local Tax Incentives Affect Economic Growth? What Mean Impacts Miss in the Analysis of Enterprise Zone Policies," *Regional Science and Urban Economics*, 37, 121–136.

Devereux, Michael, Rachel Griffith, and Helen Simpson. 2007. "Establishment location decisions, regional grants, and agglomeration externalities," *Journal of Public Economics*, 91, 413–435.

Ellison, Glenn and Edward Glaeser. 1999. "The Geographic Concentration of Industry: Does Natural Advantage Explain Agglomeration?" *American Economic Review*, 89, 311–316.

Gabe, Todd M. and Kathleen P. Bell. 2004. "Tradeoffs Between Local Taxes and Government Spending as Determinants of Business Location," *Journal of Regional Science*, 44(1), 21–41.

Glaeser, Edward. 2001. "The Economics of Location Based Tax Incentives," Harvard Institute of Economic Research Discussion Paper 1932.

Greenbaum, Robert T. and John Engberg. 2004. "The Impact of State Enterprise Zones on Urban Manufacturing Establishments," *Journal of Policy Analysis and Management*, 23, 315–339.

Greenstone, Michael, Richard Hornbeck and Enrico Moretti. 2010. "Identifying Agglomeration Spillovers: Evidence from Winners and Losers of Large Plant Openings," *Journal of Political Economy*, 118, 536–598.

Hanson, Andrew. 2009. "Local Employment, Poverty, and Property Value Effects of Geographically Targeted Tax Incentives: An Instrumental Variables Approach," *Regional Science and Urban Economics*, 39, 721–731.

Hanson, Andrew and Shawn Rohlin. 2010. "The Effect of Location Based Tax Incentives on Establishment Location and Employment Across Industry Sectors," *Public Finance Review*, forthcoming.

Holmes, Thomas. 1998. "The effect of state policies on the location of manufacturing: Evidence from state borders," *Journal of Political Economy*, 106, 667–705.

Kolko, Jed and David Neumark. 2008. "Changes in the Location of Employment and Ownership: Evidence from California," *Journal of Regional Science*, 48(4), 717–743.

Krupka, Douglas and Douglas Noonan. 2009. "Empowerment Zones, Neighborhood Change and Owner Occupied Housing," *Regional Science and Urban Economics*, 39, 386–396.

Oakley, Deirdre and Hui-Shien Tsao. 2006. "A new Way of Revitalizing Distressed Urban Communities? Assessing the Impact of the Federal Empowerment Zone Program," *Journal of Urban Affairs*, 25, 443–471.

O'Keefe, Susan. 2004. "Job Creation in California's Enterprise Zones: A Comparison Using a Propensity Score Matching Model," *Journal of Urban Economics*, 55, 131–150.

Papke, Leslie. 1994. "Tax Policy and Urban Development: Evidence from the Indiana Enterprise Zone Program," *Journal of Public Economics*, 54, 37–49.

Rosenthal, Stuart and William Strange. 2003. "Geography, Industrial Organization, and Agglomeration," *Review of Economics and Statistics*, 85, 377–393.

———. 2004. "Evidence on the Nature and Sources of Agglomeration," in J. Vernon Henderson and Jacques-François Thisse (eds.), *Handbook of Urban and Regional Economics*, Vol. 4. Amsterdam: North-Holland.

U.S Department of Housing and Urban Development. 2001. "Interim Assessment of the Empowerment Zones and Enterprise Communities Program: A Progress Report," Washington, DC.

U.S. Government Accountability Office. 2004. "Federal Revitalization Programs Are Being Implemented, but Data on the Use of Tax Benefits Are Limited: Report to Congressional Committees," GAO–04–306, Washington, DC.

Wallace, Marc. 2004. "Congressional Considerations and Urban Characteristics in the Selection of Empowerment Zones and Enterprise Communities," *Journal of Urban Affairs*, 26, 593–609.